UNIV. OF
TORONTO
LIBRARY

EDINBURGH

MEDICAL JOURNAL

Vol. 35 v.1

July - December
1889

430071
28.11.44

Edinburgh
Oliver and Boyd, Tweedale Court

UNIVERSITY OF TORONTO LIBRARY

R
31
E22
v. 35
no. 1-6

Vol. 35 Pt. I
(1889, Jly-Dec) 1425

THE

EDINBURGH MEDICAL JOURNAL.

Part First.

430071
28.11.44

ORIGINAL COMMUNICATIONS.

I.—THE PATHOLOGICAL IMPORTANCE OF THE BROAD LIGAMENTS.

By LAWSON TAIT, F.R.C.S., LL.D., etc., Professor of Gynæcology in Queen's College, Birmingham.

THE usual anatomical statement that there are two broad ligaments instead of one has certain advantages in pathology which are sufficient to justify us in retaining it as a method of description. It is, however, not quite accurate, and occasionally it introduces a slight confusion. The real fact is that the two broad ligaments consist of but one fold of peritoneum, in which are invested the uterus and its appendages, together with certain survivals of fœtal structure which happen to be symmetrical, or nearly so. But the experiments of disease often display the fact that this fold is not truly separable into two parts, though the symmetry of its bilateral contents often lead us to forget or overlook the fact.

At first sight it would almost seem absurd to devote much attention to the broad ligaments, a mere flexure of serous surface, by no means well defined in extent, but very constant in character and relations, and differing in function in no way, so far as is known, from any other part of the peritoneal layers.

In the male the broad ligament is a mere representative structure, recognisable only by the transcendentalist, and it has, so far as I know, not the slightest pathological importance peculiar to itself. But I doubt if there is any other organ of the body of so much importance in women; there is certainly no structure which bears so strongly on the differentiation of pathological changes taking place in its neighbourhood, and I am equally clear that it has a surgical importance in the special diseases of women which cannot be exaggerated.

The broad ligament (taking its two halves for the moment as one) is formed by the peritoneum which has descended as one fold from the base of its mesenteric union into the pelvis in front of the rectum, sacrum, and great vessels, forming at the lowest point of its descent the "pouch of Douglas." The floor of this cul-de-sac corresponds with a small portion of the posterior flexure of the vagina—a fact often forgotten by those unaccustomed to pelvic surgery, and forgotten with disastrous results.

In the middle line the peritoneum rises upwards and forwards on to the posterior wall of the uterus, and passes over that organ, dipping down over the front wall for quite two-thirds of its extent, thence passes over the base of the bladder on to the anterior abdominal wall, making in this course the anterior or vesico-uterine peritoneal cul-de-sac. All these relations are so variable in extent that no common description can be given of them; for sometimes the anterior cul-de-sac will be found almost as capacious as that behind the uterus, whilst in other cases it is hardly represented at all. But its importance must never be forgotten, especially in cleansing out the peritoneum when it has been the seat of purulent inflammation, or an accumulation of débris from any other cause.

On each side of the uterus the peritoneum rises over the Fallopian tube and passes a short distance in front of it, and again in front of the round ligament of the uterus, here sometimes forming supplemental folds of great importance; but these vary in extent very greatly in different women. Further onwards, on each side and at the outer wall of the bladder, the peritoneum rises abruptly to the anterior abdominal wall, whilst the posterior layer goes obliquely upwards to the brim of the pelvis. In this way the free peritoneal surface forms two cups of different sizes, depths, and shapes, ovoid in circumference, and irregularly based according to many varying circumstances. Thus, when the bladder is distended the anterior basin is filled up, and this it may be also by a tumour of the bladder or the uterus. The retro-uterine cavity may not exist by reason of defective development of the peritoneum, or it may be destroyed entirely and permanently by the adhesions of a retroverted uterus. It may also be filled up by the occurrence of a broad ligament hæmatocele, by the occurrence of a uterine tumour, or of a tumour originating in the cavity of the broad ligament, or an abscess in the same part. Finally, these two cavities may be imitated in the allantoic cyst (remnant of the original cavity—retained after the separation of the splanchnopleure) without having a particle of peritoneal surface near them.

Withal, it must be borne in mind, these cavities, both ante-uterine and retro-uterine, exist more in the eye of the dissector than in nature, for under normal circumstances they are practically closed and contain little. But in diseased conditions they contain much, especially that behind the uterus; and these contents may be of the most vital importance, and be most difficult of discrimination.

At the central point of each wing of the broad ligament its folds are practically in contact, though lying between these are many structures of interest and importance. From the uterus and towards this central point these layers (theoretically) approach one another, and between these and near the uterus lies some small amount of cellular tissue to which a most undue and altogether undeserved importance has been attached. From the central point outwards the layers tend to separate again, and when they separate they actually do contain between them an appreciable amount of cellular tissue to which sufficient importance has not yet been given. The anterior layer runs off to the pelvic wall on the curve of the anterior basin (the vesico-uterine pouch), whilst the posterior layer runs off on the curve of the posterior basin (Douglas's cul-de-sac).

We shall find that the dissections made by diseased growths can remove this complicated set of plications and put them into a single stretch of membrane, better even than the dissector's knife could do it. Then we have the two surfaces of the membrane alone to speak of—the serous and the cellular tissue side, the intra- and the extra-peritoneal surfaces of the membrane.

I need not remind my readers that, save for the opening of the infundibulum of the Fallopian tube, the peritoneum is a closed cavity—not only closed in the sense of having no opening into it, but closed in a sense that can be understood and thoroughly realized only when a frozen section has been made before the body has been opened in any way, and carefully examined. Then it is seen how erroneous dissecting-room notions of the relations of organs really are, conveying, as they do, ideas of disassociation rather than those of normal relations. No matter what the training of the student be—even if both methods be combined—it is by no means easy to get a practical notion instilled into the mind of the enormous practical importance of the broad ligaments, and as to what really is meant in speaking of them by the phrases "extra-peritoneal" and "intra-peritoneal."

To the beginner the phrases seem to be self-contradictory, and even to those who believe they know a great deal about this subject it is perfectly clear that those phrases are elements of the utmost confusion. How anything, such as an ectopic pregnancy, can be in the cavity of the broad ligament and be extra-peritoneal is a puzzle to those who do not know the anatomy of the pelvis thoroughly; but even to many of those who do know it, the rupture of the broad ligament and the escape of the ovum into the cavity of the peritoneum seems to be a matter past comprehension.

There is of course no real *cavity* of the broad ligament: the phrase is one used only to express the fact that there is a space, practically of microscopic size, containing organs which are of much embryological interest, and often of great pathological importance. First of all there is the Fallopian tube, which deserves a special lecture for itself, and therefore, as with its associate the ovary, I need say no more about it here. Attached by a long pedicle to one of the fimbriæ of the Fallopian tube is a small cyst, stupidly called the "hydatid" of Morgagni, which is the only structure in the pelvis which never, as far as I have been able to observe, obtains any pathological importance. Between the folds of the broad ligament lie the tubules of the parovarium. The main tube, known as Gartner's duct, lies somewhat horizontally, and seems generally to arise in an ampullary dilatation (the organ of Rosenmüller), which I have seen developed into a cyst of large size. A varying number of ventral tubules run from below upwards into the horizontal tube, like the branch field-pipes into a main agricultural drain. They are often broken in their course by closures of the tubes, and in this way they seem ready to develop into pathological cysts. That a very large proportion of what are called parovarian cysts, or cysts of the broad ligament, arise in this way is entirely beyond doubt; but this simple explanation by no means applies to all. The true parovarian cyst has certain characters by which it can, I believe, always be recognised, and its surgical as well as its pathological relations are quite peculiar and far more serious than are usually supposed. The epithelial lining of such cysts, even in their early stages, shows an activity which is most remarkable. The cells are irregular in size and often in form, often ciliated, but incompletely so, and they evince an immaturity of growth which I hold to be the only characteristic of malignant disease. When the tumours have reached large size this epithelial proliferation becomes dendritic and papillomatous, and some of these simple-looking tumours are amongst the most malignant things I have met with. I have removed a perfectly simple-looking parovarian cyst from a young girl—a cyst in which not even papillomatous nor dendritic growths were visible—and that poor child had cancer in the pelvic glands perceptible in six weeks, and died before three months had elapsed from cancerous deposits in all the important organs; and this, though a special instance of the malignancy of these growths, by no means stands alone in my experience. Such facts afford a strong argument against the practice of tapping such tumours.

The horizontal (or Gartner's) tubes may occasionally be traced down through the substance of the uterus and in the walls of the vagina close to the opening of the urethra; but as a rule it is like the vertical tubules broken up in pieces, each piece being closed at both ends, and without doubt these disjointed sections form the basis of large cystic formations, just as is the case with the vertical tubules. They are all fœtal relics, and this fact may explain the malignant tendency towards rapid immature growths to which I have already alluded.

That there are cysts developed in the broad ligament which do not arise from these fœtal survivals is almost certain, but from what source they arise I do not know. There is one kind, not uncommon, which develops a large quantity of unstriped muscular fibre in its walls, so that occasionally it looks almost like a recently pregnant uterus greatly distended. The largest tumour I ever removed in my life (estimated to weigh at least 110 lbs.) was a cyst in the broad ligament, the walls of which were more than an inch thick when it had contracted after removal. These walls were composed as exclusively of unstriped muscular fibre as is the pregnant uterus. The patient had lain on her side for many months before the operation; had therefore large bedsores, and was altogether in a deplorable state. She (case 4 in the list) survived the operation only a few hours.

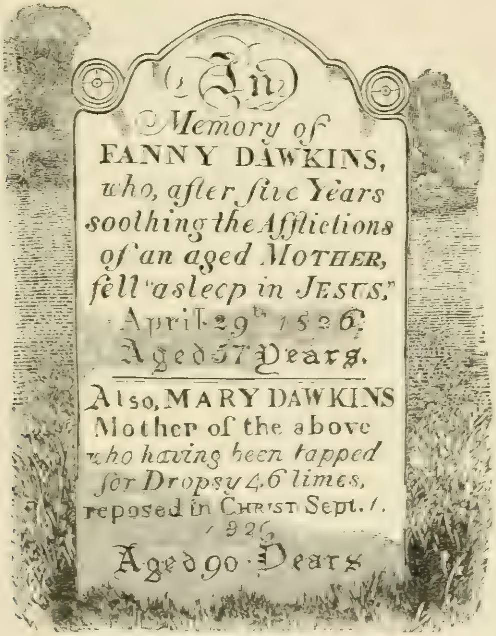
I have removed three large solid myomata from the broad ligament by enucleation. In microscopic structure they could not in any way be differentiated from soft œdematous uterine myoma. Cystic tumours of the broad ligament are easily recognised by the fact that they are almost uniformly unilocular—in fact, they are uniformly so, for any apparent exception can easily be shown to be due to the growth of two or more cysts perfectly separable; in this particular differing widely from cystic tumours of the ovary, which are hardly ever perfectly unilocular. I have known only one unilocular tumour of the ovary, and it was of very small size. Broad ligament cysts are, therefore, uniform in their outlines. The walls are usually very thin—sometimes extremely thin, resembling wet tissue paper more than anything else. Sometimes they grow very rapidly, at other times very slowly. When removed they can always be recognised by an anatomical peculiarity: their peritoneal investment is *perfectly* distinct from the cyst wall, and can be stripped off from the cyst with the greatest ease. In their growths they separate the folds of the broad ligament, pushing them up before them; and this process very often results in an embedding of the cyst in the pelvis, all the pelvic peritoneum being lifted out of the cavity by the growth of the tumour. I have given to these cases the name of "embedded cysts," and in my early practice they were a source of great trouble to me. Alarmed at the condition, I only partly removed the cyst as far as I could, then I stitched the edges of its cut walls to the edges of the abdominal wound, and put in a drainage tube, hoping in this way to cure the tumour. But I never did, and in several of the cases recorded in the list it is the second operation which is given at which I opened the abdomen and completed the case, as I ought to have done at first. As a matter of fact, the enucleation of these cysts can always be accomplished under two conditions. The operator must be skilful with his fingers, and determined never to leave any operation unfinished; and when he selects his point for opening the peritoneum he must work steadily onwards from that point, and not let his fingers dance about from one point to another. If he does he will lose his bearings and make a mess of the case.

There are some operators who still advocate tapping this kind of tumour, in the hope that a cure will be effected in this way. I doubt very much if such a thing ever happened. Certain it is that when tapped they may remain dormant for some years. Many of the cases in my list are illustrations of this. In one which I had tapped myself the patient was troubled no more for eight years, but then the cyst refilled after that long interval. I removed it, and found the scar of my old tapping-wound.

The contents of these cysts are generally limpid, but I have seen thick, gluey, and gelatinous contents of all shades of colour and consistency. I have seen many in which the cyst contents were hæmorrhagic, and the smooth ovoid broad-ligament cyst seems to have a special tendency to rotate and become strangulated.

At the meeting of the Medical Society of Strasburg, November 15, 1875, M. Kœberlé read a paper on the diagnosis between ovarian cysts of the broad ligament and cysts of the Fallopian tube, based on the chemical examination of the fluid contained in them. He found that the fluid of ovarian cysts contains some albumen, but a much larger proportion of the variety of albumen called paralbumen, the precipitate of which by nitric acid is soluble in acetic acid. The fluid found in cysts of the Fallopian tube, on the contrary, he says, contains albumen, but no paralbumen, so that the precipitate formed by nitric acid is rather increased by acetic acid. The fluid of cysts of the broad ligament is generally very limpid, containing salines, but very little albumen. Sometimes, however, it contains a distinct quantity of albumen, and the precipitate formed by nitric acid may be soluble in an excess of that acid. The researches of Schutzenberger with the tanning process for estimating the quantity and kind of albumen have, however, thrown great doubt on these conclusions, and by the same means I have quite satisfied myself that M. Kœberlé's conclusions are not to be accepted. At my request, my friend Dr McMunn, of Wolverhampton, undertook to investigate the possibility of determining the source of fluids by means of the spectroscope. I furnished him with a number of specimens of fluid the sources of which were absolutely known, but the results of his researches were entirely negative. They are given in detail in his valuable work on *The Spectroscope in Medicine* (London 1880).

When the contents are limpid and of low specific gravity tapping probably does little harm to the constitution, for the system is robbed of little more than water. Such instances probably yield the instances in which tapping has been performed an enormous number of times over a long series of years. Two such cases I found recorded on tombstones in the churchyards of Romsey, in Hampshire, and Bunhill Fields; and I leave the epitaphs to tell their own story, doubtless in these instances quite veracious.



Tombstone in Romsey Churchyard.

In Bunhill Fields is the monument of a courageous lady, whose memory is thus perpetuated. On one side of the tomb we read—

> HERE LYES DAME MARY PAGE,
> RELICT OF SIR GREGORY PAGE, BART.
> SHE DEPARTED THIS LIFE MARCH 11, 1728,
> IN THE 56TH YEAR OF HER AGE.

And her fame rests on the inscription on the opposite side—

> IN 67 MONTHS SHE WAS TAP'D 66 TIMES,
> HAD TAKEN AWAY 240 GALLONS OF WATER
> WITHOUT EVER REPINING AT HER CASE
> OR EVER FEARING THE OPERATION.

The constant character of the relations of the peritoneal investment of these tumours from the true cyst is aided, for their identification, by another and almost as constant relation of the Fallopian tube and ovary. Some twenty years ago I had occasion to make a medico-legal examination of the body of a woman far advanced in life, and I found in her left broad ligament a cyst as large as an orange, filled with clear, limpid serum. It was pressing upward and backward out of the pelvis, the ovary being at its lower and anterior aspect, and the Fallopian tube arched over its anterior surface. On the side next the uterus two smaller cysts were lying close to it, and nearer still a very minute sac, which was evidently, from its mere shape, a distended parovarian tubule. The ovary was white, puckered, and shrivelled, and had not a continuous relation to any of the cysts, though it touched the largest at its hilum. The Fallopian tube was normal, and had no other relation to the tumours than slight connexion by loose areolar tissue. There was in my mind no doubt that this was a pathological indication of value, for in an ovariotomy that I had performed not long before I was struck by the fact that the ovary was perfectly healthy and separated from the tumour, as was also the tube, by a mesovarium of some extent; in fact, I did not do ovariotomy at all in the removal of the tumour, for, in passing the chain of the écraseur round its base, I did not include either the tube or the ovary, and they were both returned into the abdominal cavity. In the records of ovariotomies performed these cases have, up till now, always been stated as ovariotomies, and the ovary and tube associated with the tumour have been removed with it. Both the record and the removal of the ovary are mistaken. The operation is not an ovariotomy at all, and nine times out of ten both ovary and tube might easily be separated from the tube and left, and this practice I now always try to follow. It is very curious that all the early cases, with one exception, of so-called ovariotomies—I mean those between 1827 and 1842—are instances of removal of parovarian cysts; and it is still more curious that those who are crying out most loudly against the unnecessary removal of ovaries have been in the habit of pursuing this practice in the case of parovarian tumours without compunction. I have therefore placed my cases of broad-ligament cyst in a special list, at least so far as it is possible to do so. In the early part of my practice I did not discriminate these cases as carefully as I do now, and therefore some of the cases may have escaped separation.

The result of all my observations has been that in every truly unilocular tumour, with one exception, I have found the ovary unaffected, though on several occasions I have seen it stretched over the cyst-wall. I have three or four times observed the ovary separated from the cyst by a more or less distinct mesovarium, and on one occasion I found in that fold some unaffected parovarian tubules, in the case of a lady, a patient of Mr Hall-Wright, from whom I removed a large unilocular cyst about thirteen years ago. In another instance the healthy ovary was left at least an inch below the clamp; and in a third the ovary and tube were found glued on to the cyst, but forming no part of it. In this cyst the walls were extremely thick, and contained, as in other examples of the same kind of tumour, large quantities of involuntary muscular fibre—a fact which I do not think militates against my view that it was of parovarian origin; for nucleated muscular fibre-cells exist in the broad ligament to some considerable extent, and myomatous tumours are found occasionally within its folds.

The case to which I have previously alluded as presenting a tumour with many cysts, but which ought to be placed under the same category as the unilocular cysts, occurred in the person of a lady aged sixty-six. She was a widow, having been married forty-three years before the tumour appeared. The menses had ceased for nearly twenty years, and her youngest child was twenty-five years of age. There was every reason to believe, therefore, that the condition of the cell-growth of her ovaries would be one of very low activity. The tumour was first discovered about five years before I saw her, and had grown slowly for four years and a half, but with extreme rapidity for six months. The abdominal parietes were very thin, and the percussion-wave was communicated with extreme and uniform rapidity in every direction. I diagnosed, from my former experience, that it was a unilocular Wolffian cyst, and that the ovary would, in all probability, be found uninvolved. I was right about the ovary, for that was found, along with the tube, almost undisturbed, and not in any way involved in the tumour, the latter having apparently escaped from between them backward and upward. I had made a mistake, however, about the tumour being unilocular, for it was composed of five or six sacs. The walls of these were very peculiar in being of uniform thickness, or rather thinness, for they were like tissue-paper, and had no thickening toward the base of the tumour, as is always the case in the multicystic adenoid or multifollicular tumour of the ovary. My belief is that this tumour was a specimen of dropsy of a number of the parovarian tubules; for, if one alone may become dropsical, there can be no reason why a number should not be so coincidently. My opinion was confirmed, however, by a re-examination of the tumour for the special investigation of one point drawn attention to by Dr Bantock; that is the possibility of separating the outer or peritoneal coat of the cyst. This could readily be done toward its base for a short distance up from the ovary, discovering the fact that the gland and its duct could be stripped off the tumour without damaging its wall. The rapid growth during the later periods of its existence, however, seemed to have so stretched the walls that, beyond two or three inches from its base, the peritoneal layer could not be separated from the cyst wall proper. I quite satisfied myself that this case was really one of multilocular parovarian tumour, and I was confirmed in this view when I found that Dr Bantock refers to a case of Mr Spencer Wells's, which was recognised as one of bilocular parovarian cyst.

The peculiar relation of the Fallopian tube to these tumours, of which I have already spoken, exists in many of them to a very exaggerated degree. In one case I saw the tube enormously increased in size, nearly nine inches long and correspondingly thickened, so that a No. 12 catheter passed along it quite easily to within one inch of the point of division, when it suddenly contracted to its normal dimensions. It was bound over the tumour by the peritoneal investment of the cyst, just as the ureter is bound on the posterior abdominal wall; and it came away with the peritoneum when that membrane was lifted off the cyst, just as the ureter similarly rises with the serous membrane.

The diagnosis of parovarian cysts is generally very easy to the practised hand, for they give a uniform and very rapid wave of fluctuation in every diameter of the tumour. Their shape is usually globular, and they do not project into the pelvis, as is very often the case with the minor cysts of an ovarian tumour. They very rarely give rise to symptoms of any kind, and still more rarely to any symptoms of urgency. They sometimes grow very rapidly. I removed a very large parovarian cyst some years ago from a patient under the care of Dr Campbell, of Stourbridge, where the fact was fully ascertained that the tumour grew in less than six weeks. It may happen, however, that all the conditions of a parovarian cyst may be very closely imitated by an ovarian tumour, and they are absolutely mimicked by two rare forms of cyst to be afterwards described, one of which is a development of the occluded tube of the urachus, or a remnant of the allantoic cavity, and the other I believe to be developed from a wandering ovum.

A further point of great importance in recognising mere ascites from an ovarian or parovarian tumour, is that in the former condition there is generally an appearance in the patient's face of suffering from serious functional disturbance, whereas in the latter the patient often looks in perfect health. Sometimes we find the walls of a parovarian tumour very thin and flaccid, in this way closely resembling the appearance of ascites.

This class of tumours it is which has given rise to a great many different beliefs in connexion with the history and treatment of ovarian tumours which Dr Matthews Duncan has very properly designated delusions. Among these was the belief, originated by M. Boinet, that ovarian tumours had been cured by tapping, by injection with iodine, by what Mr Baker Brown called a formation of a false oviduct by the insertion of setons, and by a variety of other more or less barbarous and unscientific proceedings.

The walls of these cysts are nearly always very thin, consisting of little more than a thin basement membrane and a lining of columnar epithelium. This epithelium undoubtedly undergoes alterations such as I have described as occurring in ovarian cysts, for I have seen all the appearances on the lining of a parovarian cyst that I have seen in an ovarian tumour. They undergo malignant degeneration, they suppurate and become gangrenous, just as ovarian tumours do. Sometimes the basement membrane of their walls, which always contain some muscular fibre, becomes enormously thickened, and I have removed a parovarian cyst with walls more than half an inch thick, the greater part of which was composed of fusiform muscular cells.

They are, therefore, not matters to trifle with. Their early removal is always simple and safe. They should never be tampered with by tapping, but ought to be removed by abdominal section in their early stages, just as should ovarian tumours. Sometimes they burst and seem to disappear spontaneously, and this again has given rise to the statement that this result is obtained occasionally for ovarian tumours. When this fortunate accident takes place early in their history it will probably do no harm, but if it occurs during the advanced stages it is just as likely to result in cancerous implantation of the peritoneum as if the cyst had been ovarian. Some years ago I removed a large parovarian cyst which had several times been tapped, and which had ruptured into the abdominal cavity. On removing it I found the peritoneal surface studded with papilloma, of which the patient speedily died.

The development of an ovum in the broad ligament, resulting from the rupture of a tubal pregnancy into the cavity of the broad ligament, constitutes a most interesting phase of the progress of certain cases of ectopic gestation, and will be discussed in a chapter devoted to that strange phenomenon of pelvic pathology.

Closely allied to the above is the occurrence of an effusion of blood into the cavity of the broad ligament, and therefore most of what I have to say on this interesting subject will be found there also; but I must anticipate some of my remarks there in order to speak of abscess of the broad ligament.

It is here that we come to the ground on which we find the greatest features of importance for the broad ligament, alike pathological and clinical, for what is called pelvic hæmatocele has an importance of the greatest possible kind if it is intra-peritoneal—that is to say, if it is not in the cavity of the broad ligament; whilst, on the other hand, if it is in the cavity of the broad ligament, and therefore not intra-peritoneal, its significance is relatively very little.

The folds of the broad ligament have enclosed between them a number of bloodvessels and nerves to which I have as yet made no allusion, and of which it is wholly unnecessary that I should give a detailed anatomical description. Any injury to or rupture of these vessels will cause an effusion of blood, and if the broad ligament be uninjured that effusion will be limited to its cavity, and be extra-peritoneal. If the broad ligament be injured, then the effusion may, and probably will, extend indefinitely into the peritoneum and kill the patient. Some writers propose to term the former condition a "pelvic hæmatoma," and the latter a "pelvic hæmatocele." For reasons afterwards discussed I prefer to retain the old term "hæmatocele," mistaken in origin though it be. Like many other still more mistaken terms, it has been sanctioned by long use, and new terms, unless giving a more clearly defined meaning or a better representation of the facts, are only provocative of confusion. The term hæmatoma has neither of these recommendations, and therefore I do not accept it. Broad-ligament hæmatocele, according to my experience, arises from three causes, and always by the same means. The direct agency is rupture of a bloodvessel, and I think that is almost always a vein. The first and probably the most common cause is the sudden arrest of menstruation by a chill, mental emotion, etc., by which an undue pressure is put upon the thin-walled veins contained between the folds of the broad ligament, a rupture takes place, and blood is poured forth into the cavity.

Similarly, after operations when the broad ligament is tied as the pedicle of a tumour, there is very often set up a pseudo-menstruation, which has not its usual vent. In front of the ligature (that is, taking the road from the heart) the blocked vessels are engorged. A small capillary or capillaries give way, and an effusion of blood takes place into the cavity of the broad ligament, which is closed by the ligature. If the cavity is not so closed—that is, if the ligature slips—the hæmorrhage will be not only from a very large artery, but it will be into the cavity of the peritoneum, and it will, if left unchecked, mean the death of the patient. But if the ligature is properly tied, and the cavity of the broad ligament remains closed, the hæmorrhage will be limited, and will give rise to very little trouble. Such hæmatoceles are quite common after all operations involving the broad ligament, and experienced surgeons are quite familiar with them. Of course, writers of library papers and irresponsible (and inexperienced) writers of anonymous reviews in the medical journals cannot be expected to know anything about such occurrences, and their ignorance has to be pardoned. I have strong reason to believe that such hæmatoceles occur in about eight per cent. of all operations, and very rarely give rise to serious trouble. In another chapter I give an account of one case where I had to re-open the abdomen to save the life of the patient, the hæmorrhage, entirely into the broad ligament, being so extensive. In some three or four others subsequent suppuration has necessitated drainage of the cavity, but all the others have recovered with a transient rise of the pulse and temperature, usually lasting four or five days. These effusions generally occur on the fourth or fifth day after the operation, and are indicated by pelvic pain and discomfort *without any abdominal distension.* The pulse and temperature also rise, and if a vaginal examination is made the floor of the pelvis on one side, sometimes on both, will be found fixed. If the effusion is on the left side it will be found encircling the rectum like a ring, and this is the pathognomonic sign of the accident, as I first pointed out nearly fifteen years ago. Mr Pridgin Teale has favoured me with a remarkable observation on this point in male patients, which so completely substantiates my views that I reproduce his own words as he has been kind enough to convey them:—

"38 *Cookridge Street, Leeds,*
"*December* 24*th*, 1888.

"DEAR TAIT,—Enclosed I send you a short statement of my two instances of perirectal hæmatocele in the male. You will no doubt be able to condense them into a short statement for your book.—Yours truly,

T. PRIDGIN TEALE."

"*Notes of Two Cases of 'Perirectal Hæmatocele, simulating Cancer of Rectum.'*

"Both instances occurred as a result of attempts to relieve retention of urine in the male. In the first case I was myself deceived, as well as a neighbouring hospital surgeon and a London consultant. The second case, which occurred many years afterwards, I recognised, and then for the first time understood the first.

"Case 1, the subject of retention of urine from prostatic difficulty, had been relieved several times by catheter; at last his medical attendant failed to reach the bladder, and I was called in. I failed also, and eventually tapped the bladder above the pubes. During my examination of the rectum I was conscious of an irregular bulging of the rectal wall. About a week afterwards, on examining the rectum with the finger, in consequence of an irritable condition of bowel almost amounting to dysentery, I discovered a hard ring of constriction, which I pronounced to be scirrhous stricture of the rectum. This view was corroborated by the other hospital surgeon associated with me, and also a few days later by a leading London surgeon, who was also called in to the case. Much to our amazement, the patient recovered, and lost all trace of trouble in the bowel. We, the doctors, won no small discredit for our opinion. The second instance occurred some ten years later, and about six years ago. I was called to a case of retention of urine, in which there were extensive false passages between the bladder and rectum. I succeeded in passing a large catheter, and afterwards the medical attendant drew the urine without further difficulty. As there was intense discomfort and irritability of the bladder, at the end of three days I examined the prostate by the rectum and felt a swelling, which I punctured from the rectum, letting out offensive pus. In making the incision I was conscious that the opening in the mucous membrane was too small, for on searching for the opening I was not able to enter it with the knife. On examining the rectum the following day I discovered a similar condition to that found in No. 1. The tube of the rectum was closed by a firm ring, which I at once recognised as a surrounding hæmatocele. In the course of a few days this simulated stricture began to soften and to disappear. I have no doubt that the abscess was the result of broken-down blood-clot, and the hæmatocele the result of my puncture, which did not permit adequate escape to the submucous hæmorrhage. Both cases recovered.

"T. PRIDGIN TEALE."

This contribution is the first confirmation I have seen of my observation of the ring structure of the rectum, as a sign of broad-ligament hæmatocele. The only harm done by such occurrences is a delay in the convalescence. I have never seen any serious or permanent ill result.

(*To be continued.*)

II.—TWO CASES OF UNREDUCED DISLOCATION OF THE SHOULDER WITH UNUSUAL HISTORY.

By JOHN SHAW M'LAREN, M.A., M.B., F.R.C.S. Ed.

I PUBLISH the two following cases together, as the history of the accident is the same in both, and is a very unusual one; the treatment on the other hand varied greatly, reduction in one case being effected by the heel in the axilla, and requiring an arthrotomy in the other.

The first case occurred in Dr Heron Watson's practice about thirty years ago. A gentleman was brought to him with the following history. One night he wakened in his bed with an acute pain in his left shoulder, which he soon discovered was accompanied by complete loss of power to move the arm. His medical attendant, considering the case a peculiar form of rheumatism, treated it accordingly, but without avail. About three months after the accident he was brought to Dr Watson, who, on examination, found that the shoulder was dislocated. Chloroform was administered on the spot, and reduction was effected by the heel in the axilla. Little difficulty was encountered in reducing the dislocation, and the patient eventually recovered very free movement of the arm.

The second case resembles the previous one in the first part of its history, though the treatment differed widely, as the joint required to be opened before the bone could be replaced.

The patient, a gentleman under forty years of age, went to bed at twelve on a Saturday night in the summer of last year. About two hours later he wakened by his moaning his brother, who was sleeping in the same bed. He complained of great pain in his left shoulder, and he was unable to move his arm. The doctor who saw him treated it as a case of neuralgia, and injected morphia subcutaneously to relieve the pain. The arm became much swollen and discoloured. After a fortnight the patient returned to business, but was still unable to move the arm. Four weeks after the accident he saw Dr Watson, who discovered a sub-coracoid dislocation of the humerus. A few days later chloroform was administered, and Dr Watson attempted reduction by the heel in the axilla, and then by his crutch and spring (the apparatus devised by him for dislocation of the shoulder), but failed to replace the bone. Four days afterwards both methods were again tried, and finally the pulleys, but without success. All was done with great care not to apply excessive force. Dr Watson then decided to open the joint, and, when the patient was placed on the operating-table, he discovered slight but distinct crepitus on pressing firmly beneath the acromion.

An incision about eight inches long was made over the interval between the deltoid and the clavicular portion of the pectoralis major, so as to gain access to the cavity of the joint. The capsule was found torn completely away. The finger encountered first the glenoid cavity, and then a rough surface on the head of the humerus. This proved to be the spot from which a piece of bone, comprising the two upper facets of the greater tuberosity, had been broken off. The fragment was found attached to the under surface of the tendons which are inserted into it, and was lying at a little distance from, not in, the glenoid cavity. The biceps tendon was in position. The head of the humerus lay under the coracoid process Though it was difficult to see what was preventing its return, all manipulative methods failed to replace the bone. Dr Watson accordingly inserted a narrow lithotomy-scoop into the wound, and passing it to the inner side of and underneath the head, succeeded in levering it up into the glenoid cavity. A drainage tube was inserted and the wound was stitched and dressed. The dressing was renewed a week after the operation, when most of the wound was found to be healed. About a week later the patient's mind began to wander, and he became subject to delusions. He was sleepless and restless, and neither chloral, bromide of sodium, nor hyoscyamine had any effect in soothing him. The delusions continued and became almost incessant. The patient got very restless, and insisted on trying to leave the house. His mind was filled with notions of unusual feats he imagined he had accomplished, such as travelling from America in one night; he talked and laughed and wept in a childish manner. Each time he saw his relatives he conversed sensibly for a short time and then relapsed again. One day he attempted to leap out of the window. At the request of his friends a specialist in mental diseases was consulted, and gave as his opinion that the patient was suffering from commencing general paralysis of the insane. His attendant was directed to keep him as much in the open air as possible and to ply him with nourishing foods. He gradually got less restless and more sensible, though his manner continued markedly childish. At length this symptom passed gradually off, till, at the end of seven weeks after the mental derangement began, he was again quite well. Eight weeks after the operation passive motion of the arm was begun. The patient can now, eight months after the operation, touch with the injured arm the top of his head, the opposite shoulder, and the back of the hip on the same side. He can abduct the arm from the side about 70°, but has very little power of external rotation, though the fragment of bone has united. It is interesting to note that supination is impaired.

The way in which the dislocation was produced in both these cases was very remarkable, and the cause is to a certain extent obscure. In each case the patient went to bed perfectly well, and awoke with a dislocation of the shoulder. Such an entire absence was there of any "accident" in the ordinary sense of the word, that in both cases the dislocation was at first overlooked. Dr Watson suggests that in the first case the arm was dislocated during an epileptic fit coming on while the patient was asleep, and in the second case a similar cause may have produced the accident. I can find no record of any case in which the accident took place during sleep, though Agnew records three cases, and Gross four, in which the dislocation was produced during an epileptic fit. The first patient has had frequent epileptic attacks since the accident, which may, accordingly, be plausibly ascribed to a fit attacking the patient while asleep. The remarkable mental conditions following the operation in the case of the second patient suggest the possibility of an epileptic seizure due to some brain lesion (?), which could account for the subsequent cerebral conditions. But perhaps it is hardly necessary to assume the occurrence of an epileptic attack. Movements during a dream would suffice, for Andrews (*Ashhurt's Surgery*) mentions a case in which the shoulder was dislocated in the act of swimming, and Gross that of a patient to whom the accident occurred simply in the act of raising the arm above the head.

The reduction in the first case was easy, and the use of the shoulder-joint is free.

In the second case the dislocation was accompanied by fracture of part of the greater tuberosity of the humerus. Opinions differ as to the frequency of this accompaniment to the dislocation. Von Thaden has made a special study of this fracture, but in his paper on the subject (*Arch. für Klin. Chir.*, Bd. vi.) gives no statistics from which the frequency of the complication could be inferred. Stimson, in his *Treatise on Dislocations*, remarks that "fracture of the greater tuberosity appears to be not often capable of demonstration; at least it has often been found *post-mortem* when it had not been recognised during life." In another part of the book he goes so far as to say that it "is an habitual accompaniment of dislocation inward and downward of the shoulder." Caird (*Edin. Med. Journ.*, Feb. 1887) thinks "we are justified in recognising the true type of sub-coracoid dislocation as being always associated with an indentation fracture of the head of the humerus." This view, however, is not shared by Hamilton and many others, who consider that the dislocation frequently takes place without the accompanying avulsion of the greater tuberosity. Gurlt considered the accident a "very rare" one. Dr Stimson himself mentions a case of intra-coracoid ("sub-clavicular") dislocation in which the humerus and the supra-spinatus and infra-spinatus muscles remained intact, though of course in such a dislocation the strain is much greater.

So far on the theory that this fracture is really an *avulsion* of the greater tuberosity. I am aware that it is otherwise accounted for. Dr Joseph Bell (*Edin. Med. Journ.*, May 1863) mentions a case in which, "in addition to the primary dislocation, there is a consecutive fissure of the head of the humerus. How is this fissure produced? Not by any peculiarity in the direct violence which caused the injury; not by any conceivable action of the muscles; but by the sharp edge of the glenoid border being forced as a wedge against the posterior groove of the humerus." Mr Eve (*Med. Chir. Trans.*, 1886) advances the same theory, quoting a case in which a dissection was made of a recent dislocation, and in which a deep indentation was found at the posterior margin of the articular surface of the head of the humerus, supposed to have been caused by the impact of the edge of the glenoid. Mr Caird describes two dissections of recent cases accompanied by the fracture, and, in supporting the same theory, states that the fracture can be produced on the cadaver by making a sub-coracoid dislocation, laying the subject prone, and then striking the scapula violently. In Dr Bell's case a similar procedure was unavoidably gone through, a lad having been thrown to the ground with great violence and then accidentally trodden upon. The accuracy with which the indentation on the humerus fits the edge of the glenoid, as shown in the dissection described by Eve and Caird, and the way in which the fracture can be produced on the cadaver, support this theory especially in the case of accidents in which the violence is applied as in Dr Bell's case. I am doubtful if it accounts for all. It is admitted that muscles do sometimes produce fractures "in preference to" rupturing; and if the quadriceps extensor can break the patella, and the sartorius detach a part of the anterior superior spine of the ileum (as in a case recently brought under my notice), there is no difficulty in our attributing the detachment of part of the greater tuberosity of the humerus to the traction of the muscles inserted into it. If, as I suppose, the dislocation in the case I have described was produced by muscular action, avulsion is as likely to have occurred as splitting on the edge of the glenoid.

As regards the diagnosis of the double injury, in Dr Watson's case crepitus was almost the only sign of it, except that the finger could not be sunk into the glenoid as deeply as usual. Dr Bell describes a curious symptom not mentioned by any one else so far as I know. The outer edge of the lesser tuberosity was well defined, and there "was a puckering of the skin just outside of this edge of the lesser tuberosity. The skin was actually drawn inwards as if nipped or bitten from below, forming a very evident dimple with puckered edges." R. W. Smith, in describing his well-known case (*Treatise on Fractures*, p. 178), is at pains to distinguish between it and a dislocation of the shoulder. But as he describes the greater tuberosity lying under the acromion, and the head of the humerus at the inner side of the coracoid, though still within the capsule, it is evident that the dislocation was present as well as the fracture. It is of interest then to note that the symptoms he describes are great broadening of the shoulder, prominence of the detached tuberosity occupying the glenoid cavity, prominence of the head of the bone inside the coracoid, and the possibility of bringing the elbow into contact with the side. It is not unlikely that such a movement is made easy by the absence of the strain on the supra-spinatus and infra-spinatus, now that these muscles are detached from the bone.

It is quite possible, at any rate, that to the separation of the supra-spinatus and infra-spinatus in Dr Watson's case was due, in some measure at least, the difficulty in reduction. These muscles are attached to the humerus outside the anatomical neck, and act, in attempts at reduction, like the fulcrum of a lever, for they fix the tuberosity in such a way that the head is pushed outwards when the elbow is brought into the side. The rupture of this attachment, then, would obviously remove an aid to reduction.

An "open arthrotomy," as opposed to subcutaneous operation, is four times recorded as having been performed in order to reduce the dislocation, twice with success. Thiersch failed to replace the bone, while Albert unfortunately broke the humerus at the surgical neck in his attempts at reduction. Dr Garmany of New York was successful. Burkhardt of Wurtemberg operated successfully, though the reduction could not be effected without tearing off the greater tuberosity. "Three months after the operation the limb could be abducted 45°, and the hand placed on the opposite shoulder, on the top of the head, and behind the hips, but external rotation remained very limited." Dr Watson's case is an addition to these.

The readiness with which the joint can be opened, the ease with which obstacles to reduction can then be detected and removed, and the happy result in these successful cases, encourage the performance of the operation when cautious attempts by traction and manipulation have failed to reduce an old-standing dislocation.

III.—ON THE ONLY WAY OF RAISING THE TONGUE, THE EPIGLOTTIS, AND THE ARYTENO-EPIGLOTTIC FOLDS AT ONE AND THE SAME TIME.

By James Foulis, M.D., C.M.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 6*th March* 1889.)

Mr President and Gentlemen,—The object of the present paper is twofold. First, to demonstrate what are the natural bodily structures which occasionally act as valvular impediments to the passage of air into the lungs; and, secondly, to show that there is but one safe and thoroughly reliable way of removing the obstruction so produced in all cases of apnœa, whether occurring in poisoning, in chloroform or opium narcosis, etc., or in the apparently drowned.

Let me ask you now to perform the following experiment:—Place two fingers on the upper border of the hyoid bone, in the middle line, and then press with the finger-tips directly backwards, and at the same time try to draw in your breath. You will find that a very slight amount of backward pressure is sufficient to cause complete obstruction to the entrance of air into the lungs through the glottis. Indeed, you cannot possibly draw in your breath if you press backwards with your finger-tips in the way I have directed. Something seems to act as a valve, and it entirely prevents air passing downwards at this spot when the attempt is made to draw in the breath. The obstruction is not at the rima glottidis. It is much higher up. On removing the pressure you can draw in your breath with ease, but on reapplying the pressure the valve again acts, and cuts off the ingress of air. What is this something? It is the epiglottis.

If you look at this vertical mesial section of the head and neck, kindly lent to me for the purpose of demonstration by Dr Symington, you will see when I press back with the finger-tip at the spot, as you have just been doing, that the part which first comes in contact with the posterior pharyngeal wall is the epiglottis. When the finger-tips press backwards, they first of all push back the base of the tongue, which is attached to the hyoid bone, and the base of the tongue pushes back the epiglottis until the latter comes in contact with the posterior pharyngeal wall, against which it is flattened, and when an attempt is made to draw in the breath it acts as a valve and entirely prevents air passing.

On examining carefully the preparation, you will see that the epiglottis lies immediately behind and in contact with the hyoid bone and the base of the tongue, to both of which structures it is closely attached by ligaments, and that above the level of the upper border of the hyoid bone the epiglottis projects quite free for at least half an inch. It is this free edge of the epiglottis which is pushed back against the posterior pharyngeal wall when the fingers are made to press backwards as I have directed.

Now, in this case it is right to notice that the epiglottis does not fall down over the entrance into the glottis and thus prevent the ingress of air. It acts as a valve when it is pushed back, or when it falls back, and is flattened against the mucous membrane of the pharyngeal wall, the glottis being quite free. If, when the epiglottis is thus flattened against the pharyngeal wall, a strong expiratory effort is made, the free edge of the epiglottis can be made to vibrate coarsely; but any attempt to draw in the breath will fail as long as the epiglottis is thus flattened against the pharyngeal wall.

It is very important to observe also, while the fingers are thus pressing back the epiglottis and causing an obstruction to the ingress of air, that if the tongue is darted forward or forcibly protruded, the obstruction to the breathing at once disappears; and that as long as the tongue is thus forcibly protruded, backward pressure with the finger-tips quite fails to obstruct the breathing, even when the person experimented on is lying on his back.

The explanation of all this is easily given. In the first place, we must note that the natural position of the epiglottis is such that a very slight amount of backward pressure in the middle line is sufficient to flatten it against the posterior pharyngeal wall. In the natural position it must lie close to the pharyngeal wall, judging from the very short distance necessary to press it backwards with the finger-tips in order to cause complete obstruction to the breathing. The most important fact to remember is that the epiglottis is so closely and firmly attached both to the hyoid bone and to the base of the tongue, that the base of the tongue and the hyoid bone cannot be moved forward or away from the pharyngeal wall without dragging on the epiglottis.

When the tongue is forcibly protruded, the muscles which pass between the lower jaw and the hyoid bone, with the genio-hyoglossi muscles, so pull on the base of the tongue and the hyoid bone that the epiglottis is forcibly drawn away when these parts move forward. Hence, in our present experiment, although we can cause complete obstruction to the ingress of air by pressing back the base of the tongue and the epiglottis against the pharyngeal wall, this obstruction can be immediately overcome by darting forward the tongue, and as long as the tongue is forcibly protruded we cannot press back the epiglottis against the pharyngeal wall. If, while the finger-tips are resting on the tuberosity of the hyoid bone in the middle line, the tongue is forcibly protruded, the hyoid bone will be found to slip past the finger-tips. This movement of the hyoid bone tells us that both the base of the tongue and the epiglottis are at the same time drawn forward away from the pharyngeal wall.

These simple experiments teach us that if the base of the tongue and the hyoid bone can be drawn forwards, either by muscular action or by other means, it is quite impossible for the epiglottis

Plate I.

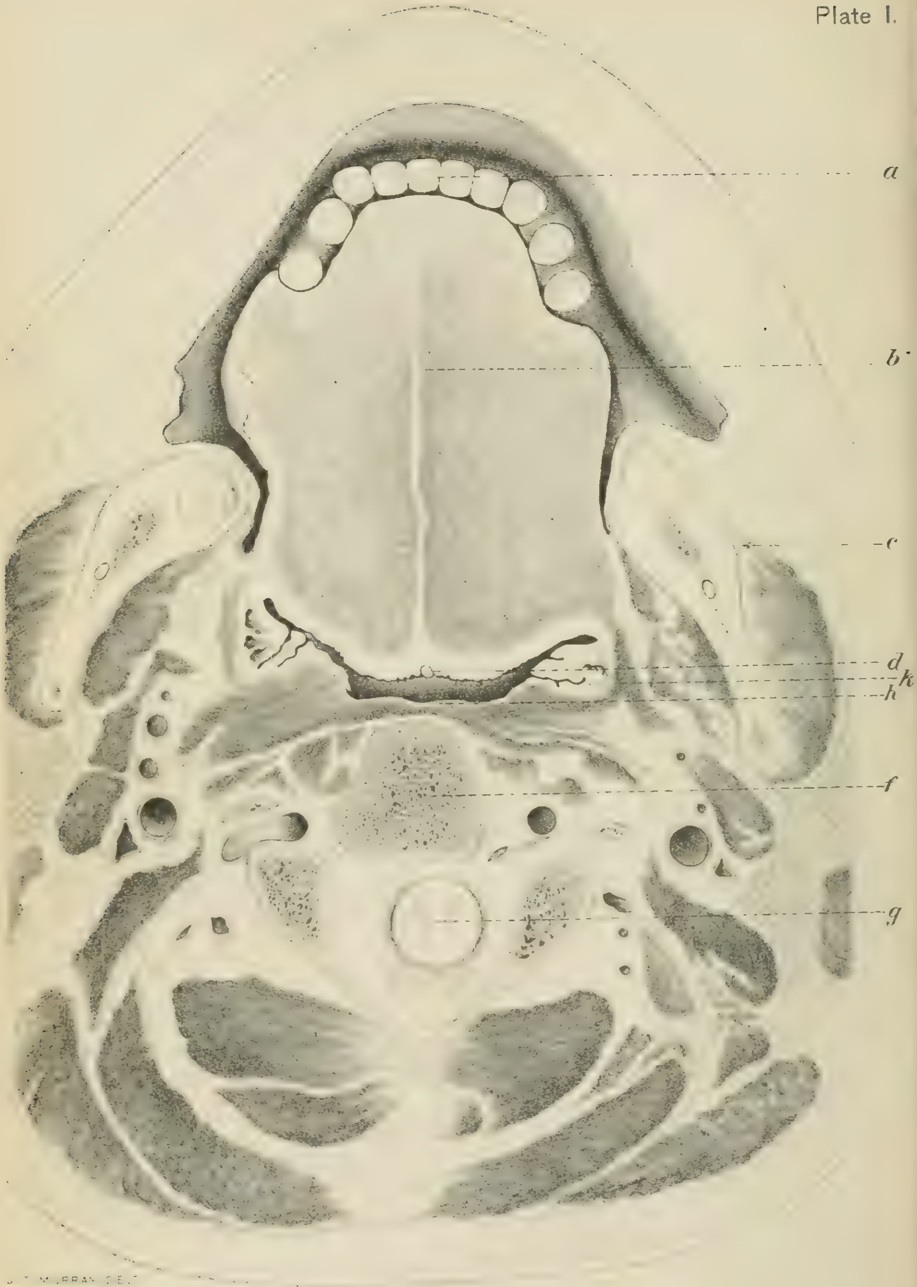


Fig. 1.

a. Incisor teeth.
b. Tongue.
c. Lower jaw.
d. Mucous membrane of tongue.
k. Tonsil.
h. Mucous membrane of post. pharyngeal wall.
f. Body of axis vertebra.
g. Spinal chord.

Plate II.

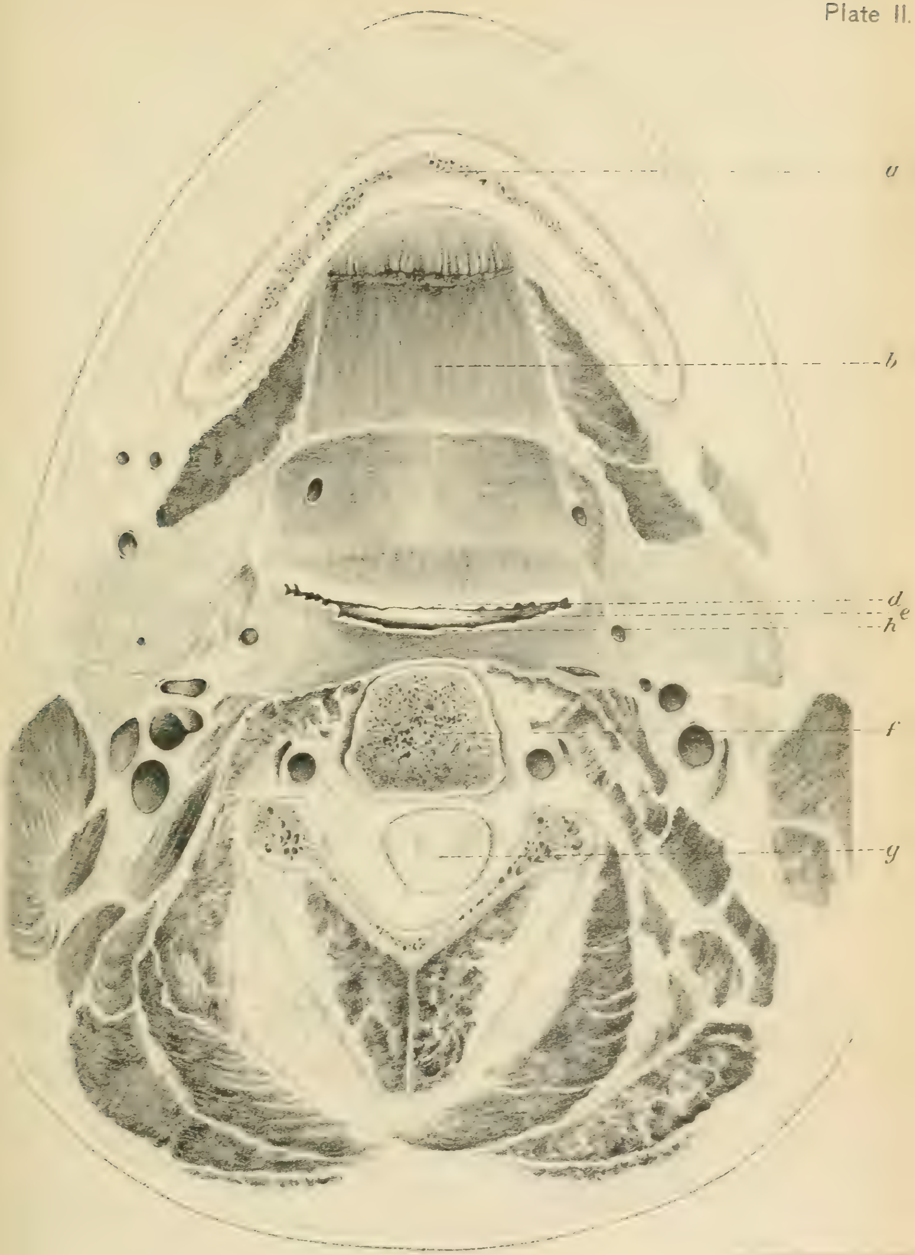


FIG. 2.

a. Lower jaw.
b. Tongue.
d. Mucous membrane of tongue.
e. Epiglottis.
h. Mucous membrane of post. pharyngeal wall.
f. Body of 3rd cervical vertebra.
g. Spinal chord.

to fall against the pharyngeal wall or in any way obstruct the passage of air into the lungs during inspiratory efforts.

If, however, the tongue and the hyoid bone are allowed to fall backwards as the result of loss of muscular tone, as in anæsthesia, or by mechanical pressure, there is always the great danger that the epiglottis will become flattened against the pharyngeal wall and thus obstruct the breathing.

Let me now show you two other preparations, which will, I think, demonstrate conclusively that in opium or chloroform narcosis, and in the apparently drowned, the mechanical obstruction to the breathing is caused by the tongue and the epiglottis lying against the posterior pharyngeal wall.

These two valuable preparations were lent to me for this demonstration by Dr Symington. The man from whom they were taken died as he lay on his back. As soon as possible after death the body was frozen, and sections were made through the mouth and pharyngeal region in the frozen condition, so as to catch the relation of the tongue and epiglottis to the posterior pharyngeal wall. These two sections demonstrate conclusively that when the muscles and tissues which attach the tongue and the hyoid bone to the lower jaw become relaxed, as in chloroform or opium narcosis, or in the apparently drowned, the tongue and the epiglottis will fall backwards until they come in contact with the mucous membrane of the pharyngeal wall. The thyrohyoid membrane is the hinge, as it were, on which the tongue and the hyoid bone *en masse* move backwards and forwards. The epiglottis lying immediately behind the base of the tongue and the hyoid bone, and being firmly attached to these parts, must necessarily move backwards and forwards with the hyoid bone and the base of the tongue, when the latter move in these directions.

Dr Symington thus describes these preparations:—

Fig. 1.—A horizontal section of the head of an adult male subject, as seen from above. The body was frozen lying on its back. The section passed through the mouth at the level of the edge of the lower lip and the upper parts of the crowns of the teeth of the lower jaw (*a*). Farther back, the vertebral column was divided opposite the body of the axis vertebra (*f*). The mucous membrane of the dorsum of the tongue was cut along a transverse line, at the level of the apex of the V-shaped group of circumvallate papillæ (*d*). For about half an inch on either side of the middle line this part of the tongue was separated from the posterior wall of the pharynx by an interval of about 3 mm. Farther outwards, the mucous membrane of the tongue was in contact with the tonsils (*k*). The cavity behind the tongue formed a curved slit with its concavity forwards, and about two inches in length.

Fig. 2.—A horizontal section $\frac{3}{4}$ of an inch below No. 1. This is also looked at from above. This section is through the body of the lower jaw (*a*) and the third cervical vertebra (*f*). The pharynx is

divided immediately above the epiglottis. The pharynx formed a transverse slit about 1½ inches in length, and was bounded in front by the mucous membrane of the tongue (*d*). Just below the plane of this section was seen the epiglottis (*e*), which projected upwards into the pharynx with its anterior surface in contact with the mucous membrane of the tongue, and its posterior against the posterior pharyngeal wall (*h*). The epiglottis was in contact with the posterior wall of the pharynx for fully an inch below the plane of this section.

In the first preparation attention is drawn to the close relationship of the tongue to the posterior pharyngeal wall at the situation of the V-shaped circumvallate papillæ. At this spot, for about half an inch on either side of the middle line, this part of the tongue was separated from the posterior pharyngeal wall by an interval of about 3 mm.; and farther outwards, on the same level, the mucous membrane of the tongue was in contact with the tonsils.

In the second preparation, which was made about three-quarters of an inch below No. 1, we find the pharynx was divided immediately above the tip of the epiglottis, which can be well seen in the drawing lying between the tongue and the posterior pharyngeal wall, and in close contact with both these parts. The epiglottis evidently has been pressed back against the mucous membrane of the pharyngeal wall, and lies flattened against it; and the remarkable fact comes out, on careful examination of this preparation, that the epiglottis was in contact with the posterior wall of the pharynx for fully an inch below the plane of the section. I think, then, we may conclude there is every probability that these sections represent very closely the relation of the epiglottis and the tongue to the posterior pharyngeal wall in all cases where the breathing has become obstructed in consequence of the tongue falling backwards as the result of relaxation of its muscular supports.

Granting, then, that the natural bodily structures which may seriously interfere with respiration when a patient is insensible, or under the influence of a poison or of an ordinary anæsthetic, are the tongue, the epiglottis, and the aryteno-epiglottic membranes, it is our object, in the next place, to show how these parts can be safely and completely removed in all cases of apnœa, and especially in those cases where artificial respiration becomes necessary as a means of reviving the patient.

With regard to any obstruction within the larynx itself, there is no evidence to show that the vocal chords seriously interfere with the passage of air into the lungs during the administration of an anæsthetic. One occasionally hears a peculiar laryngeal sound as if the vocal chords were in a state of tension, and were offering some resistance to the passage of air inwards; but it is very certain that if this noise results from muscular spasm within the larynx, the continued administration of the anæsthetic will quickly cause it to disappear. Not so, however, with the obstruction which occurs as the result of the tongue and epiglottis valving the air passage. The deeper the anæsthesia in this case the greater the danger of the obstruction, especially when the patient is lying on his back.

One of the first well-marked effects of chloroform anæsthesia is relaxation of muscles in all parts of the body.

When a patient is lying on his back in a state of chloroform narcosis, those muscles which attach the tongue to the lower jaw, to the hyoid bone, and to the styloid processes become relaxed, and from this cause the tongue with the hyoid bone, moving on the thyrohyoid membrane as a hinge, has a tendency to fall backwards and to come in contact with the pharyngeal wall, and during respiratory efforts the tongue, as a large ball-like flabby mass, may be sucked downwards till it presses against the posterior pharyngeal wall, in which position it, with the epiglottis, effectually prevents all inspiratory efforts.

Now, it is in the experience of all Edinburgh surgeons that, when the tongue has thus fallen back and the patient is in danger of suffocation, instant relief may be often given to the patient's breathing by dragging forcibly on the tongue with a pair of catch forceps. The method which seems to answer best is to drag the tongue forcibly *upwards* and forwards until the anterior fibres of the geniohyoglossi muscles are put on the stretch. To make the method effectual, the tongue must be dragged forwards with considerable violence. In this way the body of the tongue is drawn away from its contact with the pharyngeal mucous membrane, and allows the epiglottis to rise up by its own elasticity sufficiently to allow air to pass between it and the pharyngeal wall during inspiratory efforts. In the great majority of cases this method succeeds in relieving obstructed breathing; but all of us have seen cases in which this dragging on the tip of the tongue did not effectually relieve the obstruction. And the question has frequently been asked, Does this dragging on the tip of the tongue also drag on the hyoid bone and the epiglottis? If dragging on the tip of the tongue did draw away the hyoid bone and the epiglottis from the pharyngeal mucous membrane, we should have a certain method of relieving the obstruction in all cases where the tongue and the epiglottis were the cause of the obstruction; but experiments on the dead body have proved that dragging forcibly on the tip of the tongue does not drag upon either the hyoid bone or the epiglottis. It only stretches the fibres of the tongue until the anterior muscular fibres of the geniohyoglossi muscles and fibres of the styloglossi muscles are put on the stretch. Experiment has also shown that, before the hyoid bone and the epiglottis can be dragged upon in this way, the fibres of these muscles must be actually torn through. Dr Benjamin Howard, of New York, ten years ago demonstrated all this; and eight or nine years ago I also demonstrated these facts to this Society by means of numerous wet preparations, which were lent to me for the occasion by Professor Turner. If you now look at this preparation, which is a vertical mesial section through the head, neck, tongue, hyoid bone, and epiglottis, you will find that when I drag the tongue forcibly upwards and forwards, I simply drag the tongue away from the pharyngeal wall until the anterior fibres of the geniohyoglossi muscles and the fibres of the styloglossi muscles are put on the stretch, and there I must stop, or I shall tear through those muscles. You will see that, though I am dragging forcibly, the hyoid bone and the epiglottis are quite unaffected; but I wish to draw your attention to one important point, and it is this—if I now continue to drag forcibly upwards, you will see that the lower jaw is drawn upwards, and as a consequence of this upward movement of the lower jaw the muscles which pass between it and the hyoid bone are put on the stretch, and the hyoid bone is now pulled a little upwards, and the epiglottis moves with the hyoid bone, but the distance moved by the hyoid bone and the epiglottis is very slight.

We may then take it as a fact that dragging forcibly on the tip of the tongue with a pair of catch forceps cannot directly affect the hyoid bone or the epiglottis, except through the medium of those muscles which pass from the lower jaw to the hyoid bone.

I do not suppose there is any one present to-night who has not seen the good effect of dragging on the tongue in cases of obstructed breathing during chloroform administration. How then does this method act if dragging on the tip of the tongue does not drag either on the hyoid bone or on the epiglottis? It acts simply by stretching the fibres of the tongue in such a way that the tongue is drawn well away from the pharyngeal wall, and allows the epiglottis by its own elasticity to rise up from its contact with the pharyngeal wall sufficiently to allow air to pass downward during inspiration; but there is always this danger in the method—even though the tongue may be drawn quite away from the pharyngeal wall, and the epiglottis be not in actual contact with the mucous membrane of the pharyngeal wall, yet the epiglottis may lie so close to it from complete relaxation of the muscles which pass between the lower jaw and the hyoid bone, that there is always a danger of its acting as a valve during the inrush of air in inspiratory efforts. This is what undoubtedly occurs in those cases in which we see that dragging on the tongue more or less fails to give complete relief to the breathing. It cannot be too strongly insisted upon, that in such cases of obstructed breathing the obstruction is not caused by the epiglottis falling down on top of the glottis and thus closing the chink of the glottis.

If the muscles which pass from the lower jaw to the hyoid bone are quite relaxed, the base of the tongue, the hyoid bone, and the epiglottis may fall back against the posterior pharyngeal wall; but the chink of the glottis is not actually closed by the epiglottis falling upon it.

The air-passage, which naturally lies between the epiglottis and the pharyngeal wall, is a long narrow slit, and a very slight pressure by the finger-tips in the way I have described flattens the expanded epiglottis against the posterior pharyngeal wall. In the condition of relaxed muscular supports, as in deep anæsthesia, the very act of breathing inwards may cause the epiglottis to valve against the pharyngeal mucous membrane; and if it is true that dragging forcibly on the tip of the tongue cannot directly affect either the epiglottis or the hyoid bone, we are forced to the conclusion that this method of dragging on the tip of the tongue is not one that we can always depend upon, and therefore we must look for another method which will enable us, at one and the same time, not only to raise up the tongue from its contact with the posterior pharyngeal wall, but also to raise the epiglottis, and render tense the aryteno-epiglottic folds; for unless this is done, especially in cases where artificial respiration is necessary, the passage of air in and out of the lungs will not be free and unobstructed.

It was Sir Joseph Lister who taught us that forcibly dragging on the tip of the tongue relieved the breathing in a reflex manner. As I understood it, his theory was this—that the powerful stimulus of traction on the tip of the tongue set up in certain muscles a contraction, and this contraction acting on certain obstructing parts drew them away out of the road of the inrushing air. It is difficult to see how reflex action can in any way be brought into play in deep anæsthesia, and the deeper the anæsthesia the weaker is the reflex action; but I can quite believe that in the early stages of anæsthesia strong traction on the tip of the tongue may set up in a reflex manner a contraction in those muscles which pass between the lower jaw and the hyoid bone; and, as we know, if the hyoid bone is drawn away by this muscular contraction, the epiglottis will go with it. In the same way the hyoglossi muscles might be stimulated to contract, but there comes a time in deep anæsthesia when all reflex action is abolished, in which condition the stimulus of powerful traction will be of no avail.

My friend Dr John Duncan has suggested to me that this strong traction on the tip of the tongue acts mechanically on the hyoid bone and the epiglottis—that is to say, the strong traction necessary to put the anterior fibres of the geniohyoglossi muscles on the stretch if continued will draw up the lower jaw, and at the same time draw on the hyoid bone through the medium of the muscles which pass between the lower jaw and the hyoid bone.

For years past it has been my custom when giving anæsthetics in cases of obstructed breathing not only to push the jaw forward at its angles, but also to draw the lower jaw well forward by seizing it at the symphysis, which may be easily done by hooking a forefinger behind the front incisor teeth, and placing the thumb below the jaw at the symphysis. In this way, of course, traction acts on the hyoid bone through the muscles attached to it. In all our methods of relieving obstructed breathing, we must not forget that whatever acts on the hyoid bone acts similarly on the epiglottis.

The method of relieving obstructed breathing occurring during the administration of anæsthetics, by dragging forcibly upwards and forwards on the tip of the tongue, is now so well established in our hospital here, that it will be difficult to displace it by another method, even though it can be shown how a new one acts, and that it is a perfectly reliable one under all circumstances.

That dragging forcibly on the tip of the tongue is a valuable means of relieving obstructed breathing all of us are forced to admit; but few of us would assert that it is a perfectly reliable method in all cases, or deny that it is a very cruel method, especially in the case of young people and children.

We may have some doubts as to the effect on the epiglottis and the hyoid bone of forcible traction on the tip of the tongue with catch forceps; but there can be no doubt as to the effect on the epiglottis if we draw forward the base of the tongue and the hyoid bone.

Many of our leading surgeons years ago taught that hooking forward the base of the tongue and the hyoid bone with the forefinger was a reliable method of relieving obstructed breathing. The late Professor Spence taught this method to his class—any method by which the hyoid bone and the base of the tongue can be hooked forward will at once draw away the epiglottis from its contact with the pharyngeal wall.

The latest attempt to abolish the cruel method of forcibly dragging on the tip of the tongue with catch forceps is that which was embodied in a paper by Dr Benjamin Howard of New York, entitled, "A New and Only Way of Raising the Epiglottis." This paper was read before the Medical Society of London on the 20th of October 1888. Dr Howard undoubtedly, as the result of his anatomical investigations, saw that if the hyoid bone could be drawn forward, either by muscular or mechanical means, the epiglottis will move with it, and he has proposed the following plan as the best means of drawing away the epiglottis and of thus relieving the obstruction to breathing in a patient under the influence of an anæsthetic:—One hand is placed under the chin and another on the vertex of the skull, and the head and neck are now to be bent back until the spines of the vertebræ lock against each other and the skin in the middle line of the neck is rendered tense. When this has taken place, the muscles which pass between the lower jaw and the hyoid bone (the elevators of the hyoid bone) are so put on the stretch that the hyoid bone will be drawn well forward, and with it will move the epiglottis. The key to the situation is the hyoid bone, as Dr Howard has remarked. Now the object of this particular method is to draw away the epiglottis from the pharyngeal wall and to open up the post-oral breathing channel,—that is to say, the breathing must, in this method, take place through the nares, as the mouth is closed by the upward pressure of the hand which is placed below the jaw. The fact that the mouth is closed in this method is enough to condemn it, as we could not trust to it in cases of artificial respiration where everything depends on the rapidity with which air is pumped in and out of the lungs. As we all know, the nares are often obstructed with dirt and disease, and when the mouth is closed the free nasal flaps may act as valves during forced inspiratory efforts.

That this extreme extension of the head and neck, until the skin of the neck in the middle line is stretched, will render tense the muscles which elevate the hyoid bone no one will deny; but there are very grave doubts whether at the same time the large flabby tongue will be drawn away from its contact with the posterior pharyngeal wall even though the epiglottis itself may be drawn away.

If you turn to the *British Medical Journal* for 11th June 1888, you will find that Dr Jamieson of the Indian Medical Service has recorded a case in which Howard's method of head and neck extension failed to move the tongue away from its contact with the pharyngeal wall. Here is what Dr Jamieson said:—"The case was that of an old Hindu woman, aged about 50, who insisted on having chloroform for a slight operation. Heart and lungs revealed nothing abnormal on stethoscopic examination. The chloroform was administered by a senior student by means of a Junker's inhaler. After about one minute, respiration suddenly ceased, and I at once tried, by means of extending the head and neck to their full extent, to free the respiratory passages. This I found I could not do, for, on applying artificial respiration, air neither passed in nor out of the lungs. I at once passed my finger into the mouth, and found the tongue fixed at the back of the pharynx. The tongue was drawn forward, held with forceps, and artificial respiration resumed. Now I was at once enabled to pass air freely in and out of the lungs, and, after some five or ten minutes, had the satisfaction of seeing my patient recover."

Dr Howard, in reply to Dr Jamieson, thus writes in the *British Medical Journal* for 2nd February 1889:—"I am indebted to a recent correspondent of the *Journal* for the opportunity of adding to the prominence of a single point I had failed, perhaps, to make sufficiently conspicuous. Whereas by head and neck extension the raising of the epiglottis cannot be missed, the post-oral air-way is most easy altogether to fall short of. In every effort I have witnessed to do my method, as was thought, the first time, this is just what has happened: From an apparent fear of doing harm, that part of the extension I have described by the word 'utmost' has been stopped just short of, whereas this is just the part of it which, to secure the post-oral air-way, is indispensable. Even this will be ineffectual, unless with it is combined a perpendicular pharynx and a horizontal os palati. To dispel the fear of harm it should be understood that there is a definite and insurmountable safety check. This consists in the firm wedging together of the cervical spinous processes, which form a solid and unbreakable arch."

So that, according to Dr Howard, everybody who has tried his method for the first time has failed; and there is likely to be failure again, unless extension of the head and neck is made to the "utmost." That is to say, to make the method successful in every case, the neck must be bent back until there is a "firm wedging together of the cervical spinous processes, which form a solid and unbreakable arch." "Even this will be ineffectual, unless with it is combined a perpendicular pharynx and a horizontal os palati."

(*To be continued.*)

IV.—ON THE LIGHT WHICH SECTIONAL ANATOMY WILL THROW UPON THE MECHANISM OF LABOUR.

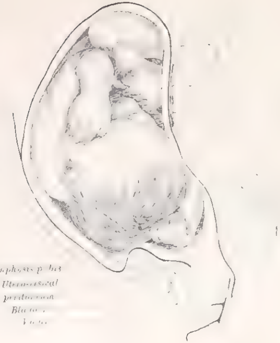
By A. H. F. Barbour, M.D., F.R.C.P.E., Lecturer on Midwifery and the Diseases of Women in the School of Medicine, Edinburgh, etc.

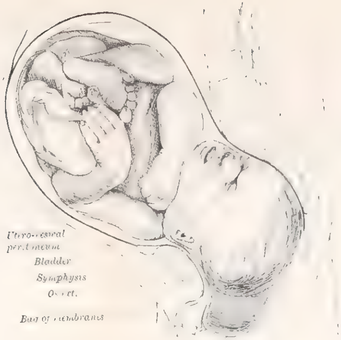
(*Read before the Edinburgh Obstetrical Society*, 13*th February* 1889.)

Labour is a process extending through successive stages, and implying various movements on the part of the body passing along the genital tract. "Mechanism of Labour" is the term applied to the movements of the whole or of parts of the child during the Second Stage as it passes through the pelvis. These movements have hitherto been studied by two methods: theoretically with a fœtal skull and dry pelvis, and clinically on the living subject; and there has been gradually evolved, as the product of an enormous amount of patient investigation, a systematized account of the movements which has been formulated in our text-books as the Mechanism of Labour. A third method of inquiry has been recently introduced—that of making frozen sections. This last is of recent date, only of the last fifteen years; and the results are as yet scanty and to be received with caution. Hence in this short paper I intend chiefly to indicate the light which Sectional Anatomy is fitted to throw on the Mechanism of Labour, and to refer to questions which it has raised without as yet answering them.

In indicating the lines along which Sectional Anatomy must advance to gain the information hitherto wanting, I would first draw attention to the point that the sections hitherto published have been mostly vertical mesial, and hence only give us the relations in the

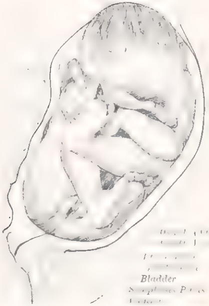
From BRAUNE'S SECTION before Labour: a head presentation in right occipito-anterior.

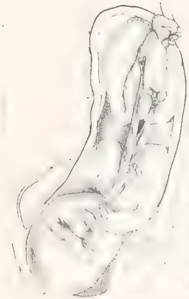




From BRAUNE'S SECTION of the Second Stage (during a pain): unruptured bag of membranes at the vulva, and head in part in vagina.

From WALDEYER'S SECTION before Labour: a breech presentation with [illegible] to the left.





From CHIARI'S SECTION of the Second Stage: with membranes unruptured, and head descended to the perineum.

THE FŒTUS BEFORE LABOUR AND DURING THE SECOND STAGE.

[illegible], the [illegible] of the limbs, and the attitude of the head in the two periods; further, the change in long axis produced [illegible].

Compare (after Waldeyer and Chiari's) the cervical canal, the [illegible], and the position of the utero-vesical peritoneum and bladder.

middle plane of the body. The few facts with regard to the cervical canal, the displacement of the upper portion of the bladder, and the utero-vesical peritoneum, and the extent to which the bony pelvis is encroached on in the conjugate diameters, are of great value (see Plate); but much more is wanted. To get the desired information as to the mechanism, sections must be made cutting the pelvis in those diameters and planes which are concerned in the Mechanism of Labour. Sections from above downwards in the transverse diameter and axis of the brim, and in the oblique diameters and axis of the brim, would be invaluable; and cross sections in the various planes should also be made. Veit has recently published some cross sections of the pelvis in the non-pregnant female; but to get the required data, these must also be made for cases in labour, because the distribution of the cellular tissue is different in pregnancy, and some parts will yield more than others before the child's head. Such sections, when made in that diameter in which the head is for the time lying, will show its shape, for the head will be bisected so that we can measure it: *e.g.*, the most instructive section of a case with the head lying L.O.A. would be one made through the body in the direction of the right oblique diameter of the pelvis. Such sections will further show us what the movements of the mechanism of labour actually are. To catch the movements of the head of a fœtus as it passes through the pelvis is about as difficult as to catch the movements of the limbs of a horse trotting along the road. Instantaneous photography has got over the difficulty in the latter case by catching the limbs at various points in their movements and fixing them; and by comparing a series of such photographs we can follow the movements throughout. What instantaneous photography has done for the horse trotting on the road, frozen sections will do for the child passing through the pelvis. The translator of Chiara's *Atlas* makes a happy hit in the title by his very literalness: it is a fœtus undergoing spontaneous evolution, and "*caught in the act* through corpsy congelation."

Accordingly, sections should be made in the following directions. 1. *Longitudinal sections in one of the diameters of the bony pelvis, either the transverse or the oblique.*[1]

2. *Cross sections of the pelvis at right angles to the axis of the canal and in one of the recognised planes of the pelvis:* (1), in a horizontal plane passing either through the conjugate of the brim, or (2), at a slightly lower level through the transverse diameter (for we must remember that, owing to the high position of the promontory, the conjugate is above the transverse diameter); (3), in a plane passing through the lower edge of the symphysis and parallel to (1); (4), in the conjugate of the cavity; (5), in the conjugate of the outlet.

Having thus indicated the scope of Sectional Anatomy and the nature of the results to be expected from it and given these hints as to the most profitable mode of conducting this investigation, I wish in the rest of this paper to draw attention more specifically to certain questions which these sections raise, though they have not as yet answered them:—

---

[1] Sections in the conjugate, which has been the line always chosen hitherto, will not tell us so much unless the head be just at the vulva.

1. What are we to take as the starting-point in the Mechanism of Labour: at what point in the bony pelvis does the Mechanism of Labour begin?
2. How is the form of the bony pelvis modified by the soft parts?
3. What is the form of the fœtal head?
4. What are the movements of the Mechanism of Labour?

1. *Where does the Mechanism of Labour begin?*

From the text-book descriptions the student carries away the idea that the Mechanism of Labour begins at the brim of the pelvis. This idea has been gained, I believe, from our working out the mechanism theoretically with a fœtal skull in one hand and a pelvis in the other. A study of the frozen sections of the First Stage published has led me to doubt the correctness of this description, and to ask whether the Mechanism of Labour should not rather be described as beginning in the cavity. In primiparæ it is an established fact that the head is already in the pelvis, but in multiparæ we are in the habit of describing the head as being above the brim, and of making the mechanism begin there. Yet in the three frozen sections of multiparæ[1] dying in the First Stage we find the head in the pelvis. Now, in these cases, though labour has begun, the Mechanism of Labour has not yet begun—for that belongs to the Second Stage, and, as a matter of fact, we can see that it has not begun in these sections. It is to clinical experience, however, that the final appeal must be made to settle this question; and the condition present in these sections has led me to note carefully the position of the head in multiparæ at the beginning of the Second Stage. It is very difficult to locate the head in the pelvis exactly, but I have satisfied myself that it is *in the cavity*; and I should like to know whether the wider experience of the Fellows confirms this observation.

2. *How is the form of the bony pelvis modified by the soft parts?*

As already said, the three sections from the Second Stage are all vertical mesial, and therefore show us the canal only in the conjugate diameters. In Braune's we have the conjugate of the bony pelvis diminished by ·4 in. at the brim, and ·7 in. in the cavity; while in Chiari's the room taken up by soft parts is 1·4 in., the increase being due to moderate distension of the bladder and rectum. In Chiara's, again, the diminution is only ·2 in. at the brim, but ·7 in. in the cavity. The ·2 in. at the brim represents an abnormal degree of compression of the soft parts due to the pressure implied in spontaneous evolution, and perhaps because the bladder is drawn right up out of the way. The diminution in the conjugate produced by soft parts we may therefore put down provisionally as ·4 in. at the brim and ·7 in. in the cavity. We have no data for the outlet from these sections.

---

[1] By Schroeder, Winter, and myself. In Winter's the head is in the pelvis although the placenta is prævia and compresses the placenta.

As regards the transverse diameters, we must turn to my own sections of the First Stage. This coronal section[1] shows very well the side walls of the pelvis with their padding of soft parts. It diminishes the space between the bones by almost an inch (·8 in.) at the brim, and rather more in the cavity (·9 in.). But this padding is composed one-half of fatty cellular tissue, which, as before mentioned, would be easily compressed by the head, and 1 in. therefore exaggerates the amount of diminution. As regards the oblique diameters, this coronal section[2] through the acetabulum shows that while at the brim ¼ in. represents the amount of shortening at its anterior end, in the cavity the muscular part of the internal oblique encroaches very distinctly on the available space—taking up ½ in., to which we must add another ¼ in., for the fatty cellular tissue and uterine wall. Finally, this section[3] which I made of the back wall of the pelvis in Hodge's first parallel plane, to ascertain the parts concerned in rotation, shows that there is no muscle here to act as an elastic pad, although the oblique diameters are shortened in the cavity posteriorly by ·3 in. on the right and ½ in. on the left—by the thickness of the uterine wall and the cellular tissue of the utero-sacral ligaments.

The different degrees of shortening, then, are: (at the brim) conjugate by ·2 in., transverse by a reducible 1 in., obliques by ·25 in. at their anterior ends; (in the cavity) conjugate by ·4 in., transverse by 1 in., obliques by ·75 in. *plus* a reducible ·5 in.

3. *What is the form of the fœtal head?*

For this we have data from one section only. By a fortunate accident, one of my sections has almost bisected the head antero-posteriorly. The measurements here are:

Sub-occipito-bregmatic diameter,	. .	4¼ in.
Occipito-frontal	„	4½ in.
Occipito-mental	„	5 in.

These figures include the soft parts covering the skull, and are of the unmoulded head—before labour.

4. *The Mechanism of Labour.*

This is usually analysed into four movements: flexion, internal rotation, extension, and external rotation. The last two occur in the act of and after the birth of the head, and are therefore visible. The first two take place in the pelvis, and can be observed only with the hand. Any one who has tried, with finger on the posterior fontanelle, to follow the mechanism of labour throughout, will know how difficult it is to judge in that way of the movement of flexion. We analyse the progress of the child's head through the pelvis into movements as we may the progress of a ship through the water. The head flexes and extends as a ship pitches, it rotates as a ship rolls, it descends as a ship moves forwards; to follow the occiput in its course is as hard as to follow that of a ship's bow through the sea. Sectional Anatomy will, however, catch the head "in the act," and give us facts instead of fancies.

---

[1] Pl. vi., fig. 1, of my *Atlas of the Anatomy of Labour, etc.* W. & A. K. Johnston, Edinburgh.

[2] *Ibid.*, Pl. vi., fig. 2.

[3] *Ibid.*, Pl. iv., fig. 2.

I have put side by side the sections from before labour and the second stage, so that we may study the fœtus especially in them (see Plate), and would draw attention to the following points:—

In Braune's section, the head has descended until its vertex has reached a point 4·2 in. below the brim, and is lying in part in the vagina. I mention this point emphatically, because some one may say the membranes have not ruptured, and therefore labour is still only in the First Stage. The portion of the head which is engaging is a plane parallel to a line joining the supra-orbital ridge and the top of the ear; and if we look at the drawing of the child removed from the body, the part which is highest (and would be lowest or "present" in labour) is a point 1½ in. above the posterior fontanelle, *i.e.*, the fontanelle would need to dip 1½ in. lower to become the lowest or presenting part of the head. Both of these facts show that flexion is only partial here.

In Chiari's, the head has descended to the perineum, the vertex being 4·9 inches below the brim. It is lying in the transverse diameter of the pelvis, with the occiput to the left side. This points to its having been probably a left occipito-anterior position (for a left occipito-posterior is extremely rare), which has descended through the pelvis in the transverse diameter without rotation of the head. The attitude of the child in almost full-faced view shows that flexion is partial.[1]

Now, the usual attitude of the fœtus in utero is one of flexion. It grows flexed. If we compare the frozen section of the child before and during labour, we find that if any change has taken place it is rather an undoing than an increase of the normal attitude. I speak, of course, of labour in a normal pelvis, and we must wait for other sections; but those already made go to prove that *flexion belongs to the growth of the fœtus* (if a teleological argument were admissible, we might say it grows in that attitude in which it can most easily escape from its confinement) *rather than to the mechanism of labour.*

---

[1] Further, while the head has descended almost to the outlet, passing through the various planes of the pelvis only semi-flexed, its occipito-frontal plane (*i.e.*, a transverse plane passing through its occipito-frontal diameter) is not parallel to, but forms an angle open to the back with the plane of the outlet of the pelvis (*i.e.*, a transverse plane passing through the conjugate of the outlet). No movement of "synclitism" (parallelizing) has taken place, *i.e.*, the head has not adjusted itself so as to make that plane which was parallel to the plane of the brim when the head entered the pelvis likewise parallel to the plane of the outlet when the head leaves the pelvis.

The elongation of the fœtal ovoid with the compression of the limbs, during the Second Stage, is also of interest; and the fact that, during a pain, its long axis is thrown forward into the axis of the pelvic inlet is of significance with regard to its being made to enter the pelvis with a minimum of resistance.

V.—ON A VARIETY OF POST-PARTUM SHOCK: ITS NATURE, CAUSE, AND PREVENTION.

By J. Haig Ferguson, M.B., M.R.C.P.E., M.R.C.S., Clinical Gynæcological Tutor, Royal Infirmary; Physician to the New Town Dispensary, and late President, Royal Medical Society, Edinburgh.

(*Read before the Obstetrical Society of Edinburgh, 8th May* 1889.)

In this paper I desire to make some observations on three cases of post-partum shock which have come under my notice, one in my own practice and two which I have had the privilege of seeing with Dr Halliday Croom, to whose kindness I am indebted for the liberty of bringing them before you this evening. The cases are shortly as follows:—

Case I.—Mrs L., aged 28, was delivered of her fourth child in the end of November 1885. The patient is of a highly nervous temperament, and her family history shows a distinctly neurotic tendency. One sister frequently developed a neurotic temperature of 108° F. A second sister had a persistent nervous cough for months, and their mother was subject to fits of catalepsy. The whole family has been under Dr Croom's personal observation for years. The previous labours were normal. The labour was an unusually short one, the child being born four hours after the commencement of pains, and the patient required neither chloroform nor forceps. The child was born half-an-hour before Dr Croom's arrival. The nurse, who was a more than usually competent woman, grasped the uterus after the birth of the child and compressed it firmly till his arrival. During that time it will be observed the uterus was firmly and forcibly compressed by a nurse thoroughly conversant with the Credé technique of delivering the placenta. The patient complained of much pain during the kneading of the uterus. The uterus contracted well, and there was no hæmorrhage. Soon after the expulsion of the placenta she suddenly became unconscious, and remained so for nearly three hours. The nurse, a most experienced person, to whom I wrote for some particulars of the case, writes as follows:—"I have seen a few people die, and I never saw any one look more like it than she. She went all through the death struggles, and had the peculiar pinched look in the face of a dying person. She then became as if she were dead, and remained so for over three hours with no pulse at all. Then gradually the pulse began to flutter, and about half-an-hour after that she was able to take a little stimulant and then talk." Such is the nurse's graphic description of the case. The patient's symptoms appeared so urgent that her husband was called up, as Dr Croom supposed her to be in the very article of death. Her subsequent puerperal history was perfectly normal.

Case II.—Madame de B., aged 27, was delivered of a male fœtus in December 1887. She was a healthy young primipara. The labour was perfectly normal, except that from rigidity of the external parts secondary inertia threatened to set in, and the operation of low forceps was accordingly performed by Dr Croom. After waiting a reasonable time for the delivery of the placenta, it did not seem to separate from the uterus, and Dr Croom proceeded to apply the Credé method of delivery. The patient, who was semi-anæsthetized, complained of much pain during the process. After considerable difficulty and prolonged pressure the placenta had to be extracted with the hand. There was hæmorrhage, but only slight. Immediately after the extraction of the placenta the patient became pulseless, cold, pale, and gasping. This condition was promptly combated by the injection of ether, and for some time her pulse improved and her general condition was somewhat better. In half-an-hour she was again completely collapsed, and grave fears were entertained for her life. Ether and rectal injections were again employed, and again she rallied. This sequence of events occurred at intervals during the evening. As her condition seemed to be grave at midnight, I remained in the house all night and treated her freely with stimulants. In the morning she was better, and she made an uninterrupted recovery.

Case III.—Mrs L., aged 27, was delivered of her first child in January 1888. The patient is a healthy, well-developed woman of a somewhat nervous temperament. Her first stage lasted about eight hours, her second stage about three. The passages were roomy and moist, and dilatation proceeded satisfactorily. The head at the beginning of labour was right occipito posterior. It rotated forwards, however, of its own accord. A little chloroform was administered towards the end of the second stage, and delivery was hastened by forceps as the patient was beginning to get somewhat exhausted, and the pains were commencing to be less energetic. After the birth of the child she partially awoke from anæsthesia, and complained loudly of excessive pain when the uterus was grasped, and begged hard that it should be desisted from. The pain made her feel sick and faint, and it was intensified each time the uterus was compressed, till it became quite unbearable. Just as the placenta was expelled from the vulva the patient suddenly became unconscious. A certain amount of restlessness and uneasy moving about in bed preceded the loss of consciousness. The uterus was firmly contracted, and there was no hæmorrhage either from it or from the cervix or vestibule. The pulse could not be felt, the breathing was almost stopped, and the patient appeared to be on the point of death. Several times, indeed, she seemed actually moribund. I was greatly alarmed by these symptoms, and as I feared that the patient was going to die in my hands, I asked Dr Croom to come and see her. This he kindly did, and he likewise shared in my anxiety. She lay in this condition for nearly three hours, and then gradually recovered consciousness.

The patients, it will be seen from the above reports, were all young, healthy women of decidedly nervous temperaments, two of them primiparæ and one a iv.-para. There was nothing specially remarkable about the first or second stage of labour in any of them, except that two were low forceps cases, and two of them had chloroform administered towards the end of the second stage. Forceps were applied to prevent exhaustion and for fear of impending inertia. None of the patients lost more than about twelve ounces of blood in the third stage, their uteri contracted firmly and well, and in no sense of the word did they suffer from post-partum hæmorrhage, concealed or otherwise. Eclampsia and cardiac disease could be clearly eliminated in all three. Yet they all manifested decided symptoms of syncope and shock, became perfectly unconscious, with feeble, rapid, irregular, sometimes imperceptible radial pulse, dilated pupils, shallow irregular breathing, and cold, clammy perspiration. The face, at first anxious, soon developed a vacant, torpid expression, and sensibility to pain absolutely and entirely disappeared.

This state of matters lasted for about three hours—fully longer in one of the cases—and there was of course complete inability to swallow. Free injection of stimulants both into the rectum and under the skin, and champagne by the mouth as soon as the power of swallowing returned, gradually, as it were, brought them back again to life. Sinapisms were freely applied to the præcordia, and it was with great difficulty that the body heat could be maintained in spite of hot-water bottles, hot blankets, and gentle massage. In from eight to twelve hours they had, to all appearance, quite recovered from the effects of the shock, and beyond a feeling of exhaustion, and some degree of tenderness in the lower part of the abdomen, nothing unusual was to be remarked. Their memory was a complete blank during the greater part of the attack.

In two of the cases the subsequent puerperal history was perfectly normal in every respect. In the third persistent sleeplessness was a source of trouble, with occasional incoherence. This ultimately developed into a mild but prolonged attack of puerperal melancholia, terminating, however, in complete recovery.

What, then, was the cause of these alarming manifestations? For alarming they most certainly were, even for a time making one fear that life had become almost extinct. And what lesson can be gathered from these three cases, so alike in their symptoms and in their termination?

Firstly, as to the cause. It will be observed that nothing unusual occurred in the first or second stage in any of the cases, and that the patients up to the beginning of the third stage were in an entirely satisfactory condition. It is to the third stage, then, that one must look for an explanation of the phenomena.

All the patients complained of unusual tenderness when the uterus was grasped by the hand in the usual way through the abdominal wall after the expulsion of the child; and when the uterus was compressed more forcibly, according to Credé's method, in order to squeeze out the separated placenta, the pain became so intolerable as to completely awaken them from their partial anæsthesia, and they shrieked in agony, declaring it to be worse than the whole labour put together. In one of the cases the uterus was somewhat soft and flabby, and it had to be kneaded pretty energetically before it contracted satisfactorily. There was, however, no hæmorrhage to speak of. The shock in each case occurred either during or immediately after the expulsion of the placenta from the vagina, and when the uterus was being firmly grasped through the abdominal wall. There can be no doubt that the cause of the symptoms was the fact that one or both ovaries were being squeezed and bruised by the pressure of the external hand compressing one or both against the hard contracted uterus. This accounts equally for the exquisite pain from which the patients suffered when the uterus was being compressed, and for the syncope which supervened on the intense shock thus produced.

I have repeatedly seen an analogy to this occur in cases of abdominal section for the removal of the ovaries and Fallopian tubes. If, as sometimes happens (especially in cases of fibroid tumour, where the appendages are not easily brought to the surface), the ovaries should be somewhat roughly handled or accidentally torn, say by a pair of forceps, before the ligature has been tied, it is a matter of common observation that the patient, though fully anæsthetized, in the majority of cases becomes affected by modified shock. The pupils dilate, the face becomes deathly pallid, the extremities cold, a clammy perspiration breaks out, the pulse flutters, and the respirations become shallow and irregular. These symptoms pass off when the ligature is tightened, and the organ removed. If such a condition happens when atrophied, cystic, or even cirrhotic ovaries are irritated, how much more is it likely to occur when the large, congested, tender ovaries of pregnancy are forcibly compressed against the hard post-partum uterus, and when the patient is either not under the influence of an anæsthetic or is only partially anæsthetized.

I have not been able to find any description of such a danger as this in the application of Credé's method; and as it seems to me of some importance and of very considerable clinical interest, I have ventured to bring the matter before the Society to-night. The reason such an accident does not occur more frequently is because the ovary, as a rule, slips away from beneath the grasp of the hand and so escapes injury, or at least is but slightly irritated. I cannot but think it, however, of the highest importance that the avoidance of such a risk as I have been endeavouring to describe should not be left to the off-chance of the ovary accidentally eluding the grasp of the compressing hand, but that the accoucheur should have a clear idea that if he grasp the uterus in a certain way he will, as far as it is possible to do so, avoid coming in contact with the ovaries. It is to this point I wish specially to direct attention, as no notice is taken of it in the well-known descriptions of Credé's method.

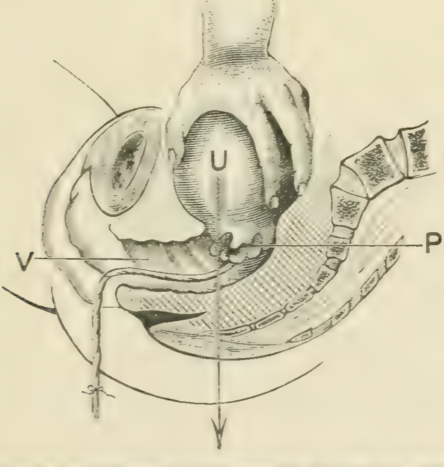


FIG. 1. Showing the position of the hand, as recommended by Credé and others, in grasping the uterus in the third stage. The fingers are in front of the sacral promontory, the thumb behind the symphysis pubis. The arrow shows the direction in which pressure is to be made (Credé).

Spiegelberg says, in describing the management of the third stage,—"The hand grasps the uterus in such a way that its ulnar side sinks deeply down behind it, the entire hollow of the hand lying on the fundus and the thumb on the anterior wall." Playfair and Lusk say exactly the same, as also does Charpentier, and Grandin of New York says that "the uterus is grasped by both hands, the fingers towards the pubes, and compression is made." Credé figures the hand grasping the uterus with the thumb behind the symphysis pubis and the fingers in front of the promontory of the sacrum (Fig. 1).

Now, from these and many similar statements it is inferred, and Grandin distinctly states in effect, that in order to grasp the uterus antero-posteriorly the ulnar side of the compressing hand must be in relation to the promontory of the sacrum posteriorly and the thumb behind the symphysis pubis. What I wish to point out is, that if the hand is in this position the uterus is not, as a rule, grasped antero-posteriorly, and that the ovaries are consequently in imminent danger. In other words, *that the term antero-posterior* quâ *the pelvis is not antero-posterior* quâ *the uterus in the majority of cases.* As is well known, the uterus in pregnancy becomes usually rotated to the right on its longitudinal axis, so that the left ovary comes to lie more anteriorly, while the right ovary lies more posteriorly. In other words, the transverse axis of the uterus lies diagonally in the pelvis in the right oblique diameter. This has been conclusively proved from post-mortem examination, and I have had an opportunity of verifying this observation on two occasions. If, as is more than probably the case, this rotation of the uterus to the right accounts in great measure for the fact that the vertex of the fœtal head, in 99 per cent. of cranial cases, lies in the right oblique diameter of the pelvis, one can form some idea of the frequency of this condition. Barbour, in his "Anatomy of Labour," has observed one case in the first stage of labour where the right ovary was opposite the centre of the crest of the ilium, and the left ovary about an inch higher than the right, and lying just above the left posterior superior iliac spine. The transverse axis of the uterus was in this case tending to lie in the left oblique diameter of the pelvis, and the vertex of the fœtal head was likewise tending to present in the left oblique diameter. It is possible that when the uterus contracts this tendency to rotation is increased, and that it becomes more pronounced as labour advances. After the expulsion of the child this rotation still persists, so that the left ovary lies forwards and to the left, and the right ovary backwards and to the right. It is to be remembered that in pregnancy the ovaries are in close contact with the lateral walls of the uterus, owing to the shortening and splitting up of the broad ligaments.

Börner, who has investigated this subject, states that out of 64 post-partum cases which he observed, in 50 rotation was entirely absent; in 12 the rotation was to the right, and in 2 to the left. Pfannkuch has observed that when the bladder is full the rotation is increased. Dr Croom found that in 40 cases which he noted, 10 showed distinct rotation to the right. These observations were made on uteri several days post-partum, and where in all probability the contraction of the round ligaments had modified the position of the uterus, so that these statistics do not bear directly on the subject of the present paper. I am not prepared to say the proportion of cases in which rotation of the uterus is present in the third stage and immediately post-partum, but I feel sure, from clinical observation, that the proportion is a considerable one.

It will thus be seen that if the hand attempt to compress the uterus in an antero-posterior direction *quâ* the pelvis, that is, with the ulnar side of the hand in front of the promontory of the sacrum and the thumb behind the symphysis pubis, the ovaries in many cases run great risk of being included in the grasp. In the majority of cases they escape by slipping from beneath the fingers as I have stated above, or in the attempts which the hand makes to grasp the uterus the accoucheur accidentally and quite unwittingly twists the uterus round, so that its antero-posterior axis and the antero-posterior diameter of the pelvis correspond. It is just possible that the contraction of the round ligaments in some cases may produce the same effect in straightening the uterus at this time. It is often a difficult thing in the third stage of labour and immediately post-partum to tell clinically which is the anterior and which the posterior surface of the uterus, as, on account of its then having a somewhat spherical shape there are no definite landmarks to guide one, unless one can feel the contracting round ligaments, which it is not always easy to do. To avoid the danger

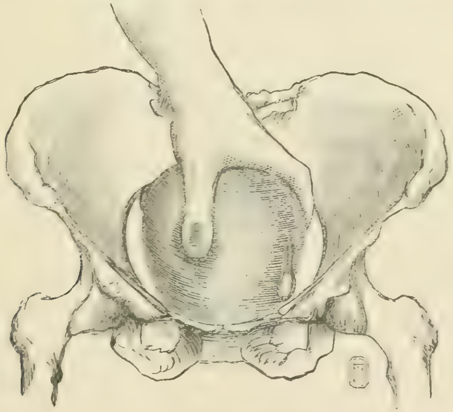


FIG. 2. Diagram to show the oblique position the uterus occupies in the pelvis after the second stage of labour. The uterus is represented as being pushed down in the pelvis by the hand, which is grasping it antero-posteriorly. Note the oblique position of the hand in relation to the pelvis. It will be seen that the right ovary lies on a higher level than the left.

of touching the ovaries, and to make sure of grasping the uterus antero-posteriorly, the hand should be passed into the brim of the pelvis obliquely, and as we can almost infer that in 99 per cent. of all vertex cases the uterus is rotated to the right, the ulnar side of the hand should be pressed deeply down in the direction of the left sacro-iliac synchondrosis, while the thumb should be behind the right ilio-pectineal eminence, the fundus of the uterus resting in the hollow of the palm. In the vast majority of cases such a grasp of the uterus will entirely avoid the ovaries, and firm compression can be safely made without causing the patient much pain or inconvenience (Fig. 2).

When a patient is lying on her left side in the usual obstetric position, it will be found that the operator's left hand involuntarily grasps the uterus in an *oblique direction quâ* the pelvis, and that the attempt to grasp it in an *antero-posterior direction quà* the pelvis involves a twisted and constrained position of the hand which it would be impossible to keep up for any length of time, and would absolutely prevent the bringing into play of any degree of muscular force. Any one can prove this for himself in his daily experience. From this it will be observed that when the patient is lying on her left side, the accoucheur naturally grasps the uterus obliquely *quâ* the pelvis, and consequently, as a rule, antero-posteriorly as regards itself, so that the ovaries usually escape danger. When, however, the patient is lying on her back, this fortuitous circumstance does not come into play, and therefore there is undoubtedly more risk of the ovaries being compressed when the patient is lying in this position if the rotation of the uterus be not borne in mind.

In many cases the employment of the hand through the abdominal wall is simply a precautionary measure for the purpose of detecting and preventing relaxation of the uterus. In these circumstances no great pressure is requisite, and the ovaries are consequently not endangered.

If a patient should complain of severe pain during the manipulation of the post-partum uterus, one should always fear that the ovaries are being compressed at the same time. It is in some cases easy to feel the ovaries through the abdominal wall, and thus one is enabled to avoid them. It depends on the temperament of the patient to a great extent, as well as on the amount of bruising to which the ovaries are subjected, whether the symptoms produced by compression of the ovaries will be severe or not. True, it is not often that such alarming reflex symptoms occur as were observed in these three cases, yet there can be no doubt that the ovaries should, as far as possible, be guarded from all sources of irritation, and that every care should be taken to prevent their being injured during the third stage of labour and immediately post-partum.

If such accidents happen in the hands of those who have made a special study of the department, how much more are they likely to occur in the hands of men who Credé the uterus on no special principle, and who are only too glad to get the placenta squeezed out in any way. But it will be said no such cases have been recorded. One, however, constantly reads and hears of cases where so-called syncope has come on in the third stage or immediately thereafter. The uterus had been a little relaxed, and there had possibly been a slight threatening of hæmorrhage. The syncope is ascribed to the trifling loss of blood, often not in excess of what one would expect in a normal labour, instead of being put down, as I think in many cases it should be, to the energetic and forcible kneading which the ovaries along with the uterus have been subjected to, in the over-zealous but well-meaning efforts of the accoucheur to avert a more or less visionary post-partum hæmorrhage; the so-called syncope being the result of shock pure and simple.

There is only one condition, so far as I know, where the post-partum uterus should, if possible, be grasped laterally through the abdominal walls, and that is in the operation for washing out its cavity. The lateral grasp is for the purpose of compressing the Fallopian tubes in order to prevent any of the fluid from passing through the tubes into the abdominal cavity. The compression in such cases requires only to be very gentle, so that there is little fear of injuring the ovaries.

VI.—TWO SUGGESTIONS FOR IMPROVING THE OPERATION OF EXCISION OF THE KNEE-JOINT FOR STRUMOUS DISEASE.

By A. G. Miller, M.D., F.R.C.S. Ed., Surgeon to the Edinburgh Royal Infirmary.

(*Read before the Edinburgh Medico-Chirurgical Society*, 15*th May* 1889.)

More than twenty years ago, when I was house-surgeon to the late Professor James Spence, I learned that to get a satisfactory result in strumous joints it is necessary to remove the whole of the diseased synovial membrane. To carry out this principle, Professor Spence was in the habit, when operating on the elbow, of cutting away as much of the synovial membrane as he could get with scissors. In operating on the knee he adopted a more thorough procedure, which many members of this Society may remember. This was, after reflecting upwards a semilunar flap of skin, to make a circular sweep with his knife round, and at some distance from the patella, and thus remove in one mass that bone and a considerable portion of the thickened and diseased synovial membrane. This procedure strongly recommended itself to me, and I think that I have even improved on it. My method is as follows:—After reflecting a semilunar flap of skin upwards, well above the patella, I cut through the tendon of the extensor of the thigh a little above the patella, and also through the fibres of the vasti, internal and external. In this way the synovial membrane is exposed, and it is then quite easy, cutting in the cellular tissue covering it, to push up the muscular substance, and to draw down the thickened synovial membrane, which comes readily off the periosteum, and has then to be cut at its attachment round the articular surface of the femur. In this way four-fifths or thereabouts of the synovial membrane is removed in one mass with the patella imbedded in it. Those portions of the membrane that cover the ligaments are then removed by the use of the Lister sharp spoon. The ligaments should be scraped till they appear clean and white. They are then cut through to permit of complete flexion of the joint, and the operation is completed by the removal of a sufficient amount of bone.

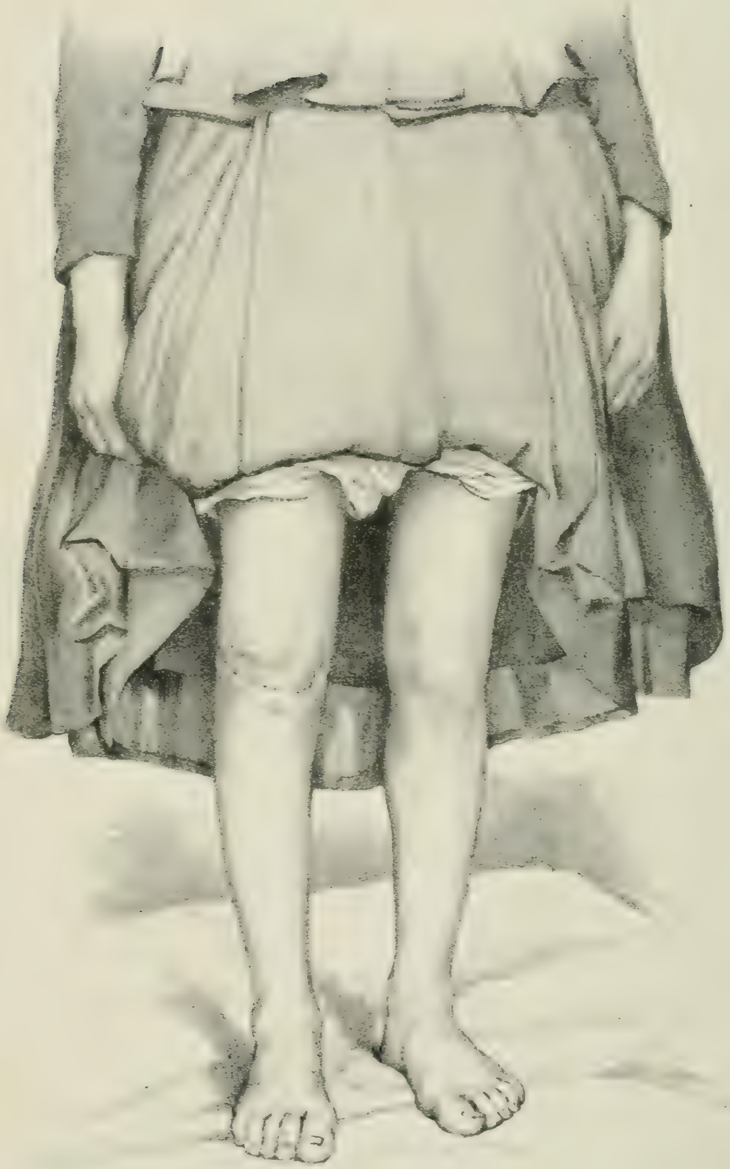
The above method of removing the whole of the synovial membrane may be somewhat tedious, but if systematically carried out is very thorough. The procedure is not difficult, but requires care. I have employed it for several years, and in the majority of the eighteen cases given in the following table. Of the minor details of the operation I will mention only two. The ligaments should be scraped clean before they are cut, for obviously the scraping will be easier when they are firm than when they are hanging loose. The other point is that the tendon of the extensor requires to be very carefully cut across, as it lies deeply imbedded in the diseased synovial membrane, and the latter is more easily removed if uninjured.

There is another procedure which I would recommend. In the old days, when a house-surgeon, I was much troubled, in dressing knee-excision cases, with the skin which lay in a loose, redundant, and troublesome manner in folds over the approximated bones. To remove this redundant skin I act as follows:—After making an incision from behind the one condyle to behind the other, across the front of the knee and at the level of the top of the tibia, I make another cut, commencing and terminating at the same points, but over the centre of the patella. These two incisions include an elliptical portion of skin, which is left attached, and the skin is dissected up over the synovial membrane from the upper cut as described above.

For this minor procedure I claim two advantages—1. Redundant and unnecessary skin is removed, which makes the limb look neater afterwards.[1] 2. The flap of skin is more easily dissected off the synovial membrane since it is about an inch shorter than the ordinary flap made by Watson, whose method of treatment I have adopted with the above modifications.

---

[1] *See* Plates.



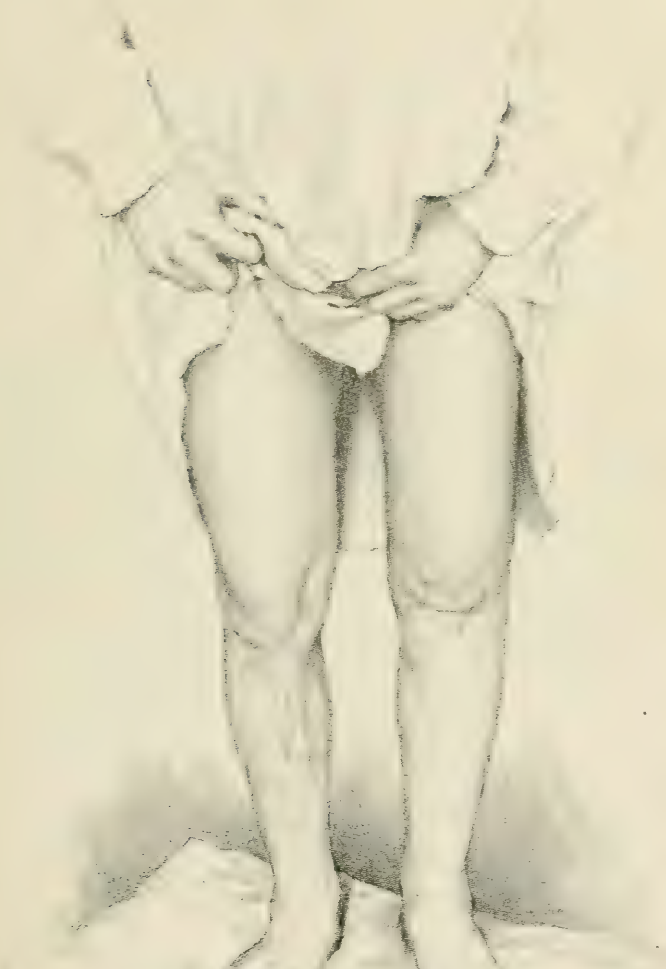


Table of all the Cases Operated on by A. G. Miller.

Name and Age.	Date of Operation.	Date of Discharge.	
1. D. M., m., æt. 23.	Jan. 19, 1885.	May 1, 1885.	102 days.
2. J. B., f., „ 25.	Mar. 19, 1886.	May 8, 1886.	50 „
3. J. A. S., f., „ 29.	June 7, „	Nov. 18, „	164 „
4. J. S., m., „ 17.	June 25, „	Aug. 20, „	56 „
5. J. M., f., „ 18.	Aug. 12, „	Amputation, Sept. 23; discharged cured, no date.	
6. K. B., f., „ 18.	Aug. 16, „	Nov. 17, 1886.	93 „
7. J. B., m., „ 23.	Aug. 27, „	Nov. 22, „	87 „
8. M. D., f., „ 13.	Mar. 8, 1887.	May 18, 1887.	71 „
9. W. C., m., „ 20.	Sept. 6, „	Amputation, Nov. 10; discharged cured, Dec. 8, 1887.	
10. G. B., m., „ 30.	Sept. 28, „	Nov. 30, 1887.	63 „
11. M. F., f., „ 22.	Oct. 27, „	Jan. 5, 1888.	69 „
12. J. M., m., „ 42.	Feb. 21, 1888.	Jan. 17, 1889.	330 „
13. J. C., f., „ 11.	April 20, „	May 7, 1888.	17 „
14. J. M., f., „ 12.	June 22, „	July 28, „	36 „
15. D. S., m., „ 7.	June 26, „	Sept. 1, „	67 days. Arthrectomy.
16. Mrs C., f., „ 30.	Dec. 11, „	Feb. 9, 1889.	60 days.
17. J. L., m., „ 19.	Feb. 19, 1889,	Amputated May 21, 1889.	
18. W. B., m., „ 19.	Feb. 26, „	April 4, 1889.	37 days.

Average duration of 15 cases in Hospital about 87 days = 12½ weeks.

Of these 18 cases, 15 were successful, 3 suffered amputation after an interval, none died. In 16 cases the excision was performed for strumous disease, in 1 for rheumatic ostitis, and in 1 for dislocation of four years' standing. In the three unsuccessful cases, amputation was recommended at the first.

In case 12, J. M., a second operation (gouging) was required for return of caries in the tibia.

The Plates represent the result in cases 14 and 18, from photographs taken by Mr J. L. Lorimer, medical student.

VII.—HOSPITAL EXPERIENCES.

Part I.

By John Chiene, Professor of Surgery, University of Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 15th May* 1889.)

At the termination of last winter session (1888–89) eleven years had elapsed since my appointment as Surgeon to the Edinburgh Royal Infirmary. The time had come when it appeared to me that benefit might be derived from a general retrospect of the cases which have been treated in the wards under my charge.

Beginning in the old Infirmary, where I had charge of two small wards for one year, and afterwards in the present Infirmary, first in Wards IV. and V. for three years, and then for seven years in Wards XIII. and XIV., there is much of interest to me personally to look back upon, and perhaps a tabulation of the experience gained may be of use to others.

I wish it to be distinctly understood that I place on record all the cases of which notes have been preserved in the Infirmary records. The great majority of these cases have been under my own care, but I have been ably assisted by the assistant-surgeons, Dr Macgillivray, the late Dr Bishop, Dr Cotterill, Mr Cathcart, and Mr Caird. I have therefore to place on record our combined work. It seems to me that over such an extended period this will be more valuable than if I had made it only a personal record. It will be the only true record of the routine work of a surgical ward as at present conducted in the Edinburgh Royal Infirmary. I am entirely responsible for what I have done, and for the way in which it has been done. Mr Wallace and Mr Robert Robertson have given me much assistance in the tabulation.

I find that notes have been preserved of 2658 cases; of these 1600 are operation cases, noted in the operation book. While I believe that the record of the operations is very accurate, I am of opinion that all the non-operation cases have, especially during the summer holidays, not been carefully noted. I ask it also to be remembered that the clerks and dressers who take the cases are second year students, and I therefore ask some allowance for their inexperience.

The classification which I have followed is the same as in the syllabus of the systematic lectures in the University; it has faults, but at the same time it has advantages. I am best acquainted with it; and although in a clinical retrospect I would have desired a purely regional classification, still I think on the whole it will be best to adhere to the classification in the syllabus. In its main features it is pathological and anatomical, looking to the organs and tissues. In some cases the basis is regional, as in head injuries, mammary, abdominal, and pelvic cases, which could not properly be included in the primary divisions of the classification.

The cases are made up as follows:—

Inflammation and its results,	340 cases,	24 deaths.
Injuries, . . .	563 "	29 "
Diseases of organs, .	690 "	23 "
Deformities, . .	113 "	1 "
Tumours, . . .	285 "	29 "
Diseases of regions, .	667 "	59 "
	2658 cases,	165 deaths.

The figures may vary slightly as regards the non-fatal cases. As regards the deaths, I believe that they are correct. They are given at present as a general indication of the work done. Their analysis will be work for the future.

In dealing with this mass of material, the first thing that attracted my attention was the deaths, and I purpose to-night directing attention to them. I do not consider them in relation to the successful cases; that subject will, if opportunity offers, occupy our attention on another occasion. I wish to consider these deaths in relation to their causes, and I wish from the experience of eleven years to draw some general conclusions which, while they will directly bear on my own practice and the dangers which I and those who may be associated with me will have to avoid in the future, may indirectly be of service to those who may hear me.

The question at the outset arises—What is the best method to adopt in classifying a series of deaths? The practical answer seems to be—Classify them under the different causes antagonistic to success; in other words, under the different conditions against which the surgeon has to contend in order to avoid death.

Given blood in a healthy condition, and given a healthy nervous system, our patients would not die. We have, then, blood lesions and nerve lesions. The blood lesions have reference to the channels which convey the blood, and to the blood itself. We know that the great cause of blood contamination is accumulation of organisms and their products in the blood-stream. Organismal causes are therefore closely allied to blood lesions.

The condition of exhaustion under which some deaths must be classified seems to me intermediate between organismal causes and nerve lesions. Some of these cases of exhaustion are apparently organismal, some are allied to nervous shock; and, further, the condition termed shock has an intermediate position between nerve lesions and blood lesions, because in surgical shock we have very often a mixed condition, partly nerve shock and partly shock from loss of blood. We have in this way a cycle. Beginning with blood lesions, we pass from them to organismal causes; from them we pass to exhaustion, from it to nerve lesions, and then we reach shock, which in its turn brings us back to blood lesions—the point where we began.

Under these five heads I find that

7½ per cent. of the deaths are due to blood lesions.
47½ „ „ „ organismal causes.
12 „ „ „ exhaustion.
14 „ „ „ nerve lesions.
19 „ „ „ shock.

Blood Lesions—13 deaths. 7½ per cent.

Seven cases of non-inflammatory gangrene, due to imperfect supply of blood. One of these cases was aged 25, and died of phthisis; the remainder were people above 65 years of age. In one case the arm was affected, in the remainder the foot.

One case of thrombosis following tapping of a cystic goitre. One case of fatty heart in a female with scirrhus tumours in both mammæ. One case of angina pectoris in a man who suffered from strangulated inguinal hernia, which was reduced *en bloc*, and on whom, after relieving the strangulation, the radical cure was performed. Three cases of hæmorrhage—two secondary; one an epithelioma of the neck, in whom the common carotid required to be ligatured; another, a scirrhus of the mamma, in which the glands in the axilla were removed, and in which the axillary artery gave way by ulceration. In both of these cases sepsis was the predisposing condition. One case of reactionary hæmorrhage following excision of a large vascular tumour on the posterior aspect of the thigh.

Organismal Causes—79 cases. 47½ per cent.

Acute organismal fever (sapræmia—septicæmia), .	20 cases.
Chronic organismal fever (pyæmia), . . .	13 "
Tuberculosis and tubercular meningitis, . .	16 "
Erysipelas, ...	7 "
Tetanus (traumatic), ...	2 "
Diphtheria, ...	2 "
Septic pneumonia, ...	3 "
Septic peritonitis, ...	4 "
Septic meningitis, ...	2 "
Surgical kidney, ...	3 "
Traumatic gangrene, ...	5 "
Carbuncle, ...	2 "

These cases may be grouped with advantage under three heads:

First, Those patients who suffered from organismal poisoning before admission—24 cases.

Second, Those patients in whom there was an ulcerated surface on admission—29 cases.

Third, Those patients in whom the skin was unbroken on admission—26 cases.

In the third class we have a series of cases which must be considered as distinct calamities—cases in which antiseptic precautions failed to keep the wound aseptic.

Exhaustion—19 cases. 12 per cent.

The predisposing causes to the deaths were as follows:—

Tumour infection, ...	6 cases.
Old age (above 80), ...	2 "
Youth (a case of double hare-lip in a weakly child), ...	1 "
Chronic bronchitis, ...	1 "

Œdema of lungs,	1 case.
Disease of heart,	1 „
Starvation,	2 „
Carbolic poisoning,	1 „
Persistent vomiting,	1 „
Double operation,	1 „
Loss of blood before admission,	2 „

Nerve Lesions—23 cases. 14 per cent.

Uræmia,	6 cases.
Inflammation of lungs in extensive burns,	2 „
Cardiac syncope,	3 „
Brain injury,	10 „
Rupture of spinal cord,	1 „
Locomotor ataxy,	1 „

Shock—31 cases. 19 per cent.

Accidents,	8 cases.
Burns,	4 „
Tumours,	4 „
(One case of retroperitoneal sarcoma; two cases of rectal tumour; one case of sarcoma of the synovial membrane of the knee-joint.)	
Excision of the knee-joint,	1 „
Radical cure of hernia in a child 16 months old,	1 „
Strangulated hernia,	5 „
(4 of which were moribund on admission.)	
Amputation at the hip-joint,	1 „
Gastrotomy,	2 „
Œsophagostomy,	1 „
Laparotomy,	1 „
Anæsthesia—2 of chloroform, 1 of ether,	3 „

One case of death during administration of chloroform was reported by Mr Silke in the *Edinburgh Medical Journal*, vol. xxv., p. 1007. The second case occurred in an epileptic in whom an attempt was made to reduce an old-standing dislocation of the shoulder.

The case in which ether was associated with death was a woman in whom the breast was excised for scirrhus. She died on the third day of acute bronchitis, in all probability due to the administration of ether.

From another point of view these 165 cases may be considered. They may be divided into three distinct groups:—

1. Cases in which disaster was to be expected—85 cases.
2. Cases in which there was a fair hope that disaster would not occur—43 cases.
3. Cases in which unexpected disaster occurred—37 cases.

Under the first head we may include 85 cases.

1. Under organismal causes—		
Blood-poisoning at time of admission,		24 cases.
2. Under exhaustion—		
Severe loss of blood before admission,		2 „
Tumour infection,		6 „
Starvation,		2 „
Old age,		2 „
Lung and heart affections,		3 „
3. Under shock—		
Severe accidents, death within 24 hours,		8 „
Severe burns,		4 „
Strangulated hernia, operation performed too late, bowel gangrenous,		3 „
4. Under bloodvessels—		
Senile gangrene,		7 „
Fatty heart,		1 „
Angina pectoris,		1 „
5. Under nerves—		
Severe injuries to brain,	9	10 „
and spinal cord,	1	
Locomotor ataxy,		1 „
Uræmia,		6 „
Cardiac syncope,		3 „
Inflammation of lungs (burns),		2 „
		85 cases.

Under the second head, 43 cases.

Broken skin at time of operation,	29 cases.
Laparotomy,	2 „
Tumours,	3 „
Gastrostomy,	2 „
Œsophagostomy,	1 „
Hernia,	3 „
Thrombosis,	1 „
Persistent vomiting,	1 „
Amputation at hip-joint,	1 „
	43 cases.

Under the third head, 37 cases.

These cases must be considered as calamities, the disasters of surgery which have taken place in my Hospital practice during eleven years.

Twenty-six cases of persons in whom the skin was unbroken, and in whom antiseptic management failed to prevent organismal poisoning. Two cases of secondary hæmorrhage due to sepsis.

One case of double hare-lips. One case of carbolic poisoning following excision of the hip-joint in a boy seven years old. One case of rupture of middle meningeal artery from fissured fracture of the skull in which trephining was not performed. One case of sarcoma of the synovial membrane of the knee-joint, which was supposed to be chronic rheumatic arthritis, and in which the operation was delayed. One case of excision of the knee-joint. One case of radical cure of inguinal hernia in a child 16 months old. Three cases associated with anæsthesia.

In looking back on the cases in which sepsis occurred, I am of opinion that the lesson to be learned by me is—more thorough purification of the skin before operating. It may also be noted that during eleven years many different antiseptics have been tried in the search for a reliable non-irritating antiseptic. The carbolic spray was used during the whole time except for one year, in which corrosive sublimate irrigation took its place.

The above record of the deaths which have taken place in the wards under my care is preliminary to a consideration of all the cases. I am now proceeding with a tabulation of those cases, and I hope that they may at a future time be laid before the Society.

CLINICAL REPORT OF CASES AT EDINBURGH ROYAL INFIRMARY

Under the care of Dr Claud Muirhead, during the Winter of 1888-89.

Notes of Cases of Disease of the Nervous System.

For these notes of the cases I am indebted to my excellent resident, Mr Alex. Gillespie, M.B. For the commentaries I am responsible.

The first case is that of W. A., male, aged 33, a floorcloth maker. He was admitted on 20th November 1888. History is unimportant; he never had syphilis, and is a steady man. His present illness began in February 1888 with a severe pain in the lumbar region, worse in bad weather; the pain was nearly constant, but in July began to come in paroxysms. In September the pain spread from the lumbar region down both legs to the feet, and in October the spasms of pain woke him up at night.

Condition on Admission.—Height, 5 ft. 9 in.; weight, 9 st. 6 lbs. He has lost weight recently. He complains of a pain as of a string tied round each calf, below which region there is analgesia, anæsthesia, and delayed sensation. His feet are numb and cold. There is pain on percussion over the lumbar spines. The electrical reactions of the leg muscles are normal. Patellar tendon reflex is exaggerated, the plantar lost, but the organic reflexes are normal. A bedsore is threatening over the sacrum. He can stand with his eyes closed, but there is great weakness of the muscles of his lower limbs. He is unable to raise his leg so as to place the foot upon a chair. To effect this movement he has to lift the leg to the desired position with his hands. His gait is not characteristic, but he gets over the ground slowly and with great difficulty; his feet are jerked out, and the heels come forcibly to the ground first, then the toes, with a double tap. The urine is normal.

Diagnosis.—Chronic inflammation of the spinal cord at the lower portion of the lumbar enlargement and first sacral nerve.

Treatment.—Paquelin's cautery applied firmly over the lower part of the vertebral column; this was repeated thrice. Cod-liver oil and 15-grain doses of Iodide of Potassium every six hours, with 1–24th grain of Perchloride of Mercury. The Iodide was increased to 20-grain doses after four days.

Progress.—His temperature was never above normal, and on the 1st of December the pain over the spines had disappeared, as had the girdle pains also. He could then walk with much more freedom, and raise his leg easily and spontaneously, and there was no delay in sensation. The plantar reflex had returned; the patellar was only slightly exaggerated. He made an uninterrupted recovery, and was dismissed on 11th December cured. He reported himself a month afterwards, when he was in perfect health.

The chief interest in this case rests in the rapidity with which the patient recovered his health. Nothing could be elicited, even on repeated questioning, as to the probable cause of the congestion, other than the usual indefinite and eminently unsatisfactory statement of "exposure to cold." Being one of those steady uncomplaining workmen, he tried to ignore his ailment, thinking that it would "wear away in time," and continued at his work till forced by sheer inability to give in and apply for aid. When he presented himself at the Infirmary he wore the expression of one who was a sufferer from continuous, rather than acute, pain. He was anxious looking, much emaciated, and anæmic. In two days after admission—in fact, immediately after the application of the cautery—the pain was greatly mitigated; and he enjoyed, for the first time for many months, a refreshing sleep. The Iodide of Potassium and Perchloride of Mercury were given for the purpose of causing absorption of the inflammatory products. In ten days after admission the pain had entirely disappeared, and the man gained daily in weight, and his countenance had lost the drawn, pinched, distressed, anxious look.

The second case worthy of note is that of D. S., aged 30, a soldier until three months ago, a farrier since. Has a history of syphilis, contracted nine years before. He was very intemperate until he left the army; he has been teetotal since. He was admitted on the 5th of November 1888; for three weeks before this had felt out of sorts only. On 3rd November he felt giddy, but went to his work; by twelve o'clock he had lost the power of his left arm and leg, but managed to walk home. For the last three months he has been troubled with headache over the left side of his head, and felt pain when he brushed his hair over this area.

Condition on Admission.—Temperature normal. Paresis of his left arm and leg; he cannot grasp anything with his left hand, nor raise his arm to his head. His left pupil reacts sluggishly to light, and his tongue, on protrusion, is directed to the left side. Left plantar reflex lost; left patellar exaggerated; organic reflexes normal. When walking he drags his left foot along the ground, the ball of the big toe catching on the ground. Urine normal. Mental faculties normal, with the exception of some sleepiness.

Diagnosis.— Gumma, most probably in the right internal capsule.

Treatment.—Grs. 30 of Iodide of Potassium every six hours; Faradaism to the affected muscles.

He left on 12th December, able to walk very well and raise his arm with ease above his head. His tongue came out quite straight. He had increased one stone in weight since admission.

This second case illustrates in a most effective manner the power and value of drugs when administered judiciously, and in sufficient dose. The hemiplegia was diagnosed to be due to the presence of a syphilitic gumma. Accordingly Iodide of Potassium was ordered in half-drachm doses, in water, every six hours. To some persons this may appear a large dose, possibly even heroic, and lead them to expect iodism as a sure result. But, as the happy termination of the case shows, these doses were not only tolerated without any accompanying iodism, but brought about very speedily the cure of the ailment. When the treatment seems to be slow in producing the expected result in similar cases, I do not hesitate to order the dose to be taken every four hours, or to increase it to 40 grains. By insisting upon the dose being thus taken systematically every six hours, I am able to prevent the supervention of the unpleasant iodism, and the addition of ten or twelve drops of tincture of belladonna I sometimes find of value to this end. Should the overpowering gravedo manifest itself in spite of these precautions, and occasionally it will, then this condition may be very quickly and effectively relieved by the introduction of a camels'-hair brush, previously dipped in a 4 per cent. solution of Cocaine, well up into the nostril, the patient being told at the same time to snuff up the fluid rather vigorously; or the Cocaine solution may be sprayed up the nostril. In two minutes the headache is relieved.

On 20th November a very interesting case was admitted. It was that of A. L., aged 33, a diver, complaining of loss of power in the legs. As a diver he, of course, has been accustomed to work under high atmospheric pressures when at great depths below the surface, but this he never felt the worse of until a few weeks ago. He denies ever having had syphilis, but has been intemperate. His present illness began three months before admission, one day after having made three dives of 150 feet. (At this depth the diver remains under water only for a quarter of an hour at a time, and does not descend more than four times in the day.) He sat down to wait his turn for his next descent, and when this arrived he found that his legs were numb and stiff. After removal of his diving dress, he perceived that he could only walk with great difficulty. He also found that he could not walk in the dark. This occurred in Trinidad. Finding himself in this crippled condition, he gave up his occupation, and came back to Scotland for treatment.

Condition on Admission.—Height, 5 feet 11¾ inches; weight, 16 stones 6 lbs.; is a very robust and muscular man. Temperature normal. Patient feels as if he had a tight hat pressing on his head just above his eyebrows. On walking he has a sensation as if there were two or three pairs of stockings between the soles of his feet and the ground; on sitting this passes off, and the soles of his feet become hyperæsthetic.

He has no lightning pains or tingling, but experiences great sense of weight in his legs. He becomes very giddy when he rises, which sensation is aggravated if he closes his eyes. There is no pain on percussing either the head or the spine. No girdle pain is complained of, except the one before mentioned above his eyebrows; special senses all normal. There is hyperæsthesia and hyperalgesia of the soles of his feet, but anæsthesia and analgesia of the inner parts of both legs; his sensibility to temperature is delayed in both feet. Electrical reaction, normal in his arms and in his thighs, but below the knees the muscles react very sluggishly to both currents, and it is with great difficulty that a contraction can be got with the faradic battery at all. Patellar reflex greatly exaggerated; no ankle clonus. Plantar reflex greatly increased; other skin reflexes doubtful, because of the large amount of adipose tissue. Organic reflexes normal. Fibrillary twitchings are common in the muscles of the legs. The muscular power of his arms is not impaired, while that of his legs is much less than normal. Pupillary reflex normal. He can only stand, and that with difficulty, when his feet are brought together, by keeping his eyes fixed on them. His gait is very peculiar. If he takes short steps only, he shuffles along; but if he attempts to take long steps he loses his equilibrium at once, and has to be supported. He raises his leg and puts it forward in an uncertain manner. Immediately he does so, the muscles of the calf are thrown into rigid tonic contraction, the heel being drawn completely up. For a few seconds he stands in this position, being totally unable to advance the leg. As the spasm yields, the leg is slowly brought forward, and the ball of the foot first comes to the ground, the toes being extended. As he bends forward and throws his weight on the advanced limb, the latter is thrown into rapid clonic contractions in the endeavour to bring the heel to the ground. There is also great difficulty in initiating movement with the other leg. If he falls he cannot raise himself, but has to creep to some object by which he can help himself up. If fatigued, there are fibrillary contractions over the outside of the thighs. The calf muscles, if sharply struck, contract spasmodically. Co-ordination good; intellect clear. Other systems normal.

Diagnosis.—Central myelitis.

Treatment.—He was given Ergot, and ordered rest as far as possible in the prone position; later Iodide of Potassium in 20-grain doses, along with $\frac{1}{12}$ gr. of Perchloride of Mercury every six hours.

On 4th December he has improved greatly. He is more sensitive to the electric currents, the muscles responding much more readily to the faradic current. He still experiences difficulty in taking long steps, but it does not seem so great as before. He is also much better able to keep his equilibrium. At Christmas time patient felt a craving for drink, and getting a pass on some pretext he took advantage of the opportunity to have a debauch for a whole week. After this he came back, and was readmitted on promise of amendment. He had been thrown back a good deal during his week's outing. He improved, but not so rapidly as was hoped; and went out, on 22nd January, immensely better, being greatly disappointed at the advice given him to give up his occupation, he himself believing that he was now quite fit to resume it.

At first this case appeared somewhat obscure by reason of the very multiplicity of the symptoms presented and the suddenness of its onset. It was, of course, impossible to ignore the occupation of the man or to dissociate it from his ailment. The abrupt advent of the disease, while actually engaged in the pursuit of his calling, pointed clearly to cause and effect. Then this extraordinary pressure to which he was subjected for considerable periods of time suggested a similarity to the accidents to spinal cord and brain which occasionally arise in the workers in caissons, and that the case might be regarded as an example of caisson disease. While again, the very peculiar gait, the amazing impediment to progression, by reason of the rigidity and tonic spasm into which the calf-muscles were thrown by the effort (he spent eight hours in walking from Leith to the Hospital, a distance of about three miles), gave rise to the thought that here was presented a case of that rare disorder, "Thomsen's disease."

Further consideration of all the symptoms led me to believe that the last idea was clearly not correct. While the very immediate connexion between the occupation and the onset of the disease seemed to point to diving to this great depth as the cause of the disorder. Again, the abruptness of its development pointed to some vascular lesion. We know that in the case of caisson-workers the blood is driven, by the enormously increased atmospheric pressure to which they are subjected, from the surface into the bones, spinal column, cranium, etc., and that thereby peculiar symptoms are experienced. According to Dr A. H. Smith the principal pathological effects are—"1, Rupture of the drum membrane and inflammation of the middle ear; 2, neuralgic pains, which may come gradually or suddenly, in the extremities or in any part of the body; 3, epigastric pain and vomiting; 4, paralysis, both sensory and motor, most frequently of the lower limbs; 5, the cerebral symptoms are, headache, dizziness, double vision, incoherency of speech, and sometimes unconsciousness. Death occurs only in cases which are severe from the first, and are marked by symptoms of serous or sanguineous effusion about the brain or cord. The constant lesion in fatal cases of caisson disease is congestion of the brain or cord. It is the *removal* of the pressure and not the pressure itself that is the immediate cause of the seizure." Not very different were the symptoms discovered by our patient shortly after his return to land from the great depths, and while waiting to make a renewed descent. The paresis of the lower limbs (there never was true paralysis), both sensory and motor, came on suddenly, and only after removal of the great pressure to which the body had been subjected. My interpretation of the case was that, in consequence of the enormous pressure, the blood had been driven from the surface into the vessels of the spinal canal on this occasion with unusual force; or, perhaps, in consequence of the too free indulgence in alcohol, the vaso-motor control over the vessels had become so relaxed that the vessels, yielding to the pressure, became unusually largely dilated, and when the external pressure was removed they failed to respond to the altered condition and declined to resume their natural calibre. The result of this would probably be some outpouring of serum and congestion of that portion of the cord situated at the lower lumbar and upper sacral region, possibly even capillary hæmorrhages and inflammatory exudation in the central portion of the gray matter,—in fact, a condition of central myelitis.

In this view of the case, the somewhat anomalous and paradoxical symptoms may be explained. The pressure exerted on the posterior columns would readily account for the paræsthesiæ, hyperæsthesia, analgesia, etc., the paresis, fibrillary twitching, boils, and slight atrophy of muscles by the pressure exerted on the anterior columns and irritation of the motor nerve-cells; while the enormously exaggerated reflexes might probably be accounted for by this same pressure interfering with the perfect nutrition of the gray matter, and thus setting up increased excitability which was manifested in this manner. I omitted to mention that though ankle-clonus could not be developed, I believed this was simply due to the tonic spasm which was set up in the calf-muscles in the attempt to initiate the ankle-clonus.

The treatment was at first by rest and Ergot, later by Iodide of Potassium and Perchloride of Mercury. Improvement was slow. On Christmas Day he walked four miles like other men, as he said, and considered himself the happiest man in Edinburgh!

Part Second.

REVIEWS.

Diseases of the Eye. By GEORGE A. BERRY, M.B., F.R.C.S.E.
Edinburgh and London: Young J. Pentland: 1889.

THIS handsome 8vo of 650 pp. supplies the second volume to Pentland's Medical Series, so successfully inaugurated by the appearance of Dr Allan Jamieson's work on *Diseases of the Skin.*

Within the last three or four years so many text-books of ophthalmology have been provided for English readers as new books, new editions, or translations, that it might fairly be asked whether there was room for another.

Mr Berry and his publisher seem to have determined to solve that problem in the confident spirit of the youth who entered an overcrowded profession, declaring that there was "always room at the top."

We have no hesitation in saying that in many respects this is the most advanced text-book which has yet appeared in this country in the department of Medicine of which it treats. The work is divided into three sections.

Section I. commences with the enumeration and description of the diseases which affect the globe of the eye, the optic nerve, and the orbit, under the usual anatomical headings; but, in addition, the important questions of foreign bodies in the eye, sympathetic ophthalmitis, and intra-ocular tumours, are discussed in separate chapters.

The clinical descriptions are clear, and for the most part well proportioned to the practical importance of the subjects treated. We note, however, a tendency to undue brevity here and there, which we fear may detract from the value of this part of the book as a work of reference.

The letterpress is illustrated by a large number of coloured plates, representing the morbid appearances of both external and internal diseases of the eye. These are reproduced from original drawings by Dr J. Tatham Thomson, and though not free from faults in outline and colour, are nearly all really instructive. Several are excellent, and Figs. 13, 17, and 21, representing lymphangiectasis of the conjunctiva, staphyloma of the cornea, and dendriform keratitis, are particularly truthful. The last mentioned is of special interest, since it presents a type of corneal ulceration, probably of mycotic origin, first described by Hansen Grut, and as yet but little known in this country.

One of the best chapters is that on Glaucoma. The various forms of this mysterious affection, and the symptoms and signs upon which the diagnosis rests, are all carefully passed in review;

in each case their causation is explained or discussed, plates illustrative of the typical external and ophthalmoscopic appearances, as well as a characteristic chart of the field of vision, are judiciously introduced, and the chapter closes with a thoughtful critique and commentary on the various views which have been advanced as to the etiology of the disease and the nature of the beneficent influence of an iridectomy. The author is unable to solve the enigma in either direction, but whoso next attempts a solution will do well to read what he has written.

Not less interesting is the chapter on Amblyopia, Amaurosis, and other Anomalies of Vision.

Section II. treats of Errors of Refraction and Accommodation, Affections of the Oculo-motor Muscles, and the Examination of the Eye.

The first of these chapters is in itself a treatise of the highest merit. Certainly too elaborate, we should have said, for ordinary students, had not the less essential parts been relegated to a background of small type, but calculated to afford much mental satisfaction to "students of ophthalmology" (and for these the book is intended) who are not to be daunted by a little algebra and trigonometry. The optical diagrams are abundant and beautifully executed.

As examples of the accurate scientific exposition which characterizes this section, we may instance fig. 89, from which is deduced a lucid explanation of the circumstances under which a portion of the fundus becomes visible to an observer at some distance from the patient's eye, in hypermetropia and myopia, but is undefined in emmetropia; fig. 90, which explains the alterations in the size of the ophthalmoscopic image obtained by the indirect method, when the lens is gradually withdrawn from the patient's eye, in hypermetropia and myopia respectively; and the paragraphs in the chapter on the Examination of the Eye illustrating ophthalmoscopic magnification and the amount of the fundus visible by the indirect and direct methods, both of these being worked out with complete mathematical proof.

The chapter on the Affections of the Oculo-motor Muscles occupies 50 pp. of delightful reading.

Mr Berry states in his preface that he eschews pathological anatomy, except where it has "a direct bearing on the treatment to be adopted," because he believes that "any exhaustive discussion of the pathological anatomy of eye diseases tends to divert attention from their clinical aspects," and he adheres faithfully to this principle throughout.

We are not altogether prepared to agree with him in this matter. We call to mind a certain fable about the body and its members. To acknowledge merit only when brilliant service brings it into prominence is certainly bad policy. Morbid anatomy has always appeared to us a useful helper, not a hinderer of clinical study, and while we admit that it has not hitherto fulfilled the expectations of its youth, we believe that too much has been expected of it. That it is yet destined to play an important part in liberating therapeutics from the thraldom of empiricism is becoming daily more manifest.

The concluding Section is devoted to Operations. The descriptions are concise and thoroughly practical.

There is a good general index, as well as an index of authors referred to. The value of the latter would have been far greater had the authors' names been accompanied by references to the publications from which their views have been extracted, since no such information is given in the text.

Like its predecessor, this book is printed in clear type, and, considering its size, has evidently been carefully edited.

We have noted the following errata:—On p. 85, in the eleventh line from the top, for "*a*serine" read "*e*serine." On p. 229, in the fourth line from the bottom, for "produce" read "produces." On p. 332, in the sixth line from the top, insert "be" before "due." On p. 506, in the fourth line from the top, insert a comma after "internal" and after "superior."

In discussing the treatment of herpes zoster frontalis, to state that "when the neuralgia is severe, subcutaneous injections of morphia are useful, and *appear to favourably influence the tendency to its continuance*," is not the simplest way of conveying what is desired to the mind of a student. "*In*transparency of the cornea" is an expression which we trust will not become general. But blemishes of this kind are rare.

Mr Berry has succeeded usually in conveying his meaning in direct and vigorous language, and the whole work is one which must add fresh lustre to his already distinguished reputation as a scientific surgeon.

Ueber das Verhalten der Tuberkelbacillen im thierischen Organismus unter dem Einfluss entwickelungshemmender Stoffe. Von Dr GEORG CORNET. (*The Behaviour of the Tubercle Bacillus in Animal Bodies under the influence of Agents which restrain its Growth.* By Dr GEORG CORNET.)

Die Verbreitung der Tuberkelbacillen ausserhalb des Körpers. Von Dr GEORG CORNET. (*The Dissemination of the Tubercle Bacillus outside the Body.* By Dr GEORG CORNET.)

THESE two pamphlets are reprinted from the fifth volume of the *Zeitsch. f. Hygiene,* 1888, and the experimentation detailed in them was made in the Hygienic Institute of the University of Berlin. After Koch's discovery that the tubercle bacillus was the essence and only cause of tuberculosis, physicians were animated with the hope that they would succeed, as was easily done in pure cultivations, in either killing it off in the organism also, or at least make it harmless. Arsenals of antiseptics were ransacked, the bacillus cultivated and satisfactorily slain by such means outside the body, and yet not one of them can cause its destruction in the body. All putative successes announced hitherto have sooner or later proved themselves illusory.

We possess in quinine and in mercury two sovereign remedies against two diseases which are possibly of bacterial nature, and from a scientific standpoint we are justified in continuing the search for means to destroy the organism of tuberculosis, although our *pia desideria* have not yet been crowned with success. Fortunately or unfortunately "Man is not a test-tube," as Volkmann put it; and the substance which certainly poisons an organism in the culture-glass may, when introduced into the animal body, undergo a series of combinations and metamorphoses which shall exercise a directly opposite effect on the same bacterium which has gained admittance to the body.

Dr Cornet's experiments were made on animals easily susceptible to tuberculosis, and the antidote, successful enough in the test-tube, was given in doses as large as possible without actually killing the animal with the remedy. He also employed a novel and ingenious method of introducing the remedy, namely, using a stomach-tube, and thus having an absolute guarantee that the full dose was given.

He experimented on 112 animals, namely, 102 guinea-pigs and 10 rabbits. The substances used to counteract the inoculated or inhalation tuberculosis were—1, tannin; 2, acetate of lead; 3, garlic; 4, pinguin (alantol and alantol acid); 5, sulphuretted hydrogen; 6, menthol; 7, corrosive sublimate; 8, creolin; 9, creosote.

Dr Cornet flooded the tissues and cells of the animals experimented on with those substances, and their success was *nil*. Only for creosote has he a good word: where expectoration is profuse and rich in bacilli the phthisis may take a favourable turn under its use in so far as the secretion may lessen. We have never been able to see it of the slightest service in such cases in the Incurable Hospital, although fair trial has been given to it, and the highly-recommended German preparations have been used.

The author also endeavoured to ascertain whether elevated climate would have any beneficial effect on his infected animals, so a number of them were sent to Davos, and died in the usual way at the usual time after infection, and showed the same pathological changes at the sectio.

The second of the above-mentioned Cornet reprints concerns itself with the spread of the tubercular virus outside the body. As the commonest form of tuberculosis is lung phthisis, the author assumes that there must be a direct infection of those organs, and that the air must of necessity be the bearer of the virus, and that the dust deposited from it would be a more likely place to find the bacillus than the air itself, as only relatively small quantities of it can be examined at a time. He therefore collected the dust deposited in a natural way on articles of furniture in the wards and rooms where the phthisical patients lived, and took excessive care to select only such spots for his collection that could not by any possibility be directly contaminated by spitting or coughing, or by contact of spit-dirtied fingers, handkerchiefs, vessels, etc. He used sterilized spatulæ or small sponges to remove the dust, mixed it in sterilized bouillon, and injected into the peritoneal cavity of the animals experimented upon. By so doing he avoided missing the bacilli microscopically, as material which so examined has shown no bacilli, has yet proved infective because containing their spores, which cannot yet be microscopically demonstrated.

He selected the peritoneal cavity for injection because it can take up greater quantities of the inoculating material, and an abdominal tuberculosis gave such a characteristic model of the disease; he used freshly purchased animals, so as to avoid the source of error of a spontaneously existing tuberculosis, which, moreover, generally is a lung one.

Taken with these precautions, the dust of hospitals, asylums, prisons, workshops, and private dwellings, where consumptives lived, gave positive results in the animals injected so often as to place it beyond question that the victims of phthisis can contaminate their surroundings, and occasion not only the possibility but probability (in a remoter degree) of infection.

The author considers it to be an incontrovertible fact, that tubercle bacilli never develop save in human or bestial bodies, and that their growth naturally outside is impossible. Such organisms only afford the necessary conditions of temperature, etc. It therefore follows that all tubercle bacilli must have once lived in such bodies, and the question is, How have they left, and by what medium? He believes the chief source of infection for man is man himself, and most frequently the consumptive, and that by his dried and pulverized sputa, and not his breath, the bacilli are spread in the air.

In the chapter on prophylaxis it is shown how the physician may innocently spread the disease by using imperfectly disinfected or cleaned instruments, such as the laryngeal mirror, and the fortunes of an old feather bed which the author bought are related. He smeared a few marked feathers with tubercular sputum, dried them, reintroduced them into the bed, and sent it to six different institutions to be purified and disinfected. He easily picked out the soiled feathers, washed them in sterilized water, and injected the washings into the peritoneal cavity of guinea-pigs, with the result that the virus was still operative.

Both booklets, particularly the latter one, attest the wonderful industry, patience, precision, and care to eliminate sources of error from the experiments (upwards of 400 in number) taken by the author, and his conclusion seems a perfectly just and reasonable one, that phthisis is a highly infective disease, a belief held by many people whom we consider ignorant, and taught to them by no science but shrewd common sense and experience.

Leprosy, an Imperial Danger. By H. P. WRIGHT, M.A., Rector of Greatham, Hants; Chaplain to the Forces, First Class, etc. London: J. & A. Churchill: 1889.

ONE cannot rise from the perusal of this interesting volume without being satisfied that Archdeacon Wright has made good his propositions that leprosy is contagious, and that there is risk of its becoming once more epidemic in Europe. While there is yet much to learn as to the nature of leprosy, certain facts seem proved, and among those are the following:—1. It is communicable by contact and by vaccination, possibly through the medium of parasites, as mosquitoes or the Acarus scabiei. 2. The period of incubation is probably about three years, is certainly protracted. 3. The sole available method of stamping out the disease is segregation, not necessarily rigid confinement in special hospitals. 4. As this comes to be carried out in other countries, early cases may resort to Great Britain, just as such are now known to flee from the Sandwich Islands to America to avoid imprisonment in Molokai.

According to Leloir, there were recently eighty cases in or near Paris, yet the results of the inquiry recently made at the instance of the *British Medical Journal* show "that there is no reason to believe that we have more lepers amongst us now than there have been at any previous time for many years. Leprosy has as yet got no foothold in Great Britain." That it is possible for the disease if introduced to spread in such a temperate climate as our own is proved by past history, and is supported by a statement made by Mr Wright, "that in Russia since 1877 forty-nine cases of leprosy have been treated in the St Petersburg Hospital, half of the patients being natives of the city."

Mr Wright's work is based on the written opinions of most of the chief authorities on the subject, many communicated directly to him, and must be regarded as a valuable contribution to the literature of leprosy. Were it to do nothing more, it will render public vaccinators alive to the risk of arm to arm vaccination from children who have in any way been brought into contact with lepers, such as those born abroad in countries where the disease is endemic, or even perhaps in the case of children whose parents or personal attendants have lived in such districts. Archdeacon Wright has done well to sound the note of alarm, though the danger may be remote.

Diabetes; its Causes and Permanent Cure from the Standpoint of Experience and Scientific Investigation. By EMIL SCHNÉE, M.D., Consulting Physician at Carlsbad and Monaco. Translated from the German by R. L. TAFEL, A.M., Ph.D. London: H. K. Lewis: 1889.

IT is fortunately not often that the reviewer is called upon to notice such productions as the present. It is one of the most illogical, unscientific, and badly-written works we remember to have seen. In fact, from beginning to end it is little more than a piece of self-glorification imperfectly concealed under an assumption of scientific knowledge, which in spite of the many authorities quoted is obviously not possessed. One sentence is sufficient to show the character of the author's scientific attainments:—

"For a time this stored up material during an insufficient supply of food is able to act as a medium capable of forming in the tissues *the carbonic acid necessary for the maintenance of life.*" The italics are ours.

The theses which Dr Schnée advances are—First, "That diabetes is the result of a hereditary predisposition, namely, a hereditary lues." This he does not attempt to prove by any arguments or statistics, but by frequent reiteration of the statement—a method which will hardly convince even the most unscientific physician.

His second contention—in large capitals—is that "Diabetes is curable." Again frequent assertion is the method of proof adopted. According to the only statistics brought forward, out of 71 cases treated between 1881 and 1884, 53 were cured—74 per cent.—so that, in spite of the curability of the disease, the author by the wonderful method he has discovered yet fails to cure 25 per cent. of his patients. We are not convinced, and we still believe with the majority of physicians that some cases of diabetes are curable, while others are not, and we do not think that Dr Schnée's system is worthy of any more attention than most of the other *methods of cure* which are every year showered upon the profession only to be forgotten.

The one useful part of the book is a table of references to the works of various investigators on this most difficult subject. It is by no means a full bibliography, but nevertheless it will be found useful by those studying diabetes.

Notes on Physiology. By HENRY ASHBY, M.D., M.R.C.P. Fifth Edition. London: Longmans, Green, & Co.: 1889.

IT is with regret that we have to notice the appearance of another edition of this most typical of cram books. Its success is a very grave censure on our examinations and examining boards. That a student should be able to pass even the lowest standard examination on such a book as this is greatly to be deplored. We are almost inclined to say that a man would be better without any physiological knowledge than with the knowledge which is to be attained from the perusal of such a work.

We object to it, not so much because it is full of inaccuracies—though certainly these are abundant enough—but because it is totally destitute of the true spirit of the science, containing merely a badly arranged and disconnected series of statements. Surely the student is required to study physiology, not merely to cram himself with a set of more or less useful facts, but to prepare his mind by the study of the methods employed in the investigation of the normal functions of the body, for his after-work in the study of these functions in an abnormal state. And surely it is the duty of examining boards to ascertain that this training has been satisfactorily accomplished. As long as this book remains a favourite with students we must conclude that this duty is not efficiently performed.

The Student's Text-Book of the Practice of Medicine. By ANGEL MONEY, M.D. Lond., Assistant-Physician to University College Hospital, etc. London: H. K. Lewis: 1889.

THE author in his introduction states that he has attempted to produce a very concise book of modern medicine, and he goes on to remark, apparently in a spirit of irony, that he hopes it may prove of use, amongst others, "to practitioners who have no time or inclination to peruse treatises." We regret that we cannot congratulate the author upon any degree of success in his attempt. The work, although small, is not concise. It has evidently been written in a hurry, which is shown on almost every page by specimens of slipshod English. Take, for example, the following paragraph from p. 155, on Angina Pectoris:—

" *Treatment.*—Nitrite of amyl and nitroglycerine are the best remedies, they are antispasmodic, and though they dilate the arterioles, it does not prove that spasm of the arterioles is the pathology of all cases, for they may relieve spasm of the heart, Rest and careful regulation of the diet should be enjoined."

Such a paragraph as this might be condoned were it part of a written answer to an examination paper, produced under the trying circumstances inseparable from such an ordeal, but as a paragraph from a text-book of Medicine it cannot be in any way excused. The curious blunder by which *euthanasia* comes to be held as synonymous with *the feeling of health*, and *dysthanasia* equivalent to *the feeling of ill-health*, has a retrospective interest in regard to the extent of the preliminary examination.

What we have said of the style of the book might be applied to the contents, as the subjects seem to have been hastily written without due preparation or consideration. Such little books seem to be in great demand, and probably a second edition of this one may be wanted before long. It is to be hoped that the author will rewrite it if such is the case.

Bernd's Physician's Registers. Henry Bernd & Co.: St Louis, Mo.

ONE of the troubles of the medical practitioner's life is the keeping of books. Anything that minimizes his difficulties in this respect is therefore to be welcomed. One of the most novel and successful attempts in this direction is *Bernd's Physician's Register*, in which the visiting list and ledger are combined. By a very neat arrangement the patient's name requires to be written but once for the year, while the visit for every day and to each member of the family is noted separately. These visits are summed up at the end of each month, and accounts may be rendered monthly, quarterly, half-yearly, or yearly as desired. The register is in two forms: one for the pocket, the other for the office or surgery. Of the latter there are three sizes, for 500, 800, and 1200 accounts respectively, and of the former two for 300 and 400 each. We have examples of both forms before us. They are excellent specimens of American stationery, and, so far as we can judge from the use to which we have begun to put one of them, they reduce the labour of book-keeping to its minimum. In addition to the visiting list there are several blank pages for various memoranda. If we might suggest any alteration, it would be that they could be had either indexed all the way through as at present, or with a separate index at the end of the book, according to the taste of the purchaser. It seems to us, however, that they only require to be known in this country in order to be appreciated. They are to be had from Messrs M'Lachlan & Stewart, Edinburgh.

The Medical Annual and Practitioner's Index, 1889. Bristol: John Wright & Co.

THE annual is now in its seventh year of issue, and may be said to have fully established itself. It is as great a help as ever to the busy practitioner. Two important articles by Stretch Dowse on mechano-therapeutics, and by Kenneth Millican on electro-therapeutics, are included in the section on New Remedies. Three American physicians have been added to the contributors' staff. A very meagre section is that on New Inventions, the resemblance of which to the advertisement pages of the book strikes us as rendering one or other part unnecessary.

Bournemouth as a Health Resort. By A. KINSEY MORGAN, M.R.C.S. London: Simpkin, Marshall, & Co.: 1889.

THIS is a little book by the Medical Officer of Health for Bournemouth, in which he endeavours to show the superior advantages of the Evergreen Valley with its sea-bathing, Mont Dore, and Pine cures to those claimed for many of the popular health resorts abroad. Though padded out a little, particularly with sanitary wisdom from Mr Teale, it is well worthy of the attention of the profession.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXVIII.—MEETING VI.

Wednesday, 20th February 1889.—Dr SMITH, *President, in the Chair.*

(*Continued from page* 1143, *vol.* xxxiv.)

Mr Taylor Innes, advocate, said, in the few words which he would say he would limit himself to the subject which Dr Ritchie had been good enough to draw his attention to. The liberty of the individual was the obstacle which might be supposed to stand in the way, and which had to be attended to. He had a strong belief on the general question that this ought to be no difficulty in the way of passing some such measure as they were now contemplating—a mere measure for the compulsory detention of dipsomaniacs—nor did he believe that public opinion would find that it is a serious difficulty. He observed that Dr Yellowlees had but little hope with regard to this matter, and undoubtedly there had been difficulties in the past. He thought that now, provided that the matter were carefully looked at in detail, the public and the House of Commons could be got to face a new measure and to pass it without serious difficulty. In America the liberty of the subject was put first: yet such legislation existed. And in Britain he thought that the state of feeling with regard to the mischief of drunkenness, and this class of cases in particular, was such that it should not be looked on as hopeless to get such a matter approved of by the community. Of course that was only upon the understanding that all precautions were taken, in the particular Bill, so as to satisfy the public conscience that there was no restraint of the liberty of the subject except what was necessary. Mr Morton had done wisely in publishing the Bill, and in submitting it to bodies like the Medico-Chirurgical Society, in order that discussions might take place, and that legislation might be prepared for. With regard to the measure he had a word to say. The main point in which it (the draft Bill) differed from Dr Cameron's Act lay in its giving power to a man's family, as well as to himself, to propose detention. He thought the intention of this was good, but he thought great care would require to be taken, and he was not sure that there were no mistakes. He found, for example, that no intimation was proposed to be made to the man when one of his family, with the consent of a doctor and one or two friends, wished to have him detained. He did not think they would get the public to consent to that, and he did not think they should. He said they ought to give full and fair notice to the man that they intended to detain him. The Bill spoke of "families" and "relatives." These were words which would require definitions, and other clauses should be amended. Twelve months was the time stated for the detention. That was a hard and fast line. Was there, he asked, any medical reason why twelve months should be the time? Some cases of dipsomania would require more: twelve months in many cases would be of no use. He should think there would be cases, again, of men in business who would find it difficult to get away for more than, possibly, six months. He thought, however, that was entirely a medical question, and it should be in the medical certificate which was to be granted. The medical certificate should state what amount of time was necessary in the medical view for recovery. Lawyers in this would defer to the medical men's opinion, and the relatives also would be willing to follow their advice. His suggestion was that there was no necessity for having a hard and fast line. There was no hard and fast line in the Habitual Drunkards Act, whose three, six, nine, or twelve months might, he thought, be extended. That led him to the following. He observed that it was proposed to repeal the Habitual Drunkards Act and to put this new Bill in its place. He thought they should consider well before proposing or pressing that. There was an elaborate definition given in this Bill of the class of persons whom it was intended to deal with, but it struck one who was not a medical man as an artificial and difficult definition. They would find it difficult to push it through the Commons, who had already passed a much wider definition embodied in the title of the "Habitual Drunkards Act." He did not think they should attempt to pass a wholly new Bill with a wholly new definition. He would suggest that it would be well worth considering whether, retaining the Habitual Drunkards Act, they might not add to it a clause giving power to the man's relatives, as well as to the man himself, to propose and bring him forward for detention. He thought they would find it much less difficult to persuade the House of Commons to add to the Bill they had already passed, than to pass a Bill with such an elaborate preamble and such a definition. He was glad that it was not proposed to press the assessment clauses, but in the meantime only to shut up self-supporting people. It was, therefore, only a sort of tentative measure, and he thought if this succeeded, they would have a much larger problem to face with regard to the working class, the criminal, and the pauper. However, it was well worth their while trying an experiment. But that was another reason why he thought their experiment might be tried in the way of adding to the Habitual Drunkards Act, and why they should not try to pass an elaborate definition and a new Bill. In any event, he said, they were much indebted to Mr Morton for urging the question.

Dr Batty Tuke said he felt difficulty in rising to speak on the question, because, not being so hopeful as the speakers who had preceded him as to the effect of legislation, it might appear that he wished to throw cold water on the whole subject. That was not so: no one knew better than he the misery caused by habitual drunkenness, and no one was more desirous than he to find some means of mitigating it. But he was most anxious that the Society should look the many inherent difficulties of the whole question straight in the face, and that it should look ahead and endeavour to foresee what would be the outcome of the working of a legislative enactment such as that then under consideration. Dr Peddie might be congratulated on stating the position regarding drunkenness in a scientific manner, inasmuch as he had pointed out that drinking was one of the manifestations of many forms of insanity. But he (Dr Tuke) was not prepared to admit unreservedly that insane drinking or dipsomania was synonymous with habitual drinking, or that habitual drinking was necessarily disease. He drew a distinct line between vicious drinking and insane drinking, and it was only in the latter condition, insane drinking, that he thought they had any right to ask for legislative measure of control—control meaning seclusion from society. The first difficulty arose out of the question, Who should be the active agent in procuring such control? Was it to be the doctor who was to decide between vice and insanity? He thought that was too much responsibility to place on the shoulders of any one man. Where there was such an open question between the physician and the casuist, he thought it was one that must be determined by more than a single individual. Was it to be a member of the family of the inebriate? This would be to adopt a *lettre de cachet* system open to gross abuse. His opinion was that any such matter must be determined by a court. His old friend and master Dr Skae had long ago suggested as a proper court to determine between vice and insanity, one consisting of a sheriff of a county, assisted by a medical assessor, a representative of the inebriate, and a representative of the family. He could not think that the public would ever consent to the liberty of any individual being curtailed by a less responsible tribunal. He thought that this court should be invested with considerable powers. If it found that the inebriate was an insane drinker, it should have not only the power of deciding what length of time the patient should be subjected to seclusion, but should also be the only agency by which the length of what was practically a sentence should be curtailed. One of the great faults of all the Habitual Drinkers Acts had been the great ease by which an inebriate could obtain release. In Mr Morton's Bill he saw it was proposed that a Commissioner in Lunacy should have this power. He thought that this should not rest with a single individual, and that the court, however constituted, should be the only agent by which its decision could be modified. He thought that whatever representation the medical profession made to the

Legislature or the public, it should make a very full representation of the medical facts; and one fact especially should be put prominently forward in order to avoid anything like reflections in the future. That fact was that insane drinking cannot be cured in a short period of time. Every physician who had any experience of the condition knew that it was useless to speak of six months, or even of a year, as the period of seclusion. It was a well-established fact that, in a very large proportion of cases, it took at least two years to give the patient a chance of recovery, which, he presumed, was the main object of Mr Morton's Bill. What he wanted to impress upon the Society was, that in any representation it may make as a medical body, this important fact should be put prominently forward. He would like to ask the meeting to consider whether it thought the public would ever consent to allow a man to be incarcerated for at least two years for drinking, on such slight authority as was suggested in the Bill under consideration? In his opinion it was extremely doubtful whether such powers would ever be granted to any court, for, in point of fact, a man might be incarcerated for life. They had been told that only 30 per cent. of cases submitted to treatment in homes recovered. What, then, was to be done with the large balance? Were they to be committed and recommitted? If not, what was the good of the Bill so far as 70 per cent. of those to whom it would apply was concerned? This would be a great stumbling-block in the face of any legislation, and it was one which he would urge on the Society to consider carefully before they passed any resolution based on Mr Morton's Bill. He would remind them of their experience of public measures which had been passed by the Legislature in consequence of medical representations. He referred to the Contagious Diseases Acts, which were adopted on much more definite data than those of any Habitual Drinkers Act. John Bull was touched in his pocket, a very tender point. Repressive Acts were passed, were set in action, worked beneficially, and yet were swept away on purely sentimental grounds a few years after their enactment. What, he asked, were they to expect were analogous measures adopted with regard to drinking, which a large section of the community regarded as no very serious evil, and which would be hard to convince that it was the outcome of insanity? Then, again, if an Act was passed, what was to be done with the inebriates? To what institutions were they to be consigned? It was evident from the public prints that private enterprise had for the most part failed. In England it had met with but slight success. He was glad to see that the clauses in Mr Morton's Bill, which suggested that the poor ratepayer was to be assessed in order to provide a home for the well-to-do inebriate, had been withdrawn; but he could not help pointing out that such a suggestion, emanating from such a man as Mr Morton, was strong evidence of the inherent weakness of the scheme. He thought if any Act was ever passed, insane inebriates should be confined in special departments of public asylums. They were the proper institutions to undertake their care, and there would be less chance of abuses arising out of such an Act if worked through the instrumentality of public institutions. But supposing they had obtained all this machinery, to whom were they going to apply it? He very much feared that its action could only be brought to bear on the ultimate stages of alcoholism. Could any one suggest a plan by which it might be made to apply to the earlier stage, the only period in which it could be materially useful in a curative point of view? And then from the preamble of the Bill, and from what had dropped from previous speakers, the Bill was only to apply to a certain section of inebriates—those who by their evil habits were liable to ruin, or compromise the comfort of their families. Now, how would this apply? It would apply to the man of independent means, who might squander his capital; but how would it act in the case of a professional man who, by being shut up, would have his business ruined for him? Then, again, if the definition of an inebriate such as was set forth in the Bill was adopted, it would apply to hundreds and thousands of cases, and homes without end would have to be constructed. Further, why should ruin to families through drink be alone provided for? If it was a main object of the Bill to provide against family ruin and scandal, why not extend its action to other vices, such as lechery and gambling? He could not believe for a moment that any Bill could ever pass Parliament warranting the incarceration of habitual drunkards in the lump; he doubted very much if any satisfactory measure warranting the treatment of insane drinkers would ever be passed; but he would be willing to support such a measure, because he knew it could be advocated on well-established medical data. But he deprecated as an utterly Utopian scheme any direct interference with the vicious inebriate. The only tangible means by which the medical profession could hope to affect that vice would be by throwing its great weight into a measure providing for rational reform in our licensing laws.

Mr Cooper, advocate, said he was present at the request of Mr Morton. Mr Morton sent the Bill to him, and requested that he should be there and say anything that suggested itself to him, upon rather the legal aspect of the Bill than the medical. Upon the medical aspect he was incompetent to speak. Upon the legal aspect of the Bill he would venture a few criticisms. He felt his position rather a difficult one after the destructive criticism that had just been pronounced by Dr Batty Tuke; and if he might criticise that criticism at all, it would seem to him to be that Dr Batty Tuke objected to the Bill because it could not do everything. It has been a motto in British legislation for hundreds of years past that it was better to have half a loaf than no bread. Although they were not able to root out drunkenness altogether by this Bill, still they hoped to be able to do away with it to some extent, and to alleviate the sufferings which resulted from it. Dr Stewart and Dr Yellowlees spoke with great admiration of the preamble of the Bill. He felt sorry to suggest that the preamble perhaps was, as it existed at present, a little out of place. He thought that this very careful definition of a habitual drunkard which was given in the preamble would be better stated in the definition of the word patient. The preamble was a part of the Act of Parliament. He thought it was an awkward part in which to place an important definition; and if he had had the framing of the Act, he would have suggested that the first part of the preamble should simply read that there was no adequate provisions made in the Lunacy Acts for the proper treatment of patients. There was, he thought, no reason why the definition which was given in the preamble should not be stated in the third clause, where the word patient was defined. He thought that that would put the Bill into a proper form. With regard to the remark that narcotics did not seem to be included by Mr Morton, he noticed that in Schedule E, giving a form of declaration by an applicant for the detention of a person, the word narcotic was applied; and he thought there were other sentences in the Bill which plainly indicated that Mr Morton had in view persons suffering from the habitual use of narcotics as well as of alcoholics. He would not trouble them with the minor alterations, which were merely verbal and formal. He thought that the fear thrown out by Dr Yellowlees with regard to the Lunacy Commissioners not being ready to take up this duty was somewhat odd. The Lunacy Commissioners were the servants of the public, and he did not think they should complain for having put upon them this work, which could be a very slight addition to their present work. Dr Tuke referred to the enormous difficulties that would result in the carrying out of this Act because of the great number of persons who would become candidates, either of themselves or by their relatives, for admission into the restorative homes; but, especially since the taxing clauses had been cut out of the Bill, it should be remembered that the only persons who would really get admission to these homes would be persons who could not only pay, but could absent themselves from business for a period of a year or more. It would not be those who had to earn their daily bread. In fact, the Bill, especially after the clauses about assessment are cut out, would only be applicable to persons suffering from habitual drunkenness, and who had sufficient money to keep them in the homes for the necessary time. The only other remark which he wished to make was about the clause of application for detention by relatives. He quite agreed with what Mr Taylor Innes had said, that he did not think Parliament would look at the clause if it did not provide for intimation being made to the person whom it was desired to detain; and he thought there was in the 19th clause, in the very last sentence, a very dangerous provision made. It read,—"It shall be lawful," etc. If they passed that clause, every person who wanted to get out of the homes would make a disturbance, and would at once be put out. Another provision he would suggest,—that if a person detained in a home refused to allow his funds to be paid over for his maintenance, application could be made to the Court for the appointment of a judicial factor, who would during the time of the person's detention have control of his funds, so that they could be paid to the people who were maintaining him in the home.

Dr Littlejohn would have preferred to have waited till some of the specialists had spoken, but having been called on by the chair, he would answer the question put by Dr G. Stewart. He (Dr Littlejohn) had never known of a single case of cure having been effected by residence in jail. The reason why was, that the sentences were too short. The appetite for drink was only whetted by this temporary seclusion, and the prisoners, on regaining their liberty, fell easy victims to their former excesses. With reference to Mr Morton's Bill he had a strong feeling that it did not go far enough. The time had gone by for special class legislation. The franchise had been extended, and the working classes were now fully alive to their privileges, duties, and dangers. Were they at this time of day to legislate for the upper ten thousand who were far more able to assist themselves than the middle and lower classes who in the matter of this terrible disease were practically helpless? He saw a great deal of this form of insanity among the poor. He had the curiosity to ask the Clerk of Police in this city how many of such cases might pass before the inferior Court every year. On consulting the records, it was seen that at least fifty cases turned up annually who were fit subjects to be dealt with as proposed in Mr Morton's Bill. Now, why should not these cases be dealt with by such legislation as was proposed for the better classes? They spoke of the liberty of the subject, but the whole of the tendency of the law at present was to interfere with that liberty if the public safety, or even the well-being of the individual, was in danger. They had reformatories where children were confined and brought up as useful members of society. A child who committed theft, or was not under proper control, was taken from his parents for a series of years, and society approved. Then there was the Infant Protection Act, which insisted on compulsory registration: and evidently, after the painful occurrences of the last few weeks, it was evident that the stringent provisions of this Act would be considerably extended. Lastly, with regard to the difficult subject of infectious diseases, parliamentary committees had decided that for the public safety it was expedient that such cases of illness should be promptly notified to the authorities, and, whenever necessary, isolated in an hospital. He was, therefore, clearly of opinion that it would be safe to apply to Parliament for an extension of the principle of this Bill to all classes of the community, and the evil being recognised, everything connected with the machinery of the Bill should be open and above board. It would be hazardous to allow a man quietly to place himself in these homes. Any one might give a false name, and thus obtain temporary retirement to suit his own purposes. He thought it important that all such parties should feel their position, and while undue publicity might be avoided, no person should be admitted to these homes unless he had gone before the sheriff, and had his statement formally made and recorded that he was voluntarily depriving himself of his liberty for a certain period of time. He felt strongly on another point. He did not know, but he would like to ask Drs Tuke and Clouston if they had power to compel their patients to do any work if the patients claimed to remain idle. [Dr Tuke—We have not the power.] Now, he (Dr Littlejohn) would make work of some kind or other a *sine quâ non* in these homes. Without healthy exercise and work it was impossible to effect a cure of this disease, and the superintendents of these homes should be empowered to fix the kind and amount of work for all patients of both sexes under their charge, and to see that it is done. The occupants of the homes might get a portion of the produce of their work, the rest going to diminish the expenses of the establishment.

Dr Clouston said he was well aware that one's remarks at that late hour must be very concise. When he came there he thought they were not to discuss Mr Morton's Bill so closely. He thought they were rather to discuss the general question, and to give their opinions regarding it. To the general question he would therefore confine his remarks. In any action they might take, they would be wise, in the first place, to limit such action to that form of drunkenness which they could scientifically reckon to be disease. He thought the more quietly and the more gently they insinuated the thin end of the wedge in this matter through the House of Commons the better. In the long run they would reap the greater benefit. He scarcely believed that the definition which had been given in the Bill was a right definition of even diseased drunkenness. He would say it would be wiser to accept a general term, such as habitual drunkard. It would be a simple matter to answer, How was drunkenness related to disease? but more difficult to reply to the question, How could they make it out to be a disease? He thought the very latest investigations into the functions of the brain proved the relation, and the latest facts in regard to the disorder of the higher brain functions all tended towards the idea that the practice of alcoholic drinking in certain cases is a disease. In the first place, it was strictly analogous to other insanities. They had no difficulty when a man intellectually was so far different from his fellows that he believed things that no other man believed, in saying that that man was diseased. If he entertained delusions of suspicion in regard to actual matters of fact, believed matters of fact to be not fact, they had no difficulty in saying that that man laboured under a disease. That man might be in his affections and in his will in a wonderfully sound condition, and even in his conduct he might not be far wrong, still they called it disease; and in the same way, if a man changed in his affective nature, or if a mother totally changed her affections, they had no difficulty in saying that it was disease. If a man changed in regard to his still higher functions of inhibition or volition, they had no difficulty in saying that he was an insane man. When a man was so diseased in his impulses and in his will or inhibition that he had irresistible impulses towards suicide, no medical man hesitated to say such a man was diseased. If a man in certain other circumstances had changed in his higher inhibition—say when he has an irresistible craving for alcohol or morphia, or cocaine or chloral—why should that man be regarded as not insane? Why should such a man—especially if corroborated by other circumstances, *e.g.*, if you find him belonging to an insane family, if you find a man taking this craving at certain times, and being under its influence at certain times and free from it at other times—not be regarded as diseased? From the time he had the craving he exhibited one of the symptoms of the higher brain diseases in the shape of sleeplessness. He thought the more they knew of the brain the more certainly they were led to the disease theory on purely scientific grounds. It would be said by some that the disease was brought on by the patient's conduct. This was unquestionably true, but he asked the members of the Society, as medical men, if it was not the case that a great many diseases are not brought about by evil conduct, and which the sufferers could have helped if they had liked? Did they hesitate in classing epilepsy as disease because the man originally indulged in such conduct as brought on this complaint? Dipsomania might be regarded as disease, even though the man himself had caused it. He took it that they should legislate on the assumption that they had a disease to do with. It might or it might not be the case that disease was present in any given drunkard. In regard to the causes of dipsomania, they knew that many cases resulted from the action of causes that caused ordinary nervous diseases—from blows on the head, from loss of blood in many cases, from exciting labour, from living under bad hygienic conditions. All these were causes of ordinary disease. Hereditary drunkenness was closely allied to nervous disease. It was mixed up in many ways with real insanity. They ought to legislate for such people. Dr Batty Tuke had said that ordinary public asylums were the proper places for them. He regretted that in this he differed from him. It had been his experience—and he was surprised it had not been Dr Tuke's—that persons labouring under this disease did not do to mix with ordinary insane people comfortably and for the good of both; and there was another reason, which had been alluded to, that they could not detain them long enough to do any real good. In a case of ordinary mania they could only detain him just sufficiently long to see that his convalescence was secured. They could not detain such a case a year or two after the symptoms had disappeared. This was a test by which the dipsomaniac was to be distinguished in regard to his treatment, and this was one reason in addition to that mentioned by Dr Yellowlees why they should not be sent to the public asylums. So strongly did he feel on this point, that he never took in a dipsomaniac into the asylum if he could help it. He refused all such cases, because he knew he could not do the man the good that was necessary, and therefore he thought there was no good in taking him into the institution. He thought it was now time something should be done in this matter. It had been long evident to all the profession that something ought to be done.

In regard to the question of cure, he did not think they were in a position to express dogmatic opinions. They had never had the opportunity of putting a man under the proper conditions. He thought they must face not only the case of the confirmed drunkard, but they must ask for power to take care of the drunkard at the early stage of his disease, and at that stage place him under proper treatment, a more difficult matter from the legal point of view. If this Bill had been only to affect the confirmed case he would have taken little interest in it. Such confirmed cases were nuisances to society and their friends. Perhaps they did not deserve a great deal of money spent on them. But what of the relatives of dipsomaniacs and society generally? Had they no claims? Surely they had. Should a woman, because she had had the misfortune to marry a dipsomaniac, neither have the power to try and cure him nor to leave him? Many of the medical profession believed that the disease could be cured in the earlier stage. Was there any other disease whose proper treatment was put off till it was confirmed? There were no doubt many objections to be faced, such as that of the liberty of the subject. He thought, however, they were entering on a time when the liberty of the subject would be less thought of than it had been in past times, because there was no real risk to it now. That was a lesson they were learning from the American legislatures. They took care that the liberty of the subject did not interfere with the working of the body politic. In this country they would come to that. If the dipsomaniac interfered with society, he and his liberty would have to go to the wall. In regard to whether they could distinguish between vice and disease, he was quite ready to admit that it was an exceedingly difficult distinction in many individual cases.

They must also face the difficulty mentioned by Dr Littlejohn. What was to become of the poor diseased drunkard? The only way he could think of at the present time was to call on the public to subscribe money to provide these homes for the poor. The rich could help themselves. They could provide homes for themselves. He did not see why, as the Legislature taxed us for reformatories, they should not give the diseased drunkard in the early stage of his career a chance. Of course, they did not like to be taxed any more; but the inmates of the homes could help by working for their livelihood, and in that matter of work he most strongly and earnestly confirmed what Dr Littlejohn had said. They would never cure a drunkard unless they provided him with employment and he wished to be cured himself. He thought the best suggestion he could give at present was that they should try and get the drunkard at the early stage away to some Highland home, where he could get some real hard work to do, and where he would live in a high moral atmosphere. He said the atmosphere in which a dipsomaniac lived was most important. And for this reason these restorative homes should not be too large; they should not put too many of these people together, for they corrupted each other. They must restrict the size of them to do good. To put anything like 200 together would be outrageous. They would have to restrict them so that the domestic character would have to be preserved, and the feeling of control and direction on the part of the head would have to be preserved in regard to each individual.

He had only now to make two suggestions in conclusion. First, that this Society petition generally in favour of new legislation for the habitual drunkard. He thought they might take it for granted that the House of Commons and the House of Lords and their various committees would see that the Bill was not dangerous to the liberty of sane people. He had one more suggestion to add, to which he attached the greatest importance, viz., that a clause should be added to the Bill making provision for the compulsory detention of diseased drunkards in ordinary houses, with suitable guardians, in suitable parts of the country, under proper guarantees and under proper inspection. When they sent these diseased drunkards away to ordinary homes, they should be under proper superintendence, and the guardians should have a certain authority over the patients. There might be a difficulty in getting proper homes built. There was no difficulty in getting a Highland farmer or clergyman for £50 or £100 a year to look after a man.

Mr Shaw, advocate, thanked the Society for the honour they had done him in asking him to the meeting. He did not intend to make any observation, further than to say that he agreed almost entirely with the remarks of Mr Taylor Innes. With regard, however, to the preamble of the Bill, he rather differed from him, and he also differed from Mr Cooper. He thought they should leave out altogether the proposed preamble, which was academic rather than practical, and he would not adopt Mr Cooper's suggestion to put it into any other part of the Bill. A wiser proposal, he thought, than supporting the present Bill, which had been so carefully drafted by Mr Morton, would be to secure an addition to present legislation in the form of compulsory power, as provided for in what is practically section 17 of Mr Morton's Bill. The Habitual Drunkards Act of 1879 contained in section 10 a permissive clause, and they found that the almost entire and sole objection was that that clause had been insufficient for the purpose aimed at, as indeed any one would suppose who would read it. What they now wanted was mainly to get the compulsory power added to those at present existing by Statute. A bill presented to Parliament containing such a preamble and provisions so elaborate, which might be the means of causing needless debate in the House of Commons, would be unpracticable in the present state of public business. Any scheme emanating from this Society would be much simpler, if it merely proposed to add a compulsory power to the present permissive clause,—in fact, to dovetail section 17 of Mr Morton's Bill into section 10 of the Act of 1879, and not to trouble about preambles. He did not quite agree with the views expressed by Dr Tuke. He had come for instruction; and he confessed that he went away with a profounder conviction than ever that in the case of men who are subject to this disease, whether inherited or whether caused by actual vice in their individual history or not, they must deal with these men as not only being harmful to themselves, but as requiring to be treated by public authority in the interests of society, because they were the pests of society.

Dr Connell, Peebles, said some persons might wonder why he was present. He had come simply to give his experience. He had been Medical Officer for twelve years to one of these restorative homes, viz., the Brownsland Home; and during that time he had managed to form some opinions and to collect some facts. He had heard some remarks and opinions which were wide of the mark. He thought it right, therefore, to give them a few grains of fact.

In the first place, he might mention that the home he spoke of was certainly not for the upper class. He had a copy of the rules and the last report in his hand, and the first sentence was that this institution is "intended for women who have fallen," etc. The expense of staying there was not large. They paid only 8s. per week. He did not say, however, that that paid the expenses. The difference was contributed by a number of philanthropic ladies and gentlemen in Edinburgh and elsewhere. 122 people had been treated in the home within the last twelve years. They had resided in the home for various periods. Some had resided a short period, and others a much longer—from two months to three and a half years. They had a lady still who has been there three and a half years, and who was most anxious to continue at the home, because she did not wish to go back to the temptation. Of these 122, 44 were reported as having done well. Some had broken down; 1 had been very hopeful. One of the women treated was the wife of a Liverpool captain, and she resided for fifteen months in the home. She had then gone back to her husband, and had been a good wife for the last five years. These were facts which spoke for themselves. He thought there had been a great deal too much pessimistic criticism, especially that indulged in by Dr Tuke. Some one had remarked that Dr Tuke's objection to the Bill seemed to be that they could not get everything by it, but still surely they would get something. He thought it quite warrantable to go in for such a proposal as was made in the Bill. He thought it right also to say concerning the cases in the home he referred to, that they had never had one single case of acute illness, and some of the women had been exceedingly bad drinkers. They had spent the exceedingly cold winters of 1879-80, in which year the temperature at Peebles was on two occasions 14° below zero; and the years 1881-82-83 were exceedingly cold, and for eighteen weeks that year they never saw mother earth, and yet they lived without stimulants. He had only refused one case. She came to the home in the winter, and he would not take the responsibility of treating her through a cold winter without some stimulant; and as the Committee did not allow that, she was sent home. He had never heard anything further of her since. In the home they had women varying from nineteen to sixty-nine years of age, and they were chiefly of the poorer classes. One woman was the widow of a doctor in the Indian army. One was the sister of a doctor who supported her, and, with the exception of one or two others who had seen better days, they were all the wives of working men, such as tin-plate workers, engine-drivers, and such. It was not, he thought, the case that they would have difficulties in legislating for the poorer classes. All that the matron and the master of the home felt they required was simply the right to keep these people there. The house where they were kept was as private as possible. It was 3½ miles from a railway station, 7 miles from a town, and lay between two valleys, and high hills on all sides; and the road leading to it had a long slope, so that no one could escape without having two or three pairs of eyes fixed on them, and thus being detected. There had, however, been escapes. Two or three women had escaped, but only to wander for a while on the hills and to return to the home. A single case of suicide had occurred, which he did not attribute to the want of drink. She had shown signs of it before. It was a case, he said, which would not have been received if it had been known. One of the conditions was that mental alienation should not have been shown. It was a fact that these people in the home had done well without alcohol. It was not the case that they would contract disease. Of course if there were cases where alcohol was necessary there would be no difficulty as to arranging. He had one thing to say about habitual drunkards, and he would say it in plain English. It was that when a man or a woman reduced him or herself to the level of a beast, then he or she deserved to get the treatment of a beast. Though he said this in such rough, harsh language, he did not mean to be unkind. None of them, he said, would be unkind to their beasts. He meant simply that when these people did as he had said, they should be reduced for a time below the level of manhood. That, he said, was a proposition which none of them would refuse. There was another proposition which he could not help formulating. It was, that it was not only lengthened residence in these homes that would do good. He would have been surprised if Dr Littlejohn had known of any cases of cure. A psychological reason for a year being the minimum length of time was that in the diseased condition of some of these people the coming through a certain number of months, through a cycle of the seasons, would act as a support; and if they have kept it through the various seasons they then propose they might venture back into the temptation. That was, of course, only his own opinion. Another thing that he desired: he was a pessimist in the matter of curing the patient by merely length of time. How many, he asked, even after this period, should be trusted again to give entire satisfaction? These institutions, he said, must be conducted, as this home was, upon Christian principles. He thought Dr Littlejohn's proposal as to work being compulsory was a good one. The only thing that would raise them was to instil into them Christian principles by a sound moral training, and even then he would say that the Church could never quite trust them again to themselves. They would require to be kept out of temptation. They required to put a fear and dread of it into them, such as they would have at poison. He thought doctors could do a deal to effect a cure. They should never carelessly prescribe stimulants to any one. He knew the case of a young man. He had kept away from it for eight months, and then he went and took the advice of a doctor. The doctor prescribed stimulants, and he has lost hope of him since then. The only hope he had was if the people kept rid of it entirely. He looked forward to the passing of such a Bill as this, and he hoped it would pass. It would do an immense amount of good. He had a firm conviction that if they were to keep them in these homes they must make them work, and another thing, he said, was that you must not make the homes too large. Theirs was not too large. It had eleven rooms, and there were twelve women there at a time. They did needlework and all kinds of sewing, and garden work when possible; and, he said, that during those twelve years he had been connected with the home he could look back on all the people who had been in the home, and say that there was scarcely one that had not been improved by their stay there.

Dr Clouston asked, How far off was the nearest public-house?

Dr Connell.—The nearest one was 7 miles. Carts and vans used to come, and occasionally brought liquors, but they were now kept in quarantine, and not allowed near the home.

Dr Strachan, Dollar, said they were all agreed as to the great necessity of doing something in the case of habitual drunkards. They were the pests of society, and should be shut up. He thought legislation would be well applied if they could carry out the suggestion of Dr Littlejohn. He, however, was doubtful as to the cure which could be got by such lengthened incarcerations. After any one had been in these restorative homes and gone out, it was absolutely necessary that they for the rest of their lives should be total abstainers. They could not venture at any future time to taste at all of alcoholic drink. That was his experience. He had known men for years total abstainers, and who began to feel themselves strong, and returned to the temptations and began to take a little, and as sure as they did so they fell back to their old ways. He, however, considered that this principle of total abstention could be put in practice without that long incarceration. That was his own experience. He was not a teetotaler himself, and he did not advocate teetotalism for every person, but he certainly did advocate it for habitual drunkards. It was their only safety and cure. He thought some good might be done by their going into this incarceration at the earlier stages. He would mention a lady patient of his, whom he said was as bad as most cases. One fact he would mention, the W.C. in the house had got stopped up. They could not understand the reason, and sent for the plumbers to examine it, and it was found stopped full of *Eau de Cologne* bottles, the contents of which she had been drinking. He thought she must be considered to have been a pretty bad case. That lady consulted him, and he put it before her, that her only chance of getting over it was total abstinence, and he strongly advised her to become a teetotaler. She took his advice, and has kept to it for over twelve years. And he would press strongly upon all medical men that that was a very important matter. These people required to be protected from their friends. It was through the inducement of society to take a little for their stomach's sake that brought back the craving again. He would again strongly urge upon medical men to get their patients to become total abstainers, and to take an active part in the movement. He thought that could be done quite well without legislative incarceration. There were a proportion of people who were liable to this disease, and the ordinary customs of society led them into it. It was not the lighter drinks which produced the disease and did the harm. It was the strong drinks, such as whisky and brandy and gin. These were the drinks which produced the craving. They excited the nervous system for the time with consequent reaction for which the people craved relief. He would look upon it not as an indulgence, but as a craving for relief from a great suffering. He would suggest whether it would not be possible for this Society to try and get these strong drinks classed as poison, and sold like laudanum. If laudanum were sold as the strong drinks were, they should then have the opium habit as productive of evil as the alcoholic habit was now. If they were to restrict the sale he believed they should do away with this disease of habitual drinking. That was the direction in which he should like to see legislation taken, and he was glad of the opportunity which had been afforded him to put this before the Society. It appeared to him that the advocates of abstinence went too far. Comparatively little opposition would be had to this restricted legislation in the matter of drinking.

Dr Andrew Smart said that, on account of the lateness of the evening, and inasmuch as the question had already been very fully and ably considered by previous speakers, he would only add a few words in explanation of his experience of the treatment of alcoholic cases as carried on in the ward under his charge in the Royal Infirmary. During the three years he had been in charge of Ward VI.—a ward, he should explain, for the reception of acute and chronic nervous diseases, but better known as the "D.T. Ward"—there had been in all 1770 cases under treatment, 1262 of these being alcoholic. That gives an average of 420 cases of that description admitted to and in some way disposed of every year. Of the 1262 alcoholic cases referred to, it was found necessary to send 115 to asylums for the insane—63 of these being males and 52 females. That shows an average of about 40 persons annually certified as insane and passed on from Ward VI. to asylums—the cause of the insanity in nearly all these cases being admittedly due to the drinking habit.

Looking closer at these figures, he found that in his experience of each of these 115 cases he had not been able to procure certification in lunacy by the medical men sent to the ward for that purpose until the patients had reached that stage of the disease in which the pathological conditions referred to by Dr Clouston were unmistakable. Medical men, looking to the present state of the law, and having a reasonable regard to their own safety and protection, declined to certify insanity at an earlier stage in the absence of these pronounced conditions.

It is painfully interesting to notice that the average age of those 115 drinkers sent to asylums is under 37 years; and that the time taken to acquire the necessary pathological qualifications for being certified as fit for the madhouse varied between ten and fifteen years, this difference no doubt depending upon the constitutional susceptibility in each individual to the toxic action of alcohol in bringing about the necessary degree of brain destruction. No doubt the kind and amount of drink taken also bore some relation to the period of morbid development. It will be apparent from these considerations that before the habitual drunkard can be finally disposed of by transference to an asylum, a period of ten to fifteen years' active drinking will have to be undergone in order to bring the drinker within the scope of the law as it now stands. It is, of course, during this protracted period of inveterate drinking, when nothing by means of restraint can be done, that the domestic and social inconvenience and mischief are most oppressively felt. It may assist one to more adequately realize the magnitude and urgency of the matter under consideration to know that, in so limited an area as Ward VI., there are *annually* under treatment considerably over 400 of the class of patients described—a number equal to two-thirds of all the patients in the Royal Infirmary on any given *day* of the year, and that nearly one in every ten of these will be sent to asylums as incurably insane. These facts will doubtless impress you, as they have me, with the need which exists of obtaining legal control over these infatuated persons at a much earlier period, to be of any real service to them or to others interested in them. The insane qualification arrives too late. It is enormously destructive and fatal. It will doubtless also be evident that any shred of legislation intended for the benefit of a section of the community will be inadequate to meet the necessities of the situation. To do that any suitable measure, in order to meet with general public approval, ought to possess the recommendation of being adapted to meet the case of any individual in the community who may stand in need of the benefit of its application. In order to obtain the required legislation, our present notions as to the "freedom of the subject" may have to undergo some modification.

Mr W. C. Smith, advocate, said he thought it followed from the statements of Dr Connell, and from the experience in many other quarters, that most valuable results could be got from Dalrymple's Voluntary Retreat system, but then, as he understood the matter, the patients treated in Dr Connell's home were not the mentally diseased patients who were to be provided for in Mr Morton's Bill. He thought when they passed from the voluntary system to the principle of Mr Morton's Bill, that they arrived at a system which was impracticable and had no future. He thought the advocates of this system were in a dilemma. For political reasons they could not imprison a sane man who was drinking himself into insanity. They could not apply compulsion in the early stages of habitual drunkenness, which Dr Clouston had referred to. It was necessary before they could put such men into an asylum against their will, that it should be proved that mental disease was developed and established. He was afraid, when it was possible to prove that in Court, all such cases were absolutely hopeless, and therefore the main reason for imprisonment disappeared. He thought the only way of dealing with the matter was by the use of moral influence to stop a man from drinking, and by reforming the licensing laws.

Mr J. P. Coldstream, W.S., said he had come here this evening and had listened with great interest to the discussion. They had heard in regard to this question connected with the measure of Mr Morton's that it would be exceedingly useful, and they had also heard that it probably would be detrimental, and it would be a bad measure to pass into law. He was a great admirer of Mr Morton, and anything proceeding from him would receive his careful consideration. He also was a great admirer of others who had spoken, and therefore he said he was left in a great dilemma if he was to go by medical men. What might be the result of this measure no one could tell. He certainly agreed with the sentiment which had been expressed, that they could not at this period of time legislate for one class only, and as he understood this Bill of Mr Morton's, it would apply to the upper class only, and not to the lower. He did not see why a Bill of this kind should not apply to the lower classes as well as to the upper class. If they were to be assessed for such patients, who to some extent must be considered as criminals, he said it was quite right that they should be assessed for supporting an institution such as had been proposed. He saw no objection on that score. He thought the Bill should be passed into law, but it should be one for all classes of society. The preamble had been objected to by Mr Shaw, and in regard to that matter he sympathized with the remarks which he had made. In regard to the period of detention, he should say two years was certainly a short enough time, but he should propose that it would be best not to limit the time, and detain the persons until they had a certificate of cure. He was glad to hear what Dr Connell had said, because up to that time it appeared to him that they had been labouring in the dark. He had given his experience. That experience was very valuable. He agreed that total abstinence was one of the best cures. Dr Connell had said that the work of his home was conducted on religious principles. That was in fact the best course. The spiritual remedy would in reality be the only radical cure of the disease from which these poor people suffered. They cannot always influence these people by religion, but in such cases as Dr Connell mentioned, the religious element being there was one of the utmost importance.

Professor Simpson said, that amidst all the differences of opinion, he thought there must be a wish that something should be done. Discussion would have great value in directing their minds to the need of legislative measures for the management of habitual drunkards, and in showing the importance of some of those that had been advanced in Mr Morton's proposed Bill. But it seemed to him that the discussions would not have the full effect unless they were embodied in a resolution. He had been thinking of framing such a resolution, when their energetic Secretary put a draft resolution into his hands. This, with Dr Ritchie's approval, he had slightly modified, and would at once submit to the meeting as follows:—"That the Society memorialize the Secretary of State for Scotland and the Lord Advocate, praying the Government to initiate legislation for Scotland in the ensuing session of Parliament on the lines indicated in Mr Charles Morton's Draft Restorative Homes Bill, or in other ways that may seem to them in their wisdom to be more desirable, to provide compulsory powers of control, and detention of habitual drunkards in properly regulated houses."

Dr Littlejohn seconded, and the motion was unanimously agreed to.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION L.—MEETING VI.

Wednesday, 8th May 1889.—Dr UNDERHILL, *President, in the Chair.*

Dr Brewis showed—(1.) Two MULTILOCULAR OVARIAN TUMOURS containing PAPILLOMATA in their interior. (2.) UTERINE APPENDAGES showing IMPERFORATE FALLOPIAN TUBES which were associated with a congenital antiflexed uterus. (3.) DISEASED UTERINE APPENDAGES, which were prolapsed, and were removed on account of the constant pain which their abnormal position produced. (4.) SARCOMATOUS TUMOUR, probably ovarian, which was removed from a patient post-mortem. (5.) FŒTAL HEAD removed by BASILYSIS, showing the effective manner in which Professor Simpson's basilyst broke up the base.

II. *Dr Milne Murray* showed specimens of DRUGS, a NEW SPECULUM, a GALVANOMETER, and an ELECTRODE.

III. *Dr Haig Ferguson* read his paper "ON A VARIETY OF POST-PARTUM SHOCK: ITS NATURE, CAUSE, AND PREVENTION," which appears at page 33 of this Journal.

Professor Simpson had not had an opportunity of seeing cases of collapse such as Dr Ferguson had described. The series of cases were of special interest, and it was an important service to obstetricians to have their attention called to the possibility of their occurrence. In all probability the explanation which Dr Ferguson had offered of the cause of the syncope was correct, and it was of the highest importance to have in view the risk of compression of the sensitive ovaries. Dr Ferguson had correctly described the position of the uterus as being oblique, and even in the unimpregnated uterus the anterior surface was directed somewhat to the right side. To avoid the risk of such compression of the ovary he (Prof. Simpson) thought that instead of directing the hand, as Dr Ferguson had suggested, with the ulnar edge of the hand to the left sacro-iliac synchondrosis and the thumb towards the right pectineal eminence, the practitioner should simply aim at grasping the uterus on its anterior and posterior aspects whatever might be its direction. The paper was an interesting and valuable one, and would do good in calling attention to an accident which was rare and worthy of further observation.

The President thanked Dr Ferguson for his interesting and suggestive paper. He had not happened to meet with such cases as Dr Ferguson described, but he thought the explanation put forward in the paper was probably the correct one. He had frequently felt what he believed to be coils of intestine slip away from under the fingers while manipulating the uterus after-labour, but he was not conscious of ever having felt the ovary. He could not understand, however, why these cases of collapse were not more frequently observed if Dr Ferguson's views were correct, because the teachings of the present day laid so much stress upon the prevention of hæmorrhage after labour that every student and nurse thought it was their first duty after the child was born to seize and roughly squeeze the uterus to insure contraction, and he failed to see how it was that the ovaries so constantly escaped compression. Dr Ferguson's paper would, at all events, lead them to observe more carefully than heretofore the exact position the uterus occupied, and to be more careful and, perhaps, less vigorous in their manipulations.

Dr Haig Ferguson thanked the Fellows of the Society most heartily for the very kind way in which they had received his paper, and for the friendly and favourable criticisms which it had evoked. He had little to add by way of reply. Professor Simpson laid stress on the importance of securing an antero-posterior grasp of the uterus, and it was with a view to this that he had ventured to bring his paper before the Society to-night, and to try to show the danger which a lateral grasp is apt to entail. It seemed to him that the risk of injuring the ovaries during the third stage of labour and immediately post-partum had been to a great extent overlooked. Dr Underhill was surprised that the ovaries are not injured more frequently, and that there are not more instances of shock in consequence. He (Dr Ferguson) thought that such accidents are commoner than is supposed. The reflex symptoms may be, and often are, comparatively slight, and are not often so grave as in the three instances he had recorded. He was convinced, however, that the cause of the symptoms is usually not recognised, and that they are erroneously attributed to something else,—often to a trifling hæmorrhage, which certainly would not account for the symptoms. He further thought that these severe reflex symptoms are more liable to occur in the upper classes than in the lower, as in them the nervous system is usually more easily affected by outside influences, and there is often a considerable degree of morbid sensitiveness to external stimuli.

IV. *Dr Berry Hart* read his paper on "MITRAL STENOSIS IN LABOUR," which will appear in a future number of this Journal.

Professor Simpson thought the Society indebted to Dr Hart for the record of the cases which he had just read. It was important to get the results of the post-mortem examination in all the patients who died in pregnancy or the puerperium with various forms of heart disease, especially when, as in Dr Hart's first case, there had been observation made as to the physical condition during life. He (Professor Simpson) had seen that patient in the Maternity, where from the moment of entrance her case was hopeless from the morbid conditions of lungs and kidneys which complicated the heart affection, so that it was not one from which we could draw conclusions as to the influence of cardiac disease pure and simple.

Dr Haig Ferguson had had an opportunity of seeing one case bearing on Dr Hart's interesting paper. The patient was a primipara who suffered from double mitral, double aortic, and tricuspid murmurs, with extremely dilated right heart. She was admitted into the Hospital, and with treatment and rest she became much better as regards her symptoms, the dyspnœa being almost gone while she lay in bed. The patient was six months pregnant. Labour pains came on suddenly, and immediately all her symptoms became aggravated, and orthopnœa and cyanosis developed with great severity. As the pains set in more regularly the patient's condition became more and more serious. The os uteri just admitted the finger, and was fairly soft. The indication for speedy delivery was apparent. For this purpose a little chloroform was administered, and he proceeded to dilate the cervical canal with his fingers. This he accomplished in little more than half an hour. He then applied forceps, and delivered the fœtus as rapidly as possible. The placenta, unfortunately, was adherent, and required to be scraped away from the interior of the uterus. The uterus contracted well, and there was no hæmorrhage. After this the patient rallied a little, but her heart began to fail again, and she died from cardiac failure about three hours after delivery was effected. He regretted much that more hæmorrhage did not occur from the uterus, as it would have relieved the over-distended right side of the heart, and possibly would have allowed the circulation to have become re-established. If he should ever again meet with a similar case he should feel inclined to perform venesection, for the purpose of relieving the strain on the right side of the heart. Unfortunately, there was no post-mortem examination, so that the actual condition of the heart could not be ascertained.

The President was much interested in Dr Hart's cases; they brought to his mind the great papers on heart disease in connexion with labour which were read before the Society by the late Dr Angus Macdonald. He thought that, had the first case been in a position to have received careful treatment during the earlier part of her pregnancy, the result might have been different. He did not quite follow Dr Hart's explanation of the sequence of events in the third case, nor did he quite understand how the left heart came to be empty and the right heart over-distended, as the post-mortem showed was the case. He had recently had under his care a woman with a bad mitral stenosis. During her pregnancy she had frequent attacks of dyspnœa, and was seldom able to sleep unless propped up in bed. By continued use of strophanthus throughout the pregnancy she arrived at the full time without mishap and in fair condition. The labour was tedious in its earlier stage, but as soon as the os was dilated chloroform was given and the forceps applied, and after a period of considerable dyspnœa and consequent anxiety, she rallied and made an excellent recovery.

Dr Berry Hart thanked the Fellows for their reception of his paper. In the aortic stenosis case the heart disease was primary and the pleural and pericardial effusions secondary to it. The renal affection with its old infarcts was also in all probability secondary. Dr Underhill's question was a necessary one. The right-sided dilatation was secondary; of course left-sided primary affections were the usual ones.

Part Fourth.

PERISCOPE.

MEDICAL PERISCOPE.

By Francis Troup, M.D.

Berlin. Klin. Wochensch., Nos. 17, 18, 19, 1889.—Ebstein of Göttingen treats of the causes, symptoms, and treatment of gout. He points out the interchangeability of gout in many cases with stone, obesity, polyuria, and diabetes mellitus. His treatment is simple: moderate feeding, not denying patient good butter; plenty of water, alkalescent if necessary or wished; abundant use of fruit, such as cherries and strawberries. Great quantities of water are not, however, to be taken, being neither advantageous to stomach nor patient. The diet being so regulated that intake and output are in equilibrium; measures and manipulations which promote the circulation of the juices must also be adopted. For what these are he refers his readers to his book, *Das Regimen bei der Gicht.*

He considers alcoholic drinks harmful, not only when one is actually gouty, but also in one predisposed merely. They should be forbidden in every form unless conditions of debility arise and call for them as well as other stimulating means. If gouty people would only follow this advice about alcohol, they would spare themselves many a pain and many a "Kur," which can never render such service as abstention from alcohol, and never can thoroughly repair the damage which alcohol has occasioned.

Ebstein, like Sydenham, knows no specific medicamentation for gout, and apparently thinks that, *mutatis mutandis*, many new methods of cure which find believers *intra et extra muros* are about as absurd as the entrails of frogs and toads boiled in oil recommended by the younger Pliny.

In the treatment of the actual gouty paroxysm, colchicum, and more recently soda salicylate in 8 to 10 gramme doses per day, play the principal part. Some praise the former as a never-failing panacea, and others find it useless or even harmful. It is the same with the salicylate, and Ebstein has seen it cause the gout to promptly disappear from a joint only to crop up in another. At the present time we as little possess a specific against the gouty paroxysm as against the gout. Nevertheless he thinks it important (together with a rational intelligent dietetic and symptomatic treatment, which must not weaken the patient) that as long as the general symptoms and violence of local conditions will permit, the patient should not be condemned to absolute rest, but according to his actual condition appropriate means must be enforced to secure the circulation of the fluids in the diseased parts.

Ibidem, No. 18.—Kaltenbach of Halle gives his experience of total extirpation of the uterus. From 15th May 1887 to 1st April 1889 he has subjected 30 patients to total extirpation of that organ for carcinoma. He summarizes the results as follows:—1 death in consequence of the operation, 12 free of relapse at this date, 15 recurrences and deaths within sixteen months, 2 whose fate was unknown at date. Those results are brilliant, and when patients and surgeons look upon the operation with more favour, still better permanently good results will be obtained. The absolutely hopeless cases will become rarer as operative local treatment of the early stages of carcinoma becomes more general.

Ibidem, No. 17.—Dr Valentini of Königsberg has a contribution to the pathogenesis of the bacillus of typhoid. He considers it likely, although not yet proved, that relapse of typhus abdominalis arises not so much from new infection as from isolated colonies of the bacillus remaining capable of reproduction in the parenchyma of organs or in the bowel, and in consequence of trifling injurious influences which find out a weakened labile organism, penetrate into the circulation, and bring about a general infection. Fränkel has demonstrated the bacillus, 147 days after the beginning of the fever, in the pus of an encapsuled peritonitic abscess. Valentini relates his similar experience, where, in the pus of an abscess on left shin, typhoid bacilli were found and cultivated on potatoes.

Baumgarten corroborated Valentini that it was really the typhoid organism. In another case of empyema after typhus abdominalis, Valentini was able to obtain pure cultivations from the pus.

Ibidem, No. 17.—Dr Tuczek relates a case of antipyrin poisoning. A strong, healthy boy, aged four years, had taken for three weeks antipyrin in three daily doses of about 6 grains each as a cure for hooping-cough. He suddenly vomited one day, and became somnolent and comatose; repeated epileptic attacks came on, complete or incomplete; rigidity of muscles, grinding of teeth, and jactitation; peculiar type of breathing, snuffling inspiration, and long pauses, as in Cheyne-Stokes phenomenon; arhythmy of heart's action and dilated pupils. At same time a macular exanthem appeared, and temperature and pulse were subnormal.

OCCASIONAL PERISCOPE OF DISEASES OF THE TROPICS.

By R. W. Felkin, M.D., F.R.S.E., Lecturer on Diseases of the Tropics and Climatology, Edinburgh School of Medicine.

Leprosy.—A curious case of leprosy—the anæsthetic variety—has been published in the *Finske Läkaresällskapets Handlingar*, vol. xxx., part 1. The disease was at first mistaken for myelitis. The patient was a man aged 33. He suffered from atrophy of the muscles of the extremities, paræsthesia, and diminished sensation, but no affection of the skin could be detected. Several years later, however, the ordinary changes in the skin made their appearance, and the diagnosis of leprosy was established. The reporter called special attention to the rare fact that there was almost complete paralysis of all the branches of the left facial nerve.

Malaria.—*Golgi on the development of the malarial parasite in tertian ague.* (Differential diagnosis between the endoglobular parasites in the tertian and those in the quartan ague.)

In 1885 Camillo Golgi investigated forty cases of intermittent fever, mostly belonging to the quartan type. He came to the conclusion that the so-called plasmodia of Machiafava and Celli were not merely the characteristic of malaria, but that they occur with such regularity as to enable the physician, not only to diagnose malaria, but also to prophesy the approach of a paroxysm by observing the plasmodia in the blood. The pigmented bodies which are found in the red blood corpuscles at first only occupy a small part of the corpuscle, presenting a round form with delicate contour and amœboid movements. This gradually grows larger and consumes the substance of the red blood corpuscles. Signs of radial segmentation then appear, which segmentation becomes at last complete.

In the present paper, which is based upon hundreds of observations, Golgi states that in only two cases did he obtain a negative result, and this negative result he attributes either to the fact that the cases were not malaria at all, or that, being amongst the first he investigated, he had not been persevering enough in his observations. He states that he confirms the observations made by Laverand and by Machiafava and others, and, although their observations were apparently contradictory, he harmonizes them all by proving that each of these observers had seen the parasite at a different stage of its development.

In general terms Golgi states that the amœboid parasite of tertian ague is the same as that seen in quartan ague, but at the same time it presents special and characteristic biological and morphological points of difference, so that an experienced observer can easily make a differential diagnosis. From the mere examination of the blood, he says, the observer can not only state with certainty that the patient suffers from a malarious fever, but he can also diagnose the type of the fever during the period of apyrexia and also during the paroxysm of fever itself. Indeed, so constant and characteristic are the changes which take place in the parasite, that it is not only possible to say the day on which the last paroxysm occurred, but also to calculate when the next will arrive; and he believes it to be also possible to state pretty certainly whether the patient will suffer from one, two, three, or more paroxysms, and whether they will be of regular or irregular type.

With regard to the development of the amœboid parasites in tertian ague, three phases can be demonstrated. The first and second phases blend to a certain extent together; the third, however, is distinctly separate.

First Phase.—If the blood, taken from a patient who has had a paroxysm of fever a few hours previously, is examined, it will be found that the red blood corpuscles contain the well-known granular protoplasm (the so-called plasmodia of Machiafava and Celli) which measures one-fourth to one-fifth the diameter of a red blood corpuscle, being the smallest size of plasmodia which is ever seen. These bodies have a very indistinct contour, but they are differentiated from the substance of the corpuscles which contain them by their whitish colour and a certain amount of refraction; their amœboid movements are very distinct. This movement gives them all kinds of shapes, and it is indeed the characteristic which calls our special attention to them. It is to be noticed that in the tertian ague this movement is far more active than that seen in quartan ague, being indeed sometimes so rapid that it is difficult to describe them. At this time the parasites of tertian ague have either no pigment at all or at best very few pigment granules. In this first phase it is not always easy to find the affected corpuscles, but their discovery is perhaps rendered less difficult by the fact that they have less tendency to shrink, and that they are generally rather larger than their fellows. Sometimes two or more parasites may be contained by one red corpuscle.

Second Phase.—All the changes which take place in the parasite, in the day between two paroxysms of fever, belong to the second phase. During this period the amœboid bodies increase in size so that they occupy the half or two-thirds of a red blood corpuscle. The contour becomes well marked, and the parasite contains more pigment; the amœboid movements are less marked, but still they are considerably more lively than the movements seen at the same stage in quartan ague. One of the most characteristic peculiarities of the malarial parasite in the tertian ague is caused by its special relation to the substance of the red blood corpuscle, and consists in the great rapidity with which it decolorizes the substance of the blood corpuscle by changing the hæmoglobin into melanin. Even during the first hours of the day in which there is complete apyrexia the red blood corpuscles containing a parasite can be easily distinguished by their lighter colour. It will be found that the nearer the time approaches to a new paroxysm of fever the paler do the red blood corpuscles in the blood become.

Third Phase.—In this phase the parasites undergo development, which leads to their division, and therefore to the production of a new generation of parasites. This division occurs at or near to the commencement of the paroxysm of fever. Golgi explains that in light attacks of fever, where the number of parasites is small, their division takes place before the paroxysm of fever; but in bad cases, where the parasites are exceedingly numerous, although division commences before the paroxysm of fever, yet it is not completed until one or two hours after the rise of temperature. Now, although in quartan ague the manner of the parasite's division has only slight if any variation, in tertian ague there are three methods of division, which may be watched under the microscope if the blood be examined at the commencement of a paroxysm of fever.

The First Method of Division is as follows:—As soon as the pigment contained in the parasite has gradually arranged itself in the middle, one notices at the periphery of the parasite a certain amount of differentiation, from which lines radiate to the centre, dividing the parasite into fifteen or twenty whitish divisions. These particles become more and more individualized, first becoming oval, then globular. The globules gradually become very distinct, get separated from one another, and at last present a circular arrangement around a central disc. Whilst this division proceeds, it will be noticed that the pigmented part of the body becomes completely separated from all connexion with the periphery. It is not exactly known what becomes of these central pigmented bodies; some of them, doubtless, are taken up by the white blood corpuscles, and it is quite possible to believe that others may remain alive and continue to possess the power of further development. It is, however, certain that the small parasites above referred to rapidly invade new red blood corpuscles, in order to commence anew their developmental cycle.

The Second Method of Division.—After the pigment has become collected and has formed into a small thick heap in the middle of the parasite, the division occurs by the gradual formation of irregularly situated globules. This differentiation does not occur in this case at the periphery, and it is this form of division which Machiafava and Celli have described.

Third Method of Division.—Golgi is not able to describe this method with absolute certainty, as he has not been able to observe sufficient cases; but in some which he has seen, after the complete destruction of the hæmoglobin has taken place, and the point has been reached at which the pigment usually collects in the centre of the parasite, it collects at one or two points near the periphery, and it is noticed that there is a distinct line of division surrounding each collection of pigment, and the rest of the parasite forms an apparent vacuum, in which one sees one, or rarely two, small globules.

In conclusion, Golgi believes that his observations point to the fact, that all the numerous variations of intermittent fever which have been described are simply variations or combinations of two original types, namely, the tertian and quartan agues; and that in all their various forms they stand in definite relation to the development of the parasite—in the one three days, in the other two, are required for the complete cycle. He points out that, as a rule, the great law with regard to these parasites is, that the more of them present the more severe the fever, and the fewer observed the lighter the attack will be. The exception to this law is, that in children, although few parasites may be recognised, they may give rise to very severe fever. He further believes that in exceptional cases, where this law is apparently broken, we must seek for the cause either in the exceptional irritability of the nervous system, or else in some at present undiscovered modifying or intensifying influence.—*Fortschritte der Medicin,* vol. vii. part 3, Feb. 1, 1889, page 81.

Note.—Golgi's previous papers were published as follows:—*Giornale della r. Academia di Medicina di Torino,* vol. xxxiii., 1885, page 734; *Archivio per le Scienze mediche,* vol. x., Torino, 1886; *Fortschritte der Medicin,* No. 17, 1886, page 775, Tafel iii.

PERISCOPE OF SYPHILOLOGY AND GENITO-URINARY DISEASES.

By Francis Cadell, F.R.C.S. Ed.

Intracranial Syphilis.—In *Studies in Clinical Medicine,* 3rd May 1889, by Dr Byrom Bramwell, an interesting case of this kind is given. Man, æt. 44, shoemaker, 13th February 1889, complained of cramps and numbness in the right hand. The fingers were forcibly flexed into the palm, with flexion of the wrist. The day before, when at work, the fingers were spread out and the wrist extended. He also felt the elbow and shoulder a little drawn. The hand was weak and powerless after the attack. There was no loss of consciousness at the time, but his tongue felt stiff and he spoke with difficulty. The tongue felt numb and cold after the attack. There was severe nocturnal headache on the left side, chiefly towards the front, with sleeplessness, also tenderness on the outside of the head. Five years ago he had a chancre, followed by secondary symptoms. Dr Bramwell considered the case to be one of syphilitic gumma which irritated the motor centre for the right hand, and produced localized Jacksonian epilepsy. The reflexes were tested. The knee-jerk was absent on both sides. The plantar, cremasteric, and abdominal reflexes were all easily obtained. The co-ordination in the lower extremities, with the eyes closed and feet together, was normal. Thirty grains of iodide of potassium were prescribed, in a tumbler of water, three times a day, an hour before meals. On 13th March the patient felt very well. He had had no headache, was sleeping well; there had been no return of the spasms. He was told to continue the medicine for another month, and then present himself again.

NOTES OF CASES OF AN OUTBREAK OF SYPHILIS FOLLOWING ON TATTOOING.—In all 23 soldiers were tattooed by a man, S. (a discharged soldier of the regiment), who was a hawker in barracks. He was suffering from secondary syphilis, and the whole of the buccal mucous membrane was covered with mucous tubercles, while the soft and hard palate was ulcerated. During the operation he wetted his finger with his saliva and rubbed it on the wounds. Of the 23 men, 12 were inoculated with syphilis at the site of the tattoo marks. Of the remaining 11 who did not contract syphilis from the operation, 4 had suffered previously from syphilis; 2 deserted, who were said by their comrades to have had bad arms previous to desertion, and who most probably were also infected; while 5 escaped altogether, and who affirmed that S. did not use his saliva in any form or put his needles in his mouth. The incubation period varied from 13 to 87 days. The chancres, which were situated on the forearm, had prominent scabs, and when these fell off a plain, not an excavated base was left.—*The British Medical Journal,* 4th May 1889. By Surgeon F. R. Barker.

TREATMENT OF PERINEAL AND PERINEO-SCROTAL URINARY FISTULÆ.—The following lecture was delivered at the Necker Hospital by Professor Guyon, and reported by M. Joseph Récamier in the *Annales des Maladies des Organes Génito-Urinaires*, April 1889. There were in the wards three cases of perineal or scrotal urinary fistula, on all of which he had operated. The first was entirely cured and the two others nearly so. In all three the fistulæ were multiple, and opened at different parts of the perineum or scrotum. These cases were not alone peculiar on account of the multiple openings and the ramifications of the sinuses; but in all of them there was a hard perineal swelling the size of a Mandarin orange. It was not uncommon to see large infiltrations along the course of the fistulæ, caused by the penetration of small quantities of urine. The urine which penetrated the tissue did not always lead to its destruction, but might, on the contrary, produce hyperplasia. A specimen was here shown of the urethra of a young man, the perineal portion of which was entirely dissected out and perforated by numerous openings, while there was no attempt at a reparative process. It was exceptional to find lesions of the urethra arrive at this stage without any sign of new growth. In general the urethra was surrounded by much thickened tissue, which was demonstrated by other morbid specimens. It was in this way the tumours were constituted, of which the cases operated on afforded interesting examples. A fistulous tract is rarely direct, more often long and sinuous, and with the external opening at a considerable distance from the internal. Frequently several perineal openings were found, the fistula bifurcating in its course and dividing more or less often. It was possible to find several urethral orifices, but usually there was only one opening in the urethra. The opening by which urine escaped from the canal did not remain as a tear, but became organized, and the knowledge of this fact partly explained the difficulty experienced in getting old fistulæ to heal. An important peculiarity in the morbid anatomy of fistula was shown by specimens. When dissecting and following the course of a fistula a pouch was always met with, where the urine collected before it invaded the rest of the perineum. He laid stress upon this fact, as it was of great importance in treatment. Whatever was the cause of the fistula, whether it resulted from a urinary abscess or an infiltration of urine in a hard and thickened perineum, a pouch would always be found in close relationship to the urethra at the point where the urine began to drain away or where it originally collected. This central pouch—more or less distinct, but always present—was covered with a gray granular membrane, which had sometimes a fungous character. All the fistulous passages radiated from this central cavity. The pouch was the first lesion to determine the opening in the urethra; the fistulous tracts only became established secondarily. When a patient asked advice, it was the fistulæ alone which attracted attention, although the originating pouch still persisted, but reduced in size. When the perineum retained its normal consistency, the fistula might have a direct route from the internal to the external orifice without the presence of any pouch. A careful dissection was here shown illustrative of this condition. Turning to the clinical side of the question, he divided the types of cases into three distinct classes:—1. Cases with only one fistula and the perineum nearly normal. 2. Cases where, without a tumour, the parts were hard and tumefied, with one or more fistulous orifices. 3. Cases analogous to the condition of two of the patients, subjects of this lecture, where multiple discharging fistulæ existed with numerous hard and irregular proliferations round the fistulæ. The fistulous orifices were in general multiple in these complicated cases. The indications of treatment necessarily varied according to the class of case. When in the first class one or two fistulæ occurred in a perineum slightly indurated, the first care was to dilate the urethra to its normal calibre, either with bougies, or, if the stricture were too resistent, by internal section. In addition, the fistulous tract might be cauterized with the tincture of iodine or the galvano-cautery. In simple cases all that was required to insure a cure was to relieve the strictured canal. The urine became diverted from the fistulous tract as the urethra became more open for it to pass through. Pressure with the finger along the course of the fistula during micturition sometimes aided cicatrization. In the other two classes, when the tissues in the vicinity of the fistulæ had become indurated under the influence of the urine, a cure might, though rarely, be brought about by the same means. In making a careful examination the patient should be placed in the lithotomy position, and methodical pressure made over the perineum with the finger points. If urine or pus flowed to some extent from the openings it might be concluded that a central burrow existed, and that any treatment short of opening it would be useless; if no urine could be squeezed out, then it might be hoped that the fistula was direct and cauterization worth trying. Bonnet of Lyons, distinguished in the surgery of this century, misused this method of cauterization in urinary tumours. He stated that it was necessary to make eight or ten deep cauterizations to produce an effect, and sometimes he obtained a cure. This was probably the case when he penetrated as far as the central pouch, and where the urethra was protected by a thickened layer of tissue, but the practice was dangerous and uncertain. Bonnet went against the usual rule in the treatment of fistula by re-establishing the calibre of the canal only several days after cauterization. Professor Guyon did not hold the view that perineal section was of great value in the treatment of fistula. If the fistulæ were complex, this operation did not modify sufficiently either the course of the fistulæ or the central pouch. In whatever way the stricture might be treated, direct interference with the fistulæ was not to be overlooked. Strictures were often long and multiple, and situated in different parts of the canal, so that what perineal section could not effect was, on the contrary, quite within the power of internal urethrotomy. It was to it that he had recourse in his three cases of operation. This method allowed of a good-sized catheter being retained in the urethra after operation and to act as a guide during treatment. His practice was to make a median incision and to advance cautiously through the thickened tissue, separating the edges of the wound with sharp hooks, until the central cavity was reached. This done, the situation of the urethra was made out by its resemblance to a fibrous cord at the bottom of the wound and by the play of the catheter. He then cut out the fistulous tracts, paying special attention to the walls of the pouch. The loss of substance might be great, but that was soon made up by granulation. One of the cases was then examined twenty days after operation. A considerable amount of tissue was excised and the walls of the cavity cauterized, as he was not sure he could remove it. Cicatrization had so far advanced that only a small median fissure remained, which would soon be closed. Urine passed freely by the natural passage, and had almost ceased coming by the wound. The dressing after two days was almost dry. He recommended deep and free incisions, as the power of repair in the perineo-scrotal region was most remarkable. He attacked fistulæ and thickened tissue from without inwards. Then he penetrated to the central pouch, and was always sure in working outwards to remove all affected tissue. It was also generally necessary to destroy with the actual cautery the deepest portion of the cavity and to scrape away all fungoid growth, as on account of the close proximity of the part to the urethra it was impossible to excise it completely. When, instead of hyperplasia, there was destruction of tissue, the course of procedure must be varied according to circumstances, and the result was much less certain. Sometimes plastic operations on the perineum must be had recourse to, assisted by the retention of a catheter; but only on rare occasions was this mode of treatment required. In conclusion, he drew special attention to the preventive treatment of fistula. When an incision was made into an infiltration of urine or an urinous abscess always to go to the root of the matter, to the central pouch in contact with the urethra. A drain must be left in the wound until cicatrization was far advanced, and there was then a certainty that the patient would be completely cured without any risk of a consecutive fistula.

Part Fifth.

MEDICAL NEWS.

CANADIAN MEDICAL ASSOCIATION.—The twenty-second annual meeting of this Association will be held at Banff, N. W. T., on the 12th, 13th, and 14th of August next. The Canadian Pacific Railway Company has agreed to carry members and delegates with their wives or members of their families at the following rates:—From points in Ontario or Quebec to Banff and return at $95.00 each, including a double berth in sleeping car for each person, and meals in the dining cars on the way west from Montreal or Toronto and back, and four days living at the Banff Hotel. The special tickets issued by the Canadian Pacific

Railway to Banff and return will be good for 60 days, and the holders will be allowed stop over privileges on the Canadian Pacific line in either direction at pleasure. They will also be exchangeable at Port Arthur and Owen Sound, so as to enable members to travel in either direction by steamer between these points. Meal and berth coupons will be issued in connexion with these tickets, and will be available as part payment of expense of any who wish to make additional stops and spend longer time on the line. It is considered desirable, however, by the executive officers of the Association, that as far as possible the party should travel together by the all rail route as far as Banff, so that all may be present at the opening of the meeting. In addition to the members of the Canadian Medical Association, to whom this circular is specially addressed, a cordial invitation is hereby extended to all members of the regular profession in good standing in the Dominion of Canada, the United States, and Great Britain, to whom the necessary certificates will be sent on application to the Secretary. Members who intend to present papers at this meeting are requested to inform the Secretary, at as early a date as possible, of the subjects which they propose to bring forward.—JAMES BELL, M.D., *General Secretary*, 53 Union Avenue, Montreal.

ANTISEPTIC DRESSINGS.—In these days when strict antisepticism is so vitally necessary for success, it is well to get dressings accurately prepared—fresh, neat, and cheap. All these requisites seem admirably fulfilled by the dressings turned out by Mr John Milne from his great factory at Ladywell, London, E.C. One of his specialities is the manufacture of the field dressing devised by Surgeon-Major Bourke. The Alembroth wool, gauze, and bandages were first prepared by this factory.

OBITUARY.

At Viewfield House, Arbroath, on the 8th June, JOHN TRAILL, Esq., F.R.C.S. Edin., aged 80. Friends are requested to accept this (the only) intimation.

PUBLICATIONS RECEIVED.

H. DAVIES, M.D.,—The Mechanism of the Circulation of the Blood through Organically Diseased Hearts. H. K. Lewis, Lond., 1889.

T. S. DOWSE, M.D.,—Lectures on Massage and Electricity in the Treatment of Disease. John Wright & Co., Bristol, 1889.

F. FOX, M.D.,—Strathpeffer Spa: its Climate and Waters. H. K. Lewis, Lond., 1889.

WM. GOODELL, A.M., M.D.,—Lessons on Gynecology.

JOHN GORHAM,—Tooth Extraction. H. K Lewis, Lond., 1889.

M. CHARLES HENRY.—Cercle Chromatique. Charles Verdin, Paris, 1888.

M. CHARLES HENRY.—Rapporteur esthétique. G. Séguin, Paris, 1888.

GEO. JOHNSON, M.D.,—An Essay on Asphyxia. J. & A. Churchill, Lond., 1889.

WM. OSLER, M.D.,—The Cerebral Palsies of Children. H. K. Lewis, Lond., 1889.

J. F. PAYNE, M.D.,—Observations on some Rare Diseases of the Skin. Smith, Elder, & Co., Lond., 1889.

TOM ROBINSON, M.D.,—The Power of the Will. Gilbert & Rivington, Lond., 1889.

W. ROBINSON,—Cremation and Urn Burial. Cassell & Company. Lond. 1889.

Transactions of the American Orthopedic Association. Vol. I. 1889.

What will Trained Nurses gain by joining the British Nurses' Association? J. & A. Churchill, Lond., 1889.

24th Annual Report of the Sanitary Commissioner with the Government of India.

PERIODICALS RECEIVED.

Abstract and Index,—Nov., Dec.
Albany Medical Annals,—Jan.-April.
Alienist and Neurologist,—Jan.-April.
American Journal of Obstetrics,—Feb.-May.
American Journal of the Medical Sciences, —Feb.-June.
American Lancet,—Feb.-June.
American Practitioner and News,—Jan. 19-May 25.
Annales d'Oculistique,—Dec.-April.
Annales de Dermatologie, etc.,—Jan.-May.
Annales des Maladies des Organes Génito-urinaires.—Feb.-June.
Annales des Maladies de l'Oreille, etc.,—Feb.-May.
Annales de Thérapeutique Medico-Chirurgicales,—May, June.
Annales Medico-Chirurgicales,—Nov.-April.
Archives de Tocologie,—Feb.-May.
Archives Générales de Médecine,—Feb.-June.
Archives of Gynæcology,—Jan.-June.
Archives of Pediatrics,—Feb.-June.
Archivio della Riforma Medica,—Fasc. 1.
Asclepiad,—March, June.
Australasian Medical Gazette,—Jan.-April.
Berliner Klin. Wochenschrift,—Jan. 28-June 17.
Birmingham Medical Review,—Feb.-June.
Births, Deaths, and Marriages, Monthly Return of,—Jan.-April.
Do., Quarterly Return of,—Dec., March. Supplement to do.
Boston Medical and Surgical Journal,—Jan. 17-June 6.
Bristol Medico-Chirurgical Journal,—March.
British Journal of Dermatology,—Feb.-June.
British Gynæcological Journal,—Feb.-May.
British Medical Journal,—Feb. 2-June 29.
Brooklyn Medical Journal,—Jan.-June.
Bulletins et Mémoires de la Société Obstétricale et Gynécologique de Paris,—Jan.-May.
Bulletin Général de Thérapeutique,—Jan. 30-June 15.
Centralblatt für Gynäkologie,—Jan. 26-June 15.
Centralblatt für Chirurgie,—Jan. 26-June 15.
Centralblatt für Bacteriologie und Parasitenkunde,—Bd. 5, Nos. 5-32.
Chemist and Druggist,—Jan. 26-June 22.
Chicago Medical Journal and Examiner,—Feb.-June.
Dublin Journal of Medical Science,—Feb.-June.
France Médicale,—Jan. 29-June 20.
Gazette des Hôpitaux,—Jan. 31-June 20.
Gazette Médicale de Paris,—Feb. 2-June 15.
Gazzetta Medica di Torino,—Jan. 5-June 5.
Gazette Hebdomadaire des Sciences Médicales de Bordeaux,—Jan. 27-Feb. 10.
Glasgow Medical Journal,—Feb.-June.
Index Medicus,—Jan.-May.
Indian Medical Gazette,—Dec.-April.
International Journal of Surgery,—Jan.-May.
Jahrbuch für Kinderheilkunde,—Band 29, Heft 2-4.
Journal de Médecine de Bordeaux,—Jan. 27-Feb. 3.
Journal de Médecine de Paris,—Feb. 3-June 16.
Journal of Anatomy and Physiology,—April.
Journal of Cutaneous and Genito-Urinary Diseases,—Feb.-June.
Journal of Laryngology and Rhinology,—Feb.-June.
Journal of Mental Science,—April.
Journal of Nervous and Mental Disease,—Dec.-March.
Journal of the American Medical Association,—Jan. 12-May 25.
Journal of the Respiratory Organs,—Jan.
Journal of the British Dental Association,—Feb.-May.
Journal of the Scottish Meteorological Society,—No. 5.
Klinische Monatsbläter für Augenheilkunde, —Feb.-June.
L'Anomalo Gazzettino Antropologico, etc., —Feb.-April.
Liverpool Medico-Chirurgical Journal,—Jan.
London Medical Record,—April, May.
London Medical Recorder,—Feb., March.
Medical Chronicle,—Feb.-June.
Medical Press and Circular,—Jan. 30-June 19.
Medicina Contemporanea,—Nov. 15-Dec 31.
Montreal Medical Journal,—Feb.-May.
Morningside Mirror,—Jan., Feb.
New York Journal of the Respiratory Organs,—Feb.
New York Medical Journal.—Jan. 19-June 8.
Nordiskt Medicinskt Arkiv,—Band 20, Nos. 13-25.
Nouvelles Archives d'Obstétrique et de Gynécologie,—Jan.-April.
Occidental Medical Times,—Feb.-June.
Philadelphia Medical and Surgical Reporter, Jan. 19-May 25.
Philadelphia Medical News,—Jan. 12-June 8.
Philadelphia Medical Times,—Jan.-April.
Philadelphia Times and Register,—May 4-June 8.
Polyclinic,—Feb.-April.
Practitioner,—Feb.-June.
Proceedings of the Society for the Study of Inebriety,—Feb.-May.
Progrès Médical,—Jan. 26-June 15.
Progresso Medico,—Feb.-June.
Revue de Chirurgie,—Feb.-May.
Revue Générale de Clinique et de Thérapeutique,—Jan. 31-June 20.
St Louis Polyclinic,—April, May.
The Student,—Nos. 1, 2.
The Tocsin,—April, May.
Therapeutic Gazette,—Jan.-May.
Transactions of the Sei i Kwai,—Jan.-April.
Virchow's Archiv,—Feb.-June.
Weekblad van het Nederlandsch Tijdschrift Geneeskunde,—Jan.-June.

Part First.

ORIGINAL COMMUNICATIONS.

I.—THE PATHOLOGICAL IMPORTANCE OF THE BROAD LIGAMENTS.

By Lawson Tait, F.R.C.S., LL.D., etc., Professor of Gynæcology in Queen's College, Birmingham.

(*Continued from page* 14.)

The third cause of broad-ligament hæmatocele known to me is the rupture into the ligament cavity of a tubal pregnancy. Occasionally, as I have said, these hæmatoceles suppurate and form "pelvic abscess"—a term which I use strictly to define suppuration in the cavity of the broad ligament. Here, again, the pathological importance of this set of peritoneal folds is most evident. Suppuration on one side of the ligament has little damage to life, whilst on the other it is of the gravest possible consequence. In either case the abdomen should be opened without delay. Of suppuration in the broad ligament treated in this way I have had a large experience of a uniformly favourable kind, so that it is rapidly rising in professional favour.

My first contribution on this subject was made in a paper published in the seventy-third volume of the Transactions of the Royal Medical and Chirurgical Society of London, and contained a series of cases, six in number, in all of which suppuration had occurred, as far as I could ascertain, in hæmatoceles of the broad ligament. Up to the occurrence of the first case I had confined my treatment of pelvic suppuration to openings made from the vagina or in the neighbourhood of Poupart's ligament. Experience, however, had driven me to the conclusion of Dr Emmet, that "I cannot regard the introduction of the trocar into the inflamed tissues of the pelvis as a procedure free from danger under all circumstances." It is perfectly true that in very many cases where an abscess undoubtedly exists in the cellular tissue of the female pelvis, the fluid can be reached and removed by the needle of the aspirator. But, according to my experience, the relief obtained in this way is, in a large number of cases, neither complete nor permanent, and, in nearly all, the convalescence has occupied a time not at all commensurate with the extent of the lesion. This is quite as true of abscesses which have been allowed to open themselves, or have been assisted to open in the groin. They often continue as fistulous openings for years.

In many cases, even when the abscesses can be reached by vaginal puncture, the nature of their contents is such as to make their evacuation an impossibility; and I have seen several where punctures made at random through an indurated pelvic roof have missed the disease. In these cases the symptoms of the presence of pus were conclusive, but no indication of its seat could be obtained. Dr Emmet speaks of such in these words: "I can recall a number of cases which have been under my observation, with thickened tissues, where no treatment had the slightest effect, and, finally, they have passed into other hands."

The course of such abscesses is so thoroughly described by Dr West that his words cannot be improved upon, and therefore I give them at length:—

"When suppuration takes place the matter makes its way outwardly through the vagina, or through the intestinal canal, in almost all cases in which the inflammation is limited to the parts contained within the broad ligament. In those cases, however, in which the pelvic cellular tissue is implicated, the matter not unfrequently makes its way round between the muscles and the external surface of the peritoneum, and the abscess points and discharges itself through the abdominal walls somewhere in the course of Poupart's ligament, or a little below that situation.

"Though the size of the abscess is not in general very great, it not unfrequently passes into a chronic state, and emptying itself, for the most part, through some narrow passage of communication into the bowel, the patient continues for months or years liable to occasional discharges of pus per anum, the commencement of which dates back to some attack of inflammation of the cellular tissue years before."

In an instance quoted by Dr West, "occasional discharges of matter took place from the bowel, and pus was often intermixed with the fæces, five years after the first symptoms of inflammation of the cellular tissue about the uterus, the chronic results of which were still evident in a tumour which was closely connected with the rectum and the womb. These chronic abscesses generally contract, and the fistulous passages that lead to them become, by degrees, obliterated, but exceptions to this now and then occur, two of which have come under my own notice; and Sir J. Simpson has reported some very interesting cases where permanent fistulous communications have formed between the abscess succeeding to inflammation of the pelvic cellular tissue and the bladder, uterus, or intestinal canal.

In my own practice such disappointing cases formerly occurred with but too great frequency, and though I have had some successes by the employment of such means as the elastic ligature, (*Lancet*, June 27, 1874) and counter opening in the vagina (*Lancet*, April 3, 1875), yet the progress toward recovery has been so protracted as to contrast favourably only with those cases in which there was no recovery at all.

I had been, therefore, continually on the outlook for some means of dealing with such cases which would bring them as satisfactorily within our means of treatment as are collections of matter in most other parts of the body. This has been furnished by the wide, free, and successful application of abdominal section for the treatment of pelvic and abdominal tumours, and I am now able to give a list of thirty-eight cases, which includes the whole of my experience in this novel proceeding, and in which success has been obtained far surpassing anything I have yet seen or heard of. In this comparison I am, of course, excluding those cases where pointing of the abscess in the vagina is evident at an early stage of the case, but even in these the recovery has always been, in my experience, more protracted than in the six now to be narrated. In addition to this list there are about twenty others in which I have operated from above, but without opening the peritoneal cavity. These, of course, under the definition of abdominal section I have adopted, are not to be included in the present list; but in every one recovery and cure has been obtained. Fifty-eight consecutive recoveries, prompt and permanent, in such a grave condition constitute a result of a most satisfactory kind.

A patient was sent to me in February 1879, by Mr Gwinnett Sharp, of Walsall, suffering from a pelvic tumour, associated with very severe symptoms. She was twenty-two years of age, and had been married nine months. Her menstruation had always been too frequent and too profuse, and six weeks before I saw her it had stopped suddenly during its course, and this was associated with the onset of violent pelvic pain—one of the leading features of extra-peritoneal hæmatocele. A few days afterwards she shivered and became very ill and feverish, and these symptoms had become intensified when I saw her ten days after their occurrence. She was then emaciated and hectic-looking, with a high night temperature, intense pain and tenderness over the lower abdomen, and when examined, a large fluctuating tumour, adherent to and behind the uterus, and going on either side of it, was found to occupy the pelvis and rise about half-way up to the umbilicus. The roof of the pelvis was fixed and hard, and no fluctuation could be felt there.

The nature of the tumour could be open to only two suggestions: that it was a suppurating parovarian cyst with peritonitis, or a suppurating hæmatocele. I leant to the latter view, as it was in consonance with the history, and I have rarely known a parovarian cyst suppurate, whilst hæmatoceles frequently do.

In any case, I determined to open it from above, and this I did. I found a large cavity, containing about two pints of fetid pus with decomposing blood-clots. This I carefully cleansed out, and after having united the edges of the opening into the cyst carefully to the abdominal wound, I fixed in one of Kœberlé's glass drainage tubes, five inches long. Seven days after the operation I placed a three-inch glass drainage tube, and in another week this was replaced by a soft rubber tube. The patient got up on the twentieth day after the operation, and in ten days more went home perfectly well, with the abscess healed, and she remains now in perfect health (March 1880).

The second case was sent to me by Dr Flynn, formerly of Birchills, now of Kingstown, Dublin. She was forty-five years of age, and had never been pregnant, save one doubtful miscarriage soon after marriage, nineteen years ago. Symptoms resembling those of hæmatocele had occurred eight months before I saw her, and since that time she had been losing flesh, had lost her appetite, was troubled by constant thirst and night-sweats, and she had a rising night temperature. The uterus was fixed in a mass of effusion occupying the left broad ligament, and partly the right one also, and the mass on the left side encircled the rectum, forming a pronounced stricture, as hæmatoceles of the left broad ligament frequently do. No point of fluctuation could be felt in the pelvis, but the symptoms pointed clearly to the presence of pus. I therefore determined to open the abdomen, and readily obtained the consent of my colleague to this proceeding.

After reaching the peritoneum a large abscess was opened just behind the base of the bladder, between which and the uterus it principally lay, but stretching round behind the rectum. The floor and posterior wall of the abscess were found to consist of organized blood-clot, so that its origin was in a blood effusion into the broad ligament. A glass drainage tube was inserted, and this was changed for one of Chassaignac's wire tubes on the eleventh day after the operation. She sat up on the twenty-first day after the operation, and the tube was finally removed on the twenty-sixth. She went home on the thirtieth day perfectly well, and has remained so ever since, now nearly ten months.

The third case was a patient of Mr Hall-Wright's, in whom he had diagnosed hæmatocele some four weeks before I saw her. Symptoms of suppuration set in, and I performed exactly the same operation as before; that is, the peritoneum was opened, and the abscess emptied and cleansed, and then the edges of its opening fastened to the edges of the parietal wound, and a glass drainage tube fastened in. This case also was an undoubted hæmatocele of the broad ligament, and, I think, had its origin in a tubal pregnancy rupturing into the cavity of the broad ligament. Eight days after the operation the glass tube was changed for a wire one, and this was removed in twelve days more. She left the hospital perfectly well only thirty-three days after her admission, and has since remained in good health.

Mary Ann B——, aged thirty, has been married eight years, and has had four children, youngest fifteen months old. Seen first by Mr Hall-Wright on December 12th, when she stated that she had become unexpectedly unwell about five weeks ago, that this was accompanied by violent pain, which has never since been absent. She was seen by Mr Hall-Wright, who diagnosed the occurrence of hæmatocele from the presence of a large hard tumour behind the uterus. About a fortnight previous to my seeing her the nocturnal exaltation of temperature, night-sweats, thirst, and increased pain, led Mr Hall-Wright to suspect that the effusion was suppurating, and when I saw her I had no difficulty in confirming his opinion. I therefore admitted her to hospital, and on December 22nd I performed abdominal section, as I found the intensity of the symptoms increasing, and on examination under ether the mass of the effusion seemed too high up to be opened safely from the vagina. The tumour consisted of a large effusion of blood in process of disintegration, contained in a cavity formed by the lifting up of the posterior layer of the broad ligament, the rectum being carried up in front of it, together with the large vessels of both sides, as high as the bifurcation of the aorta, whilst anteriorly the peritoneum dipped to a most unusual depth, so that had I tapped the tumour from the vagina, I must have gone through the peritoneal cavity. The cyst was opened and emptied, and a drainage tube fastened in, and the peritoneum closed in the usual way. Her recovery was neither so easy nor so rapid as any one of the others, probably because the cavity was the largest of all, and her condition before the operation was very bad, though another case to be related was, in the latter respect, much worse. The temperature in the present case was 38°·4 before the operation, and it rose to 40° on the second day. It did not fall to 37° till the tenth day, December 31st. The drainage tube was removed on January 10th, and on the 17th the wound had perfectly healed, and she left the hospital on the 26th. I saw her on February 26th, when she was hardly to be recognised, so astonishing was her restoration to health. From a thin, emaciated, and apparently dying woman, she had been transformed into a perfect picture of health, and she stated that she was able to do her work and get about as well as ever she did in her life.

In this case I am quite satisfied that the delay of the operation for a few days would have been fatal, and no vaginal tapping, even if it had missed the peritoneal layers, would have emptied the cyst of its clotted contents.

Ann S——, aged twenty-eight, placed under my care in January 1882 by Dr Gordon of Walsall. She is the mother of three children, the youngest being three years of age. About four months ago she had symptoms resembling those of sudden effusion of blood into the broad ligament. For a month she was able to get about, but during the last three months she has been entirely confined to bed, the subject of symptoms clearly pointing to the occurrence of suppuration. Dr Gordon had discovered the presence of a pelvic mass behind the uterus, in which no fluctuation could be discovered, and which was fixed.

I admitted her into hospital, and, suspecting the case to be one of suppurating hæmatocele, I opened the abdomen on January 5th, and found the case to resemble the preceding one, save that the disintegrating effusion was not so large. It was dealt with in the same way, and the patient made a much more easy and rapid recovery, leaving the hospital on January 17th, and being restored to perfect health before the end of February.

Mrs H——, aged twenty-nine, was married at eighteen, had a child within the year, and has never been pregnant since. I saw her at the request of Dr Millington of Wolverhampton, under whose care she had been, in conjunction with Dr Blackford of Cannock, whom I met in consultation over the case on the 15th of January 1882. The history given to me was that about nine weeks previously, when driving in an open carriage with her husband on a very cold day, and during a menstrual period, she was suddenly attacked by a violent pelvic pain, and coincidently with this the discharge ceased. This pain had continued ever since, and had of late increased in severity. Menstruation had occurred at two irregular intervals since the beginning of her illness with great profuseness, and during these periods her pain had been much easier. A pelvic tumour had been discovered by Dr Millington some weeks before my visit, and this he had regarded as an effusion of blood. She had suffered for about three weeks before I saw her from night-sweats, almost constant sickness, utter loss of appetite, intense thirst, with various other symptoms of pronounced hectic. The tumour, when I examined it, involved all the pelvic organs in a fixed mass of cartilaginous hardness, with the uterus imbedded in it; the bladder spread over it in front, and the rectum encircled by a ring of hard effusion. The mass could be felt above the pelvis as a round and non-fluctuating tumour, with intestine in front of it. The patient had reached almost the final stage of exhaustion and emaciation. There was no difficulty in diagnosing the case as one of suppurating hæmatocele. With Dr Blackford's concurrence we had her removed to Birmingham, and on the 21st I opened the abdomen and found matters quite as I had anticipated. The posterior layer of the broad ligament was lifted completely up out of the pelvis, and so was the anterior layer, as far as I could make out; at least the only structure I could identify was the base of the bladder, and that seemed to form the anterior boundary of the tumour. From this point it spread backward, on a level with the brim of the true pelvis, and its posterior boundary was the bifurcation of the aorta. The contents were clearly fluid, and therefore I tapped it with an aspirator needle, and evacuated about half a quart bottle full of curdy blood-coloured pus. I then laid the cyst open from the point of puncture, in a direction from before backward, and found its floor to consist of a thick layer of laminated clot, hard and rigid. I could make out the uterus rising out of this mass, but I could not discover the rectum.

I stitched the edge of the opening into the abscess to the edges of the parietal wound, and then closed the rest of the peritoneal opening, and fastened in a wide drainage tube of glass. After the operation the patient's temperature never rose above 37°, she had no more night-sweats nor sickness, and her appetite was really keen on the third day. A small-sized wire drainage tube replaced the glass one on the twelfth day, as the discharge had become healthily purulent and free from clot débris. The smaller drainage tube was removed on the fifteenth day after the operation, and on the twenty-fourth the sinus was quite healed, she had gained greatly in flesh and colour, she was able to walk about, and on the twenty-seventh day she went home perfectly well, the uterus, however, being still quite fixed, as I expect it will remain for years. I have repeatedly seen this patient since the time of the operation, and her health remains perfect to this day.

These are the cases which I first published on this subject, and I have little need to give further details save in proof of the belief that I have that the last case and the third are examples of the suppuration of hæmatoceles due to the passage of a tubal pregnancy into the broad ligament. In the last case upon which I operated positive proof of this was obtained by the discovery of placental débris, and with the narrative of its details I shall close the subject.

E. C., married, aged forty, was sent to me by Dr Hindle of Leicester, who sent me a history to the effect that she had been under his care for fourteen days. The first symptoms were pain in the back and slight bearing-down pains; these were followed by retention of urine and severe pain. On examination per vaginam he found a hard growth distinctly felt extending unilaterally, which could be felt through the abdominal walls. She had been ailing slightly for about three months, and found that she had been getting a little bigger since August. She had been married twenty years, and had no children; she had menstruated perfectly regularly till the first week in December, since which time she had seen no discharge. On examination I found a large mass, with doubtful fluctuation, rising towards the right kidney, and reaching across on a somewhat lower level to the base of the left kidney. It blocked up the pelvis completely, the uterus being dragged up in front of it, but the cervix could not be reached by the finger; throughout the mass gave a sense of fluctuation, but not distinctly so.

I diagnosed that it was an effusion of blood, probably suppurating, in the broad ligament. I opened the abdomen on January 23rd, assisted by Dr Holford Walker, and before I could reach the abdomen I opened the bladder by mistake, the base of that viscus being raised almost to the level of the umbilicus. When the peritoneum was opened I came immediately upon the fundus of the uterus, and from behind this the tumour reached up as already described. Whilst endeavouring to ascertain its exact relations, and down between the rectum and uterus, my fingers suddenly passed into a cavity, out of which rushed a large quantity of purulent débris, occupied by masses of flocculent material. From this point I washed out the cavity thoroughly, but finding that it was impossible to drain outwards from this point I made an incision into the cyst wall—that is, the broad ligament—raised up from the peritoneal surface of the uterus, immediately behind the fundus, and from there I was enabled to remove a still larger quantity of purulent débris, one mass being of considerable size. I stitched this opening to the wound in the abdominal wall, and left in a large-sized drainage tube. The patient made an uninterrupted recovery. I changed the drainage tube for a smaller size on the 25th, and removed it completely on the 26th, all discharge having ceased, and the wound perfectly healed. The lower part reopened, and a urinary fistula was established; this slowly healed, and the patient went home convalescent on February 16th, the lower part of the wound being still slightly open.

Examination of some of the pieces of débris conclusively showed that it was broken-up placenta, and therefore there can be no doubt that this was an instance of ectopic pregnancy arising in the broad ligament, probably the right one, forming a large extra-peritoneal hæmatocele, and ending in suppuration. I think it quite likely that the case might have been dealt with satisfactorily by an incision through the vagina, but I do not think that in that way the mass of débris could have been got quit of as effectually as it was by supra-pubic operation.

In all of these cases I am satisfied that mere vaginal tapping would have been useless. In most of the cases, if the abscess had been opened by natural process, it would have been into the rectum. In the last case it would probably have been in one of the groins; but I think in every one of the cases, unless it be in the second, death would have occurred long before a natural outlet could have been established.

My general conclusion from these cases is that the opening of such abscesses by abdominal section is neither a difficult nor a dangerous operation; that recovery is made in this way more certain and rapid than in any other; and that in future I shall always advise an exploratory incision where I am satisfied there is an abscess which cannot be reached nor emptied satisfactorily from below.

Amongst the cases of parovarian cyst which I have dealt with I have met with one which is of a most exceptional character, because it contained dermoid elements, which I have never before seen outside an immediate association with the ovary; but in this case the cyst was embedded. I stripped it out of its peritoneal investment, and left the ovary intact. The operation was performed on December 15th last year on a patient aged 50, sent to me by Dr Thompson of Nottingham, and the removal of the tumour was an extremely difficult piece of work. The patient made a perfect recovery, and left my care on January 12th. For the following description of the tumour I am indebted to my assistant Mr Teichelmann.

"The cyst has been enucleated. It contained a large quantity (about twelve pints) of curdy fluid, and a mass of hair the size of two small fists matted together by a greasy sebaceous material of the consistence of butter. The hair was of a light reddish colour, and was composed of filaments varying from one to three inches in length. The cyst wall was fibrous in structure, and varied from $\frac{1}{20}$ to $\frac{1}{2}$ an inch in thickness. Its inner walls were lightly roughened and velvety in the greater part of its extent, showing under the microscope a thin layer of fattily degenerated granulation tissue on a fibrous ground. It showed here and there some light yellowish-brown patches, slightly elevated, and rough to the touch. These, which at first sight appeared to be the horny layer of the epidermis, proved, on further examination by the microscope, to be structureless cheesy material slightly impregnated with lime. The thickest part of the wall was composed of two mammillæ close together, each about the size of a large nut. They projected into the cyst, and from the summit of each grew several tufts of hair. On the surface they presented the appearance of ordinary scalp skin; on cutting into these they were found to be composed of a soft fat covered by skin similar to that on the scalp. On examining sections of these, their structure proved to be identical with that of the scalp, with the following differences:—The epidermis was thinner, the horny layer being only slightly represented, the sebaceous glands were large, multilocular, and altogether more highly developed. The sub-epidermal tissue was less fibrous, the papillæ few, and not well marked. The thickness of the fatty layer was about three-eighths of an inch in its thickest part, and only slightly fibrous. The fat was homogeneously distributed, and not arranged in loculi, as is usually the case in the scalp. The fibrous wall of the sac was indifferently supplied with blood-vessels."

Amongst the exceptional diseases of the broad ligament which I have met is a remarkable case of hydatid disease of the cavity in the case of a young married woman, aged 30, placed under my care by Dr Annie Clark. The operation was performed on March 19th, 1887. The diagnosis was made of a tumour fixed in the pelvis, and probably of the broad ligament, and when that capsule was opened several pints of the common echinococcus hydatid were removed. There can be no doubt at all as to the relations of the hydatid deposit; it was exclusively confined to the left broad ligament, pressing the uterus well over to the right, and occupying the whole of the pelvic cavity and a good deal of the abdomen. The opening into the cavity was secured by stitches to the abdominal wound, and a glass drainage tube inserted. For some days hydatids continued to be discharged. The drainage tube was removed on the tenth day after the operation, and on April 4th the patient went home perfectly recovered. She has remained since in perfect health.

As I have already said, I have removed three myomata, varying in size, from the broad ligament, tumours which had no kind of attachment to the uterus at all. One was at least eleven pounds in weight, and the others between two and three. They were all œdematous in precisely the same way as cysts, and in microscopical character presented no difference whatever from the ordinary soft œdematous myoma of the uterus. The last case occurred in a lady aged fifty-five, who was brought to me by Mr Hodgson Wright of Halifax. She had ceased to menstruate for seven years, and noticed a lump for the first time three years ago in the right side; this had speedily increased, and was removed at the operation on the 16th of July of last year, as I said, by enucleation; the stump of the capsule being closed by a serre-nœud as in hysterectomy. The following description of the tumour on subsequent examination was given to me by my assistant Mr Teichelmann:—"The tumour, which weighed just over 2 lbs., was attached by a broad œdematous pedicle to the broad ligament, close to, but distinctly separate from the left margin of the uterus. On section, a considerable amount of fluid drained away. The œdema spaces were smaller and more numerous, and there was less tendency to the formation of distinct cysts than in the uterine œdematous myomata."

This case emphasizes somewhat my belief that not only is œdematous myoma a special disease differing from the multinodular myoma, but that it exists independently of the menstrual function.

SERIES OF CASES OF CYSTS OF THE BROAD LIGAMENT.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.		
					1869.		
1	Wakefield . . .	Mr W. R. Milner .	45	M	Dec. 5	R	
					1872.		
2	Birmingham . .	Mr Hall-Wright .	32	M	May 8	R	
3	Wellington . .	Dr Marsden . . .	66	W	June 22	R	
4	Solihull . . .	Mr Lowe	28	S	Dec. 9		D
					1878.		
5	Bromyard . . .	Dr Etheridge . .	42	M	Jan. 19	R	
6	Birmingham . .	L. T.	22	M	May 25	R	
7	Preston . . .	Dr Moore . . .	23	S	Aug. 7	R	
8	Birmingham . .	Dr Evans . . .	49	M	,, 7	R	
9	Stonehouse . .	Dr Eshelby . . .	20	S	Sept. 2	R	
10	Birmingham . .	L. T.	27	M	,, 6	R	
					1879.		
11	Tamworth . .	Dr Fausset . . .	46	M	Mar. 28	R	
12	Wellington . .	Dr Macarthy . .	30	M	June 25	R	
13	Glasgow . . .	Dr M'Lachlan . .	28	M	July 8	R	
14	Dudley . . .	Mr Messiter . . .	30	S	,, 31	R	
15	Dudley . . .	Dr Higgs . . .	18	S	Aug. 9	R	
16	Dudley . . .	Dr Higgs . . .	39	M	,, 15	R	
17	Birmingham . .	L. T.	37	M	,, 18	R	
18	Moxley . . .	Dr Blackwood . .	40	M	,, 22	R	
19	Dudley . . .	Mr Steele . . .	36	M	Nov. 25	R	
20	Atherstone . .	Dr Handford . .	17	S	Dec. 1	R	
21	Birmingham . .	Mr Yates . . .	27	M	,, 10	R	
					1880.		
22	Kidderminster .	Dr Spofforth . . .	15	S	Jan. 14	R	
23	Birmingham . .	Dr Badger . . .	48	M	,, 27	R	
24	Birmingham . .	Mr Fairley . . .	15	S	April 1	R	
25	Birmingham . .	Dr Drummond . .	48	M	Oct. 6	R	
26	Hednesford . .	Dr Marsh Stiles .	41	S	Dec. 2	R	
					1881.		
27	Birmingham . .	L. T.	48	M	Aug. 8	R	
28	Worcester . . .	Dr Woodward . .	51	M	Sept. 12	R	
29	Llandulas . . .	Dr Turner . . .	48	M	,, 24	R	
					1882.		
30	Dudley . . .	L. T.	46	M	Jan. 11	R	
31	Birmingham . .	Dr Leech	28	M	Feb. 17	R	
32	Birmingham . .	Dr Drury . . .	37	M	Mar. 3	R	
33	Walsall . . .	Dr Oliver . . .	28	M	May 5	R	
34	Stonehouse . .	Dr Walters . . .	27	M	June 7	R	
35	Birmingham . .	L. T.	27	S	,, 24	R	
36	Bickenhill . .	Dr Quirke . . .	50	S	July 10	R	
37	Derby	Dr Boswell . . .	48	M	Sept. 20	R	
38	Birmingham . .	L. T.	26	M	Oct. 7	R	
39	Wolverhampton .	Dr Scott	44	M	Nov. 6	R	
40	Derby	Mr Legge . . .	53	M	,, 21	R	
41	Southampton . .	Dr Lake	39	M	Dec. 17	R	
					1883.		
42	Wolverhampton .	Dr Moore . . .	29	M	Jan. 28	R	
43	Birmingham . .	L. T.	27	S	Mar. 13	R	
44	Derby	Dr Copestake . .	32	W	,, 31	R	
45	Longton . . .	Mr Folker . . .	54	S	April 12	R	
46	Birmingham . .	Mr Freer	32	M	,, 14	R	
47	Birmingham . .	L. T.	54	M	,, 21	R	
48	Birmingham . .	Dr Hickinbotham .	43	M	,, 27	R	
49	Gloucester . .	Dr Ellis	40	M	,, 28	R	
50	Birmingham . .	L. T.	32	M	June 11	R	

SERIES OF CASES OF CYSTS OF THE BROAD LIGAMENT—*continued.*

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	
					1883.	
51	Coleshill	Dr Clark	28	M	July 2	R
52	Abergele	Dr Griffiths	32	M	,, 30	R
53	Kidderminster	Dr Jotham	55	S	Aug. 16	R
54	Walsall	Dr Hubbard	37	M	Nov. 4	R
55	Alderley	Dr Smith	58	S	,, 6	R
					1884.	
56	Birmingham	L. T.	42	M	Jan. 7	R
57	Oswestry	Dr Beresford	54	M	,, 19	R
58	Birmingham	Dr Craig	37	M	Mar. 12	R
59	Birmingham	L. T.	32	S	,, 13	R
60	Sutton	Dr Evans	18	S	,, 21	R
61	Nottingham	Mr Euan Smith	35	M	May 3	R
62	Llantrissant	Dr Davies	48	M	,, 26	R
63	Birmingham	Mr Whitcombe	34	S	,, 27	R
64	Coventry	Dr Fenton	29	S	July 3	R
65	Birmingham	Mr Harmar	29	M	Oct. 20	R
66	Birmingham	L. T.	25	S	,, 31	R
67	Birmingham	Dr Thomas	20	M	,, 31	R
					1885.	
68	Derby	Dr Taylor	40	M	Jan. 23	R
69	Birmingham	Dr Malins	25	M	May 15	R
70	Thirsk	Dr Hartley	53	M	July 14	R
71	Birmingham	Dr Shillitoe	40	M	,, 20	R
72	Birmingham	Dr C. Marriott	30	M	,, 23	R
73	Birmingham	L. T.	31	M	,, 25	R
74	Birmingham	Mr Hall-Wright	29	M	Aug. 25	R
75	Leicester	Dr Clifton	31	M	Sept. 18	R
76	Kidderminster	Dr Addenbroke	34	M	,, 28	R
					1886.	
77	Stroud	Dr Howsin	46	M	Jan. 15	R
78	Abergavenny	Mr Shillitoe	30	M	Feb. 23	R
79	Watham	Dr Emmerson	55	M	Mar. 17	R
80	Wolverhampton	Dr Lycett	38	S	June 9	R
81	Birmingham	L. T.	39	M	,, 16	R
82	Coventry	Dr MacVeagh	39	M	Oct. 13	R
83	Malvern	Dr Weir	29	M	,, 29	R
84	Birmingham	Mr Bracey	37	S	Nov. 29	R
85	Rugby	L. T.	53	S	Dec. 13	R
					1887.	
86	Hawick	Dr Calvert	37	M	Feb. 19	R
87	Kettering	Dr Hawkins	46	M	Mar. 7	R
88	Warrington	Dr Adams	22	S	July 15	R
89	Birmingham	L. T.	59	M	Aug. 20	R
					1888.	
90	Bridgnorth	L. T.	25	S	Jan. 15	R
91	Harrogate	Dr Souter	42	M	Mar. 5	R
92	London	Dr Sykes	41	M	May 31	R
93	Tipton	Mr Hicks	49	M	July 30	R
94	Great Grimsby	L. T.	35	M	Aug. 30	R
95	Bonau	L. T.	61	M	,, 31	D
96	Redditch	Dr C. Smith	22	S	Sept. 14	R
97	Whaley Bridge	Dr Stirling Anderson	26	M	Oct. 18	R
98	Leicester	Dr Bryan	22	M	Nov. 3	R
99	Birmingham	Drs Jones & Wilson	40	M	,, 28	R
100	Nottingham	Dr Thompson	50	M	Dec. 15	R
					1889.	
101	Cleckheaton	Dr Sykes	25	S	Jan. 11	R
102	Cadoxton	Mr G. Neale	34	M	,, 11	R

SERIES OF CASES OF PELVIC ABSCESS TREATED BY ABDOMINAL SECTION.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Date.	
1	Bromyard	Dr Etheridge	42	M	1878. Jan. 19	R
2	Walsall	Mr Gwinnet Sharp	22	M	1879. Feb. 3	R
3	Birchills	Dr Flynn	45	M	July 7	R
4	Birmingham	Mr Hall-Wright	37	M	Dec. 13	R
5	Birmingham	Mr Hall-Wright	30	M	,, 22	R
6	Walsall	Dr Gordon	28	M	1880. Jan. 5	R
7	Wolverhampton	Dr Millington and Dr Blackford	29	M	,, 21	R
8	Birmingham	L. T.	28	M	July 5	R
9	Birmingham	Dr Drummond	27	W	,, 10	R
10	Birmingham	Dr Kenny	17	S	Sept. 3	R
11	Wolverhampton	Dr Lycett	19	S	Nov. 11	R
12	Wolverhampton	Dr Totherick	11	S	1881. Jan. 7	R
13	Birmingham	Dr Drummond	27	W	,, 9	R
14	Birmingham	Dr Smith	33	M	Aug. 2	R
15	Birmingham	Dr Fairley	36	M	,, 17	R
16	Liverpool	Dr Macfie Campbell	22	S	Sept. 29	R
17	Birmingham	Dr Hickin	27	M	Nov. 12	R
18	Birmingham	Dr Newton	18	S	Dec. 30	R
19	Birmingham	Dr Taylor	32	M	1882. Jan. 13	R
20	Bilston	Dr Price	36	M	June 29	R
21	Coventry	Dr MacVeagh	26	M	July 28	R
22	Birmingham	Dr Edington	23	M	Oct. 22	R
23	Brierley Hill	Dr Ellis	29	M	Nov. 25	R
24	Birmingham	Dr H. C. Wilson	41	M	1883. Jan. 27	R
25	Birmingham	L. T.	37	M	Feb. 22	R
26	Birmingham	Dr Boddy	38	S	July 12	R
27	Coventry	Dr Fenton	15	S	1884. Mar. 6	R
28	Stourbridge	Dr Bailey	25	M	July 18	R
29	Birmingham	Dr Nicholls	29	S	Dec. 8	R
30	Walsall	Dr Somerville	26	M	1885. Nov. 1	R
31	Stourbridge	Dr Pearson	14	S	1886. Feb. 5	R
32	Cardiff	Dr Ross	26	M	April 12	R
33	Skipton	Dr Cresswell	29	M	1887. July 8	R
34	Birmingham	Dr Notley	31	M	1888. May 8	R
35	Birmingham	Dr Patrick	31	M	June 8	R
36	Birmingham	Dr Jones	18	M	Nov. 14	R
37	Leicester	Dr Hindle	40	M	1889. Jan. 23	R

II.—MITRAL STENOSIS AND LABOUR.

By D. Berry Hart, M.D., F.R.C.P.E., F.R.S.E., Lecturer on Midwifery, Surgeons' Hall, Edinburgh.

(*Read before the Edinburgh Obstetrical Society*, 8*th May* 1889.)

Since bringing before the Society in October 1887 a communication on Mitral Stenosis and the Third Stage of Labour, I have met with three additional cases. In two of these I obtained post-mortem examinations, with results confirming my previous conclusions, and demonstrating effectively the main cause of sudden death at the end of labour.

The first patient was admitted to the Maternity in a dying condition. The history of her case, for which I am indebted to Dr Eden, one of the residents, is as follows:—

"Mrs J., age 34, was admitted to Royal Maternity Hospital on 5th January at 4 p.m., on the recommendation of Dr Simpson of Leith.

"When first seen after admission, patient was sitting propped up in bed, in the attitude of complete orthopnœa, and labouring painfully for breath. The lips were markedly cyanotic, the hands cold, and the patient seemed restless and excited. The pulse at the wrist was rapid and feeble; respiration between 40 to 50 per minute; and so great was the dyspnœa that she could only with difficulty reply to questions put to her. She stated, however, that she was seven months pregnant with her first child, and had been married seven years. Three years ago she had rheumatic fever. Since the occurrence of pregnancy she has suffered from breathlessness, cough, swelling of feet, and increasing inability for all exertion. Latterly she had become rapidly worse.

"On examination of the heart a loud systolic murmur was heard at the aortic area, and a rough murmur was indistinctly heard in the mitral area. In the axilla the first sound was pure. The lungs presented loud crepitations everywhere, and some dulness was made out at the bases posteriorly. The abdomen showed a uterine tumour a little above the umbilicus. From the attitude of the patient the fœtal heart could not be heard.

"On vaginal examination the os was found to be undilated, and there had been no labour pains. The rectum was loaded, as also was the lower part of the colon; the bowels had not been moved for several days. The urine was very scanty, highly concentrated, and contained albumen. There was considerable œdema of feet and legs.

"An enema was given at once, and afterwards ℥ss. of Henry's solution with 15 ♏ tinct. belladonnæ. Every hour, ether and strophanthus were given by the mouth.

"The patient grew quieter after an hour or two of rest, and through the following day continued in much the same condition. On the evening of 6th January she became worse, and the lungs appeared to be more choked. She was therefore dry cupped in six places over the back. This produced some relief. About midnight, however, the nurse in charge became alarmed and summoned the house physician, who found her *in articulo mortis.*"

On post-mortem great œdema of the lungs with pleural and pericardial effusion was found, and the following was found to be the condition of the heart. For its examination I am indebted to the kindness of Dr Woodhead:—

"The *right auricle* was normal, and contained a large well-formed ante-mortem clot, which extended from the auricular appendage through the tricuspid valve and blocked the inferior vena-cava opening. Similarly the *right ventricle* was filled with a firm, decolorized clot, continuous with the one lying in the right auricle, as well as filling for a short distance the pulmonary artery. This clot had different degrees of consistency, and was exceptionally firm near the septum. There were only slight changes in the right ventricle, and these were confined to the muscular walls, the chamber being slightly dilated, and the muscular fibres only showing slight fatty changes.

"The *left auricle* was to a slight extent dilated and hypertrophied, but the *left ventricle* was markedly hypertrophied, and in it, lying next the septum, was a considerable quantity of firm, decolorized clot, and small recent clots in the rest of the cavity. At the inner angle of the mitral valve were a few small recent vegetations, and one or two at the opposite angle. The outer angle of the mitral valve was much thickened, and at the point where the papillary muscle joins the chordæ tendineæ, and again where the chordæ tendineæ join the margin of the cusps, was marked thickening. The angles of the valves were quite adherent, and the auriculo-ventricular opening slightly stenosed. The cusps of the *aortic opening* were bound together at each margin with quite recent vegetations, which almost filled the lumen of the aortic opening, and gave it, on looking from above, an irregular, egg-shaped appearance. The sinuses were well defined, but the valves were thickened and calcareous. In the aorta were some small ante-mortem clots. The cavity measured $4\frac{1}{4}$ inches vertically; the thickness of the wall was $\frac{3}{4}$–$\frac{1}{4}$ inch at the extreme apex.

"*Lungs.*—The lungs were congested and œdematous, the bronchial mucous membrane congested, and the glands at the root of the lungs congested and slightly pigmented. There was also a slight pleurisy.

"*Liver.*—The morbid changes in the liver were venous congestion, fatty infiltration and degeneration, with clots in the veins.

"*Kidneys.*—In the kidney were cicatrices from old infarcts. The capsule was slightly adherent; the cortex was thin and irregular; the epithelium in the tubules was full and apparently fatty, and the Malpighian tufts large and the organ congested.

The iodine reactions were negative." Microscopical examination of the mitral valve showed ulcerative endocarditis (simple) with no micro-organisms. In this case it is evident that the strain of pregnancy aggravated the heart condition, setting up fresh left-sided endocarditis, especially in the aortic segments. The aortic stenosis thus produced was of the most marked type, and as a result, we had an obstruction to the circulation causing failure of compensation, with pulmonary, pleural, and pericardial effusion. The result was exactly analogous to that of mitral stenosis, except that the resistance of the strong left ventricle had first to be overcome.

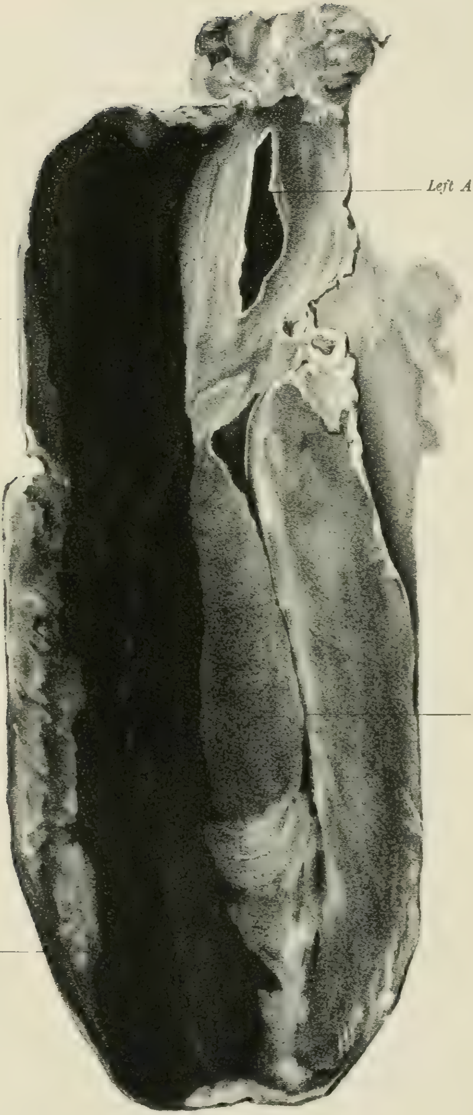
THE SECOND FATAL case has been already mentioned, regarding her second labour, in my previous paper, as "the worst case I ever had." During the puerperium of her second child she had free hæmoptysis, showing pulmonary congestion. In this, her third pregnancy, there was great œdema of the lower limbs and dyspnœa on exertion. The presystolic murmur was well marked and the heart's action irregular. Under the use of digitalis, which she had taken for several years intermittently and with great benefit, the œdema disappeared.

She fell in labour on the 2nd March, and I saw her at 9 A.M. The first stage was completed, and I delivered with forceps. In the third stage there was a fair amount of hæmorrhage with adherent placenta, as in her last confinement. I removed the placenta manually, but immediately on the completion of the separation she turned her head slightly to the left side, became unconscious, breathed with great rapidity, and died before I had time to do anything further than give a stimulant hypodermically.

Dr Bruce performed the post-mortem. On the thorax being examined after opening the chest, it could be seen that the right auricle was greatly distended. The bloodvessels of the heart were then tied, and the heart removed so as to ascertain the condition of the cavities so far as blood distension was concerned. The right auricle, as already said, was over-distended. The heart was frozen in the vertical position, with the result that the auricular distension diminished markedly. It was therefore thawed and refrozen with its left-sided aspect lower. It was then divided by a vertical saw-cut so as to expose both sides of the heart. Blood-clot was found in the right auricle and ventricle. The left side of the heart was completely empty. The mitral valve was extremely stenosed, admitting merely the tip of the little finger.

From what I have said as to the condition of the heart, it is quite a fair account to say that—(1.) On the completion of the third stage the right auricle became over-distended and the heart paralyzed. (2.) The dyspnœa was due to absence of blood in the lung, and the unconsciousness to anæmia of the brain from emptiness of the left side of the heart. It is possible that the right side became partly engorged during the second stage.

In the THIRD CASE seen by me the patient had a marked pre-



SECTION OF HEART WITH MITRAL STENOSIS IN CASE OF SUDDEN DEATH AT END OF LABOUR.

[illegible]

systolic murmur, and was pregnant for the second time. During the last six months of pregnancy she took tincture of strophanthus, 3 drops thrice daily, with no intermission. During all the time no symptoms referable to the heart appeared. She had an easy labour and recovered perfectly, except that on the second day of the puerperium there was facial paralysis, left-sided, for a few hours.

These cases seem to me to require little comment. The prognosis in such depends on two things, viz., the extent to which dilatation has advanced, and the endocarditis set up. When the failure in compensation has extended to the right side of the heart, weakening it, then the sudden strain from the excess of blood entering when the third stage is completed may prove fatal.

The extent of the dilatation of the right side may be judged of, during life, by percussion, the existence of venous pulsation and of hæmoptysis, and also, as Broadbent has more especially pointed out, by the nature of the murmur. When the first sound is not well heard, or has disappeared, this shows weakness of the left auricle; and when the murmur loses its intensity, this means, within certain limits, a weakening of the heart's action by dilatation of the left auricle and right side of the heart that bodes ill for labour.

Plate I. shows heart of fatal case of mitral stenosis cut, when frozen, so as to expose cavities.

BIBLIOGRAPHY.

BALFOUR, G. W.—*Diseases of the Heart.* Second edition. London: Churchill: 1882.

BROADBENT.—"Mitral Stenosis," *Int. Jour. Med. Sci.*, 1886.

HART, D. B.—"Mitral Stenosis and the Third Stage of Labour," *Edin. Med. Jour.*, Feb. 1888.

III.—DOES THE INCREASE OF CIVILISATION RENDER THE OCCURRENCE OF SEPSIS MORE COMMON?

By R. W. FELKIN, M.D., etc., Lecturer on Diseases of the Tropics and Climatology, Edinburgh.

(*Read before the Edinburgh Obstetrical Society, 12th June* 1889.)

I AM prepared for a good deal of criticism upon the paper which I lay before this Society to-night, for it does not deal directly with the science of obstetrics. Also I fear that you may perhaps consider that it has too little bearing upon practical every-day work. I therefore beg your indulgence, and at the same time request your criticism. The term civilisation, as used in this paper, is intended to include both the good and evil effects which result from the progress of races from a primitive to a cultivated state.

I must strictly limit the term "Sepsis" to its usual definition, "the probability of a patient who has undergone an operation, or of a woman who has been recently delivered, being attacked by a disease arising from putrefaction or fermentation." And I refer to those diseases which may find entrance into the body by either the subcutaneous or general connective tissue or through bone; such, for example, as septicæmia, pyæmia, puerperal fever, erysipelas, or gangrene. I must distinctly exclude all zymotic or exanthematous diseases, for with them this paper has nothing directly to do.

We are well aware that the various races of mankind at present existing differ in many ways; that, mentally and physically, marked contrasts are often observed, both racial and individual. There are also degrees in the powers of endurance, of fortitude, and of mental capacity, which are so well known as hardly to need mention, but for my purpose I must refer to some of these. Again, races differ very much in their susceptibility to certain diseases. For instance, Europeans suffer from yellow fever to a very much greater extent than do natives of the place where the fever is endemic, and the greater the distance of a man's birthplace north of the latitude infested by yellow fever, the more likely he is to be attacked by it should he enter the yellow fever zone. Of course, in this connexion, the climate of the previous residence of the individual must be taken into account, but it is evident that climate influences races. Pure-blooded Negro races and persons of the Mongolian stock do not suffer from yellow fever. To show how fatal this disease is to Europeans, the statistics of deaths per thousand which occurred in the New Orleans epidemic in 1853 showed that only 3·58 natives died per thousand, whereas 328·94 per thousand strangers from Holland succumbed. Again, malaria attacks people of the Caucasian stock far more frequently and severely than, for example, Negroes. It seems certain that there is something, be it what it may, which prevents the yellow fever or malarial poison from easily affecting individuals of certain races. It is the same with scarlet fever, which prevails to a great extent in Europe, and is hardly known in Asia or Africa. Measles, on the other hand, is far more fatal among dark races than among white, while syphilis affects whites far more seriously than blacks or Chinese. Even in the lower animal world marked differences in susceptibility to disease exist, some being perfectly safe from diseases which would kill others, a familiar fact to all who work in bacteriological laboratories, *e.g.*, anthrax will not affect some kinds of rats, nor old dogs, nor can it kill birds, although most rodents and young dogs are obnoxious to it.

Let me now refer for a few moments to wounds, to the occurrence of traumatic erysipelas and fevers following wounds, and the prevalence of suppuration, for we shall find that in this department also remarkable differences exist.

An instance of the non-susceptibility of semi-cultivated people to erysipelas and pyæmia is mentioned by Pirogoff.[1] He there writes, "The most successful results of my surgical practice were won in the country in Podolia (South Russia). In several hundred severe operations I did not observe a single case of erysipelas or suppuration, and I only lost one of my cases, and most of the cases operated upon were without careful after-treatment. The cases were quartered in the common sitting and bedrooms together with the peasants, and the patients did not in any way belong to the peasants' families. They were mostly strangers . . . . . most of them wore for weeks together clothes soiled with blood and matter . . . . . When I remember further that if these cases had been treated in a cleanly hospital they would mostly have been followed by erysipelas or pyæmia, I can only to some extent explain my success by the fact that I operated in the country, not in one room, for all my patients dwelt separately." Now these people were only partially civilized, as they belonged to the lowest class of the Russian population.

Looking back to prehistoric times, we find it almost certainly proved that grave operations were performed by primitive men, and, according to Broca[2] and others, there is no reason to doubt that trephining was practised when the people could only use flints with which to perform the operation. This is the more easily believed, because, even at the present time, in the South Sea Islands trephining is in vogue for headache and neuralgia, and bone-scraping is employed in cases of rheumatism in old people. The Rev. Samuel Ella writes as follows[3]:—

"A notion prevails there that headache, neuralgia, vertigo, and other cerebral affections, proceed from a crack in the head or pressure of the skull on the brain. The remedy is to lay open the scalp with a cross or T-shaped incision, then scrape the cranium carefully and gently with a piece of glass until a hole is made into the skull down to the dura mater about the size of a crown-piece. Sometimes this scraping operation will be even to the pia mater by an unskilful surgeon, or from the impatience of the friends, and death is the consequence. In the best hands, about half of those who undergo the operation die from it; yet this barbarous custom, from superstition or from fashion, has been so prevalent that very few of the male adults are without this hole in the cranium, or 'have a shingle loose,' to use an Australian phrase. . . . . Formerly the trephine was simply a shark's tooth; now a piece of broken glass is found more suitable, or less objectionable, if we may even so qualify the act. Bone-scraping is employed in cases of rheumatism in old people. The cuticle is incised longitudinally, and the centre of the ulna or tibia laid bare; then the surface of the bone is scraped with glass until a large portion of the external lamina is removed." Mr Ella goes on to say, "I never found one who had undergone the operation that said it had been efficacious in the object sought. They were still rheumatics, and some suffering great additional torture from the adhesion of the skin to the bone in the progress of cicatrization."

---

[1] *Grundzüge der allgemeinen Kriegschirurgie* (Leipsig, 1864).

[2] *Bul. de la Société d'Anthrop. de Paris*, 1874, p. 542; 1876, pp. 236, 431, 555, 573. *Langenbeck's Archiv*, vol. xxviii. p. 775.

[3] *Medical Times and Gazette*, vol. i. p. 50, 1874.

There can be no wonder at the fatal results in the operations on the skull above mentioned; but, with regard to the second operation, it does seem remarkable that we have no notice of any septic infection taking place, and practically we may say that in this country few surgeons would have such a large proportion of successful cases in trephining.

It is not my purpose to give very numerous descriptions of operations or practices which one would expect to be followed by septic infection. It will only be necessary to refer briefly to a sufficient number of instances to illustrate my point. Until the general practice of antiseptics was adopted, wounds of bones were greatly feared, and all the greatest surgeons, it may be said, were in favour of amputation in most cases of compound fracture of the extremities. Yet I think I may safely state that all observers agree that such wounds are not followed in uncivilized countries with the fatal results so frequently seen in Europe. At any rate, personal observation assures me that such is the case in Africa.

Looking at ovariotomy, we all know the care which is taken in performing the operation in Europe, but in Australia[1] ovariotomy is performed with rough stone knives, and, although statistics are wanting, yet we know it is often successfully performed.

In Central Africa I was much struck by the fact that the mortality following castration was very low indeed when the operation was performed upon boys under ten, although it was done in a very rough-and-ready manner, and the wounds were dressed most carelessly.

Again, the extensive practice of the removal of the clitoris and labia minora, and the sewing up of the women, which obtains so widely throughout Central Africa,[2] is very rarely followed by injurious results, although I must say I have seen cases of septicæmia following it.

It is very remarkable that African women are able to undergo the amount of surgical interference they do, for, in all cases before a child is born, the perineum is incised, and some time after delivery an operation is performed to repair the perineum, and the same procedure is undergone at each subsequent delivery. It is surprising, too, to notice with what impunity abortion is brought about by mechanical means amongst uncivilized races, and in this connexion it is interesting to notice that deaths far more frequently take place from this cause in Japan, China, and India, and yet still more frequently in Turkey, than among the primitive races.

---

[1] Roberts—MacGillivray.

[2] *Das Weib in der Natur und Volkerkunde*, second edition, 1887.

It is noteworthy that the cupping which is extensively practised among uncivilized races leads to no bad consequences. I show you a knife which was used in Central Africa for the purpose. It is exactly in the state in which it was used, and yet no inflammation or suppuration followed.

I was often interested to notice that in the Soudan the extensive sores caused by fetters or the slave-yoke were very rarely followed by septic diseases. You would hardly believe, unless you saw the extent of these wounds and the filthy condition of the unfortunate people, that it would be possible for them to escape blood-poisoning, and yet such is the fact.

I would here remark the difference noticed in Europeans and Negroes in regard to wounds, when the European is in a tropical climate.[1] I have observed again and again that Europeans suffer severely in the tropics from the slightest wound, such as a prick or a scratch, the natives being completely unaffected in general health by such trivial injuries. This may be, and undoubtedly is, due, to a great extent, to the condition of anæmia or malarial cachexia from which the Europeans are suffering at the time, but which is not, in my opinion, sufficient to cause the excessive ulceration, suppuration, or even pyæmia. The explanation of the matter, to my mind, is that the Negro can resist septic virus and the European cannot.

I must not omit to mention parturition; and if we briefly review the difficulties and dangers due to labour, we find that marked differences exist in various races. Although I personally believe that many observers have been too credulous in thinking that parturition is invariably easy amongst uncivilized tribes, yet there is good ground for many statements met with on the subject. Some of you may remember a paper I read a few years ago before this Society on "Labour in Central Africa," and I then said that easy labour was by no means of certain occurrence, but at the same time none will deny that in primitive races less difficulty is experienced and less serious after-effects follow parturition than in Europe. Puerperal fever is almost unknown. Peritonitis is extremely rare, and, although a labour may have been exceedingly severe or operative interference simply barbarous, yet in the majority of cases recovery is very rapid, and is uncomplicated by drawbacks and accidents so often experienced here.

The easy labours are doubtless accounted for by the normal development of the skeleton and by the well-developed and healthy muscles which do so much to render labour easy, and the lessened nervous irritability plays an important part in preventing untoward symptoms; but to this nervous influence I refer elsewhere. Looking at parturition as a physiological act, it follows that the women who have not had their frames and constitutions modified and weakened by civilisation stand a far better chance than their highly cultivated, luxurious sisters. But, apart from this, I believe that difficult or easy labours are determined far more by the environment, habits, and customs of the people than by mere racial peculiarities. An active out-door life, fresh air, moderate food, early hours, the absence of corsets, etc., all tend to the advantage of the uncivilized women.

---

[1] *Lie Leiden des Europäers im Afrikanishen Tropenklima.* Leipzig, 1887. *Surgical Diseases of the White and Coloured Races compared.* Tiffany.

One cause of the absence of septic infection after labour in primitive races is the absence of many of the channels by which septic material in civilized countries is conveyed to the patient. Vaginal examinations during labour are rarely, if ever, practised; clothing is not so likely to carry infection; the attendants of the parturient woman do not likewise attend infectious diseases, nor do they come in contact with surgical procedure, nor make or attend post-mortem examinations; and, more than all, the patients are isolated, for we do not find lying-in hospitals in vogue among primitive peoples. I do not forget the strict seclusion in Indian zenanas, nor the insanitary condition of large Moslem cities, for these circumstances go to prove my point. The people are more civilized, and we find that sepsis is more common amongst them, but still not nearly so rife as in Europe. These last remarks must not be misunderstood. I know that in many large European hospitals antiseptics have reduced the death-rate after labour from septic causes to 1 or less than 1 per cent., but as yet this can hardly be said for the general run of cases, and, even were it so, it is only due to the advance in science compensating for the increased liability to the septic influences from which our people suffer. I take it that the liability to sepsis, which we find in an ascending scale from primitive to highly civilized people, is perhaps best illustrated by the lying-in room experiences of the various races.

In connexion with the differences which exist in liability to disease and to septic infection, I would refer to the differences observed in the amount of suffering caused by disease and injury. There is no doubt that the appreciation of pain is not the same among all peoples, and that the pain caused by a specific act, such as a cut or a stab, is not felt as acutely by some as by others. This is realized very forcibly by those who are brought into contact with Eastern or semi-civilized and barbarous races, and who find with astonishment that operations which would be considered a great ordeal in Europe are not thought much of by Negroes or Arabs.

Let me draw your attention for a moment to the enormous amount of physical injury which the people of an Eastern race will endure for religious motives or in obedience to tribal customs. And it must be remembered that the suffering thus self-inflicted seldom causes any permanent injury. Wounds which, in their after consequences, would scarcely fail to be fatal to Europeans, or which would at least cause lingering and severe illness, heal perfectly well and rapidly, and often with little help save Nature's simple remedies.

I have frequently seen wounds heal which I expected to prove fatal, and I suppose that a like experience has been met with by all observers who have been brought into close contact with such races as I have mentioned. I will give you one or two cases which came under my own personal notice.

When in the Bahr-el-Ghazal I saw a man who had a spear driven into his back—"transfixed" was the word Gessi Pasha used when referring to him. His companions were afraid to withdraw the spear, but the Pasha, knowing that death must result if nothing were done, drew out the spear, upon which a gush of blood followed and the man fainted, when all bleeding stopped. Hot dregs of coffee, the favourite hæmostatic in that part of the world, were poured into the wound, and the chest was simply bandaged with bark cloth, a bit of banana leaf being the only dressing employed. The same day the man had to march for four or five miles, but when I saw him he was apparently quite well, although certainly the right lung, the injured one, did not expand properly, and little air entered it. Evidently considerable collapse had taken place.[1]

Some days after seeing this case I came across another remarkable instance of what a native can undergo and live through. A woman was walking along when a man tried to steal her cloth. She resisted, and he thereupon slashed her abdomen with his knife. The abdominal wall was severed from 2 inches above the umbilicus in the middle line downwards and outwards for nearly 7 inches, and the intestines protruded. The woman lost consciousness, and was found by some soldiers lying in the path. They tied a turban round her waist, and carried her a distance of three hours' journey to Gessi Pasha's headquarters, where I saw her, and she was then calm and collected. Temperature, 100°; pulse, 96. I washed the intestines and sutured the wound. She varied for a few days, but ultimately completely recovered.

I recently heard of a very similar case. When Bishop Hannington was killed near the Victoria Nyanza, most of his men were killed too. One boy, however, after receiving a gash in his side, from which his intestines protruded, was left for dead; at night the cold revived him, and he escaped after binding a banana leaf over his wound to keep his intestines in place. He reached a village where he was cared for, and he recovered and marched to the coast.

Such horrors show what people in various parts of the world can undergo.[2] Some voluntarily submit to mutilations and ordeals, and others to barbarous customs or the results of war; yet we find that as a rule these horrible injuries are borne and recovered from with remarkable ease, and without the grave consequences which would follow in more civilized lands; at any rate, if treated in the rough and ready style in vogue in all the places I have referred to. Antiseptics are there unknown; care for the wounded is mostly carried out in a hap-hazard fashion; in many places the wounded must take care of themselves, and yet deaths from septic causes are rare, not to say unknown. We must, I think, come to the conclusion that for some reason or other these rude children of Nature are possessed of most remarkable constitutions, or else their upbringing and their surroundings must protect them from the entrance or the power of septic influences to which their, in many ways, better cared-for brethren in more civilized lands are liable. How is this? Wherein does the secret lie?

---

[1] *Uganda and the Egyptian Soudan.* See also paper by Dr Max Bartels in *Zeitschrift für Ethnologie*, vol. xx. 1888, p. 169.

[2] Some of the examples read at the meeting are omitted to save space.

Must we not arrive at the conclusion that civilisation and culture do really increase our susceptibility to suffering; but, assuming that our physical and moral courage is not so degenerate as to account for this, we must look further for the reasons.

In the first place, we must take into account the fact that the tactile sensation of some uncivilized races, at least, is less than that of Europeans. I may here just refer to a paper I read a few weeks ago at the Pathological Club. It was an attempt to prove that the sense of feeling is less keen in Arabs and Negroes than in Europeans.

When I first operated on Arabs and Negroes, I found that both races stood pain remarkably well, and that there was less need of anæsthetics than if I had been operating on white people. The Arabs underwent operations with extreme fortitude, yet that fortitude appeared to me to be a disregard of pain induced by custom and training, and due also to peculiar mental characteristics, which one may express by the terms self-control, self-repression, or dogged endurance. With the Negroes, however, the cause seemed to be a different one. They appeared to have hardly any dread of pain, and I was determined to find a reason for this if I could. I therefore tried by a series of observations to ascertain whether the general sensibility of the Negro was equal to or less than that of the average European, and I found that the relative tactile sensibility of Europeans, Arabs, and Negroes was roughly in the proportion of 1, 2, and 3. I also discovered later that after education the acuteness of touch in Negroes was increased by one-third.

I do not, however, think that mere feeling or sensation is the only factor in the ability or non-ability of people to bear pain.

May it not be possible that, psychologically blunt as their perceptions are, natives are incapable of those mental conditions and impressions which so intensify pain, and that they thus have less consciousness of bodily suffering? Differences are met with in our own country in this respect between those born and reared in towns, and those whose ancestors have lived in the country and who have themselves been brought up there. The latter are stronger in muscle and vigorous, and they have less highly strung nerves; their brains work less rapidly, but they can endure more physical work than the hothouse plants of our cities. Remember that pain has no constant value, and that the degree of pain in a case depends not only on the potency of the cause, but on the susceptibility of the sufferer. Suffering may be due to over-sensitive nerves and weak muscles. I do not mean to a weak nervous system. Herman says, "a weak nervous system is also over-sensitive, but a nervous system more sensitive than the average is not necessarily a weak one; indeed, often the reverse, for with highly intellectual powers there is often very great sensitiveness.[1]"

Art has so conserved life that existence among us is in some cases rendered possible by entirely artificial means. But in native tribes the weakly die or are killed soon after birth, and so, the strong alone remaining, the race becomes hardy, a true survival of the fittest. It is only to be expected that these, whom one might poetically call "the flowers of the flock," should be able, on account of their physical strength, to resist morbific agents better than the more highly cultured Western races.

Civilisation, among other things, teaches us that man must be clothed as well as "in his right mind," whereas exposure to the fierce warfare of the elements has rendered the children of Nature hardy, as in everyday language we call them, thus popularly allowing this very truth, that they are more *hard* than we.

We are not abusing the laws of culture, but if, on the one hand, civilisation does refine and add bitterly to our suffering, we may also look to her to discover the remedy, and, by giving every help towards the advance of science and the fulfilment of natural laws, she may multiply those means of alleviation which have already diminished suffering and in some ways counteracted our capacity to feel pain.

I must now return to the main subject of my paper, the prevalence or otherwise of sepsis among different peoples.

I have alluded to the fact that native races endure wounds and operations with little, if any, mortality resulting from septic conditions, and this notwithstanding, in many cases, the insanitary surroundings of their immediate environment. How was it, then, that the insanitary condition of persons, houses, and hospitals among Europeans a few generations ago, and indeed in many places up to the present time, appear to have caused such immense mortality after wounds and operations from the attacks of morbific microbes or sepsis?

To my thinking, it was not so much the want of sanitary arrangements that caused the mortality, although of course that played its part, as the fact that civilisation had increased nervous irritability and lowered bodily stamina amongst the European races, whereas the primitive peoples of whom we are speaking are hardy in the widest sense of the term. I do not think this argument overthrown by the fact that the ravages of sepsis have decreased among us in these days of increased civilisation, and therefore of lessened resistance, for this decrease is owing to the use of anæsthetics and the employment of antiseptic precautions. The anæsthetics remove fear, and to a great extent shock from an operation, whilst antiseptics prevent suppuration and also pain in the after-treatment of wounds.

---

[1] *Brit. Med. Journal*, p. 1218, June 1, 1889.—G. E. Herman.

It will probably be allowed that sepsis, under whatever guise it manifests itself, is induced either by the introduction into the body of a morbific microbe and its action within the body, or that it is due to the chemical products produced by a morbific microbe in the body it has invaded, and we must bear in mind that every natural object, animate or inanimate, has its own laws or properties of existence, according to which it invariably tends to act, and every phenomenon of Nature arises from some cause or combination of causes, which it invariably and unconditionally tends to follow.

We know from Pasteur's[1] experiments that a microbe can be rendered either more virulent or less so, and we further learn that an animal's resistance to the inroads of a microbe may vary owing to surroundings, habits, etc. So, too, I think it is with man.

It will no doubt be readily admitted that in order for a morbific microbe to do harm two things are necessary. The patient must be capable of being affected by the microbe, and the microbe must find a suitable soil in which to develop.

We have seen that not every race or every individual is liable to be affected, and it may well be that accidental circumstances alone will permit a microbe to do harm. If a healthy rabbit be inoculated with the virus of "quarter evil," for instance, it will be refractory to the disease, but if a wound or a blow be inflicted upon it, and the wounded or bruised part be inoculated, an anthrax tumour will soon appear. By the wound or blow the vitality of the rabbit is reduced, and in the bruised tissue a fitting soil is produced for the microbe's development.

It seems to me that civilisation produces in man these two very conditions:—(1.) Man in his state of wild nature has a different constitution from man living under conditions of high culture. This high culture induces nervous impressions which lower vital energy, and an artificial environment lessens corporeal resistance. Thus civilized men become more prone to the inroads of septic diseases, which can be withstood by lower races successfully. (2.) It is quite possible that the power to eliminate poisonous products is greater in uncivilized than in civilized races. At any rate it is certain that there are "individual peculiarities": (*a*) in the composition of the blood and its relation to tissue change; and (*b*) in the activity of tissue change (metabolism), coupled with free access to fresh air (complete oxygenation), which give the lower races an advantage over the higher, the latter having become through their inherited civilisation the unfortunate possessors of a fitting soil for septic infection.

---

[1] See "Croonian Lecture," *Brit. Med. Journal*, vol. i., 1889, p. 1269.

Another suggestion might be thrown out, but I do not think it would hold good. It is well known that persons constantly employed in a post-mortem or dissecting room will not suffer so easily from blood-poisoning as a person newly introduced into such a place. Can it be possible, then, that uncivilized and semi-civilized races are so acclimatized and inoculated with septic material that they are as refractive to the septic poison as are the dogs M. Pasteur has rendered insusceptible to rabies?

Or again, are we to imagine that they possess an hereditary power of resistance to diseases which play such dire havoc among civilized races?

One thing is, I think, at any rate certain. We can put climatic action out of account, for in all climates—hot, temperate, and cold—there are races which are obnoxious to septic disease in every form.

I must now hasten to bring these few ideas to an end, and I shall probably do so best by giving a short summary of my paper.

I have referred to the following points:—

(1.) That races vary in different parts of the world.

a. As regards their mental and physical characteristics.

b. In their liability to suffer from various diseases.

c. In the effects which wounds exert upon them.

d. In their power of bearing pain.

(2.) That civilisation, by tending to make a race less hardy, increases sensitiveness, both to pain on the one hand, and to the inroad of morbific microbes on the other, and that therefore (3) civilisation does certainly increase the liability of a race to the power of sepsis, but that (4) fortunately science, by the strides it makes, tends to bid defiance to septic diseases, although they are rendered more potent by the advance of nations from a lower to a higher plane.

IV.—THE SURGICAL TREATMENT OF EMPYEMA.

By ALBERT E. MORISON, M.B., C.M., M.R.C.S., Hartlepool.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 5*th June* 1889.)

THE following cases of empyema have occurred during the last six years in my brother's and my practice, and I am indebted to him for notes of many of the cases, and for kind advice and assistance in difficult cases.

CASE I.—A. C., æt. 6½ years, had scarlet fever ten months ago, from which he never satisfactorily convalesced. He has steadily lost flesh, increased in weakness, and been troubled with a severe cough, profuse purulent expectoration, night sweats, occasional vomiting and diarrhœa, and loss of appetite.

When brought to me on 12th January 1882, he was extremely emaciated, and appeared to be in the last stage of consumption, which opinion had been given by his regular medical attendant.

Physical Examination.—I found the usual signs of fluid at the base of the right lung, but no sign of disease elsewhere. From the amount of pure pus (about ℥x.) expectorated every day, together with the physical signs, it was evident that a fistulous opening existed in the lung.

On 14th January the chest-wall was incised, and 7 oz. of pus evacuated. He gradually lost all his old symptoms, and in seven weeks was quite well and the chest healed.

CASE II.—J. W., æt. 8 years. His illness commenced suddenly six weeks ago, with what his medical attendant said was pneumonia. From this attack he gradually recovered, but the cough remained. He continued to get thinner, had difficulty in breathing, severe fits of coughing, and sweated profusely, especially at nights.

On examining the chest I found signs of fluid at the right base, and on 12th February 1882 removed by aspiration 12 oz. of pus. At first he was entirely relieved, but by the fourth day his difficulty in breathing had in great part returned, and fluid was reaccumulating in the chest. After this he gradually recovered, and by the middle of April was quite well. Syr. ferri iodid. in ʒj. doses t. d. was given.

CASE III.—C. S., æt. 5 years, said to have had bronchitis for seven weeks, which is getting worse. His chief symptoms were shortness of breath, amounting to orthopnœa, severe spasmodic fits of coughing, emaciation, and sweating. Signs of fluid in the left side were discovered, the heart being displaced considerably to the right.

On 16th June 1882 I aspirated his chest and obtained 17 oz. of pus. He was at first considerably relieved, but on 21st June, all his symptoms having increased and the difficulty in breathing being urgent, I incised the chest and 1 pint of pus escaped. His condition while under chloroform was unsatisfactory till the incision was made, but as the pus escaped it improved. By 22nd July he was cured.

CASE IV.—G. B., æt. 43. Has had a cavity at his left apex for some years; seven weeks ago an attack of acute pneumonia at the left base. His present complaints are of a cough, with profuse expectoration, shortness of breath, and loss of flesh. From being a stout man he has been reduced to a very thin one. The cough, he says, comes on at intervals, and at these times a large quantity of pus is expelled by it. Relief is obtained for a time, and then the coughing and expectoration are repeated. Physical examination shows that the apical cavity is 4 inches in extent, that posteriorly and laterally the left base up to the lower angle of the scapula is dull on percussion, that the vocal fremitus is unaltered, and that crepitation and increased vocal resonance can be heard over the dull area. The aspirator needle, introduced on 23rd June 1882 between the sixth and seventh ribs posteriorly, proved the presence of pus. The chest was incised at this spot, and 9 oz. of pus escaped. The immediate result was a great improvement in all his symptoms. The tube was removed in three months, and the sinus closed, but crepitations continued over the whole of the left base.

Case V.—J. M., æt. 2 years. During convalescence from scarlet fever was affected with cough and breathlessness, which are increasing. Signs of fluid at left base. Aspiration 14th July 1882, 6 oz. of pus removed, giving great relief. Incised, owing to reaccumlation, 24th July; 8 oz. of pus. Cured 6th September.

Case VI.—O. L., æt. 6 years. Chest troubles commenced after an attack of typhoid fever, and have gradually got worse. The difficulty in breathing is so great he cannot lie down. Profuse sweating, slight cough, and frequent vomiting are the other symptoms. Signs of a large collection of fluid in the left chest, displacing the heart to the right of the sternum. Incised 8th April 1883; 1 pint of pus. Cured 30th April.

Case VII.—C. O., æt. 8 months. Has been attended for two months with pleurisy and effusion on the left side. The effusion, which at one time occupied the whole left chest, has steadily decreased, but latterly the baby has been rapidly losing ground. Cough is spasmodic and severe. Night sweats, vomiting, and emaciation decided me on opening her chest. This was done on 23rd May 1883. The pus was so thick that only 1 oz. escaped at the time of operation, but during the night a large quantity came into the dressings. The child appeared in no way benefited, and continued steadily to lose ground until 23rd July, when it died in a convulsion.

Case VIII.—M. L., æt. 23 years, came complaining of shortness of breath on 4th February 1883. The usual physical signs of fluid in the left chest were present. Paracentesis was performed, but the fluid reaccumulated in a short time after every tapping, and so on 4th March I incised the chest. The fluid was sero-purulent. Cured 7th April.

Case IX.—C. W., æt. 4 years. First seen on 24th July 1883; complains of shortness of breath, weakness, and loss of appetite. Physical examination shows the presence of fluid in the left pleura.

The symptoms became worse, and he had high temperature and night sweats. On 30th September a large aspirator needle was introduced into the left pleura, the parents of the child not having allowed anything to be done previously; nothing came. Feeling confident that pus was present, on 1st October I incised the chest and evacuated 4 oz. of very thick inspissated pus. The case went on well, and was healed on 5th November.

CASE X.—E. G., æt. 4 years. First seen on 28th January 1884, with the usual signs of pleuritic effusion on the left side. Pus diagnosed on 5th March. Chest opened and drained 6th March, 2 oz. of pus escaping. Cured 20th April.

CASE XI.—J. M., æt. 30 years. First seen on 12th April 1884, when the hypodermic needle introduced into the right pleura drew off pus. Operation 15th April. Cured 20th May.

CASE XII.—T. B., æt. 9 years. First seen 12th March 1885. Physical examination revealed pleurisy with effusion on the left side, there being absolute dulness all over, and the heart displaced to the right side. Aspirated 20 oz. of clear serum. On 28th March again aspirated, and withdrew 17 oz. of flocculent serum. On 7th May repeated the aspiration, and got 14 oz. of pus. Chest incised on 18th; 1 pint of pus. Unfortunately I allowed it to become septic; and, in spite of a variety of measures, it would not close. He went on in this condition for eighteen months, gradually getting weaker; but his parents would not consent to any further operation till 27th January 1886, when I excised portions of the fifth, sixth, and seventh ribs. The incision was made slightly posterior to the mid-axillary line. A counter-opening was also made posteriorly in the sixth interspace, and a tube passed from one opening to the other. In shortening the tube it was done by pulling it through the posterior opening, so that the lateral incision could heal. Healed 26th March.

CASE XIII.—W. E., æt. 3½ years. First seen 25th November 1886. Complains of severe cough and copious, sour-smelling expectoration. Pus in the left pleura communicating with bronchus by a fistulous opening. Incised 9th December; healed 30th.

CASE XIV.—J. S., æt. 22. First seen 14th November 1887. Complains of breathlessness, night sweats, and loss of flesh for some months.

Physical Signs.—Diminished vocal fremitus on right side posteriorly. Dulness all over back of right side, absence of breath-sounds and vocal resonance, and the ordinary signs of a large vomica at the right apex. There is a bronchial fistula, through which about 10 oz. of pus are daily expectorated.

Nov. 15.—Chest incised; 1 quart of pus. After this he improved in every way. On 12th December commencing phthisis at the left apex was discovered; 29th, complains of breathlessness and fainting, due to cardiac failure.

Jan. 1, 1888.—Died suddenly.

Case XV.—C. H., æt. 4 years. First seen by me 8th August 1888. The history was that two and a half months ago he began to complain of weakness, loss of appetite, and shortness of breath. A medical man saw him, and said he had right pleurisy with effusion. He grew worse, and pus was diagnosed, and the chest opened in the eighth interspace in the anterior axillary line. No spray was used, and the dressing of the wound appears to have been neglected. He grew worse, and after two months the mother consulted me. At this time the sinus was discharging about 8 oz. of pus daily, and the boy was extremely emaciated. With her consent, on the 10th of August I excised portions of the seventh and eighth ribs. The boy rapidly improved, and the wound closed in three weeks.

Case XVI.—J. S., æt. 2½ years. First seen 13th September 1888. The usual signs of empyema on the left side, which the hypodermic needle confirmed. The child had been ill for a fortnight, and was extremely thin. Opened chest 14th September; 1 pint of pus. Cured 6th October.

Case XVII.—W. P., æt. 9 years; has been ill since the second week in May, suffering from right pleurisy with effusion. First seen by me on 12th July, and empyema diagnosed. The father refused operation till 17th October. The lung was quite collapsed, and the boy in a very grave condition. About 16 oz. of pus escaped. Cured 11th November 1888.

Besides these cases, there have also come under my notice three others in which the disease was allowed to run its course. They all occurred in children under 10 years of age, and in each case the parents would not consent to operation.

Case XVIII.—After a severe illness, followed by œdema of the chest-wall, the pus escaped externally. During the year of convalescence that followed four sinuses appeared, each before opening causing pain and constitutional disturbance. Finally the child recovered with a collapsed lung.

Case XIX.—In this case the day before paracentesis was to be performed a violent fit of coughing resulted in the expectoration of half a pint of pus, and after a fortnight's gradually diminishing expectoration recovery followed.

Case XX.—When first seen, two months previously, the left side of the chest was nearly full of fluid, which the hypodermic needle proved to be pus. The parents refused operation, and the child, after a slow and tedious convalescence, recovered, the pus being gradually absorbed.

Remarks.

From the above cases, which have all occurred within the last six years, it would appear that empyema is more common, especially in children, than is generally supposed. It may be overlooked owing to there being only a thin layer of fluid or to too hard percussion. The breath sounds can, as a rule, be heard distinctly on the affected side, but are commonly tubular in character. The vocal resonance is in some cases not diminished, and in some is increased.

These cases also show that the disease is not serious, and very amenable to treatment. Of the twenty cases reported only one terminated fatally, and even this cannot be said to be due to the treatment (Case VII.) The patient was a miserable starved infant of 8 months, and at the time of death, which took place in a convulsion, the chest showed considerable improvement. In five cases prior to operation pus had been expectorated through a fistulous opening communicating with a bronchus. In one case (XIX.) a cure without operation was effected. Although this happened, these cases pursued a thoroughly septic course; after the operation the fistula closed and the lung expanded freely.

That pus in the pleural cavity is capable of absorption is proved by Case XX., where operative interference was not allowed, and the pus disappeared, and by Case IX. In this the parents refused operation for six weeks, and on introducing a large aspirator needle no pus escaped. When the chest was incised, 4 oz. of thick inspissated pus came away, and I have no doubt, had a longer time been allowed, it would, as in Case XX., have been entirely absorbed.

In two cases (Nos. IV. and XIV.) the primary phthisis of the left and right lungs respectively was complicated with empyema. In both of these, too, there was a bronchial fistula communicating with the pleural cavity, through which pus in large quantities was daily expectorated. As in the other cases of fistula, on draining the pleura the purulent expectoration ceased, and the lung fistula closed. Both these cases went on satisfactorily as far as the empyema was concerned—one ending in ultimate recovery, the other in death from cardiac failure on the forty-fifth day after the operation, but for some days prior to his decease the discharge had been serous, and only enough to stain the dressings.

In two cases (Nos. XII. and XV.) I had to resort to excision of portions of ribs.

Diagnosis.—A short, dry cough in a child with ashy complexion, who sweats freely, and more especially at night and about the head, with signs of pleuritic effusion and febrile temperature, is almost pathognomonic of empyema. The diagnosis can only be made absolutely certain by the use of the hypodermic needle. I believe this may always be used with safety if the skin round the puncture be purified and the needle made aseptic. In some cases two or three punctures are necessary before the result is absolutely certain.

Operation.—The patient is prepared in the usual way as for any operation. The side of the chest is previously washed with soap and warm water, and carbolic lotion (1–20). Chloroform is administered with the patient lying on his back. The side to be incised is made to project over the edge of the table to avoid turning the patient on to his sound side. In all cases the spray is used both at the operation and during the after-treatment. This I look upon as one of the most important details necessary for success. The only two cases in which I have been called upon to perform Estlander's operation were those in which the spray was not used.

Situation of Wound.—The method I have adopted in all my cases is to make, first, a vertical incision through the skin in the posterior axillary line, then a horizontal incision through the muscles into the pleural cavity along the upper border of the seventh rib. The handle of the knife is then introduced into the wound, and a pair of sinus forceps is slid along it into the pleura, and the wound dilated. A large-sized drainage-tube about 3 inches long is inserted. The tube is perforated for the inner third of its length. If there are any holes in the outer half, granulations grow into them and block the tube. A safety-pin is passed through the end of the tube to prevent it slipping in, and it has also this advantage over tubes that have an expanded end to apply to the chest-wall, that the tube may be shortened without pulling it entirely out of the pleural cavity. As day by day the tube is shortened the pin is removed, the tube pulled out about half an inch, the pin reinserted, and the projecting end of the tube clipped off. I have never required to excise a portion of rib to allow of free drainage.

The dressings now used are a layer of carbolic gauze, over this a wood-wool pad, and a bandage firmly and carefully applied. If necessary, the wound is dressed the same evening, and it is generally advisable to do so. The dressings are changed every day for a week, and after that time every second day is generally sufficient. The tube is shortened in about four days, and thereafter every third day, according to the amount of discharge. The tube requires changing for an unperforated one when about an inch and a half have been removed.

During the third week the dressing is less frequent, and in a number of cases the tube can be removed, and the wound rapidly closes.

The discharge, when all the pus has been evacuated, after incision is normally serous. Should it continue purulent after the third or fourth day, it shows either that the tube is not acting efficiently or that the wound is septic.

In all the cases which have been treated by simple antiseptic incision and drainage the lung has expanded so completely as to leave no retraction of the chest-wall, even where there has been abundance of pus, or the case has been left untreated for a sufficient time to allow of absorption of a great part of the fluid. In Case XVII. the lung was absolutely compressed for six months before operation, so much that there was considerable lateral curvature of the spine. Two months after the closing of the wound the right half of the chest measured the same in circumference as the left, and all traces of curvature had disappeared.

This result I attribute to the absence of irritation from putrefaction or antiseptic irrigation, which it appears to me leads to chronic inflammation with thickening and subsequent contraction and cicatrization of the thickened pleura. The result in the two cases may be compared to the pliability of a cicatrix, the result of primary union, and of that by granulations. The former is soft, pliable, and non-adherent; the latter firm, non-distensible, and adherent to the underlying bone. In empyema the lung should expand if the irritation by injections and putrefaction is avoided, because I think these cases show that aseptic pus does not lead to so marked thickening and contraction of the pleura as to cause compression and binding down of the lung by adhesions.

As an aid to assisting the lung to expand, immediately the patient is able to be out of bed he should be encouraged in the use of gymnastic appliances, *e.g.*, dumb-bells, hanging and swinging on trapeze, etc., and in making powerful expiratory efforts, such as running up and down stairs, playing wind instruments, etc.

To sum up, my conclusions are:—

1. That in the diagnosis of empyema the only certain sign is obtained by the exploring needle, and that this, carefully used, is perfectly safe.

2. That antiseptic incision, the wound being just sufficiently large to admit a full-sized drainage-tube in the posterior axillary line, and made with the patient on his back, drainage and careful dressing are all that are required.

3. That the average time for healing, in uncomplicated cases, should not exceed four weeks.

4. Double openings, irrigation of the pleura, excision of the ribs, and such other devices, are unnecessary and harmful if the case be seen before an opening has formed through the skin.

V.—THE ORIGIN, SPREAD, AND DECLINE OF INFECTIOUS DISEASE.

By F. P. Atkinson, M.D., Surbiton.

There can be no question that there are many difficulties connected with the question of the origin, spread, and decline of infectious disease. As to its origin, I think it may safely be said to be due to the absorption into the system of certain particular forms of germ life. Ordinary decomposing matter, such as game and venison, will not, as a rule, cause disease. Fresh excreta will not necessarily produce infectious fever, since the natives of some parts, I think, of South America eat fresh excreta mixed with clay; but directly the particular germ gets imported into either the drinking water or milk, septic disease at once breaks out. The outbreak of cholera which occurred in the Wandsworth Road in the year 1847 pretty conclusively proves this point. Prior to this year no great illness had been prevalent on either side of the road, but directly one case occurred on the north side of the road, the infection rapidly spread to all the houses on this side, while those on the south remained perfectly free. The cause of the difference was that the houses on the north side had waste pipes running direct from the cisterns into the drains, while those on the south side had cesspools and no common drain.

The greatest difficulty of all is to arrive at a satisfactory explanation as to the manner in which infectious disease spreads from one to another. Certain diseases, such as whooping-cough, measles, German measles, influenza, typhus, smallpox, and mumps, are unquestionably spread by the absorption of specific germs by the respiratory passages. There can be no question here as to poisoned milk or water; but what about the spread of enteric fever, cholera, diphtheria, and scarlatina?

The poison peculiar to each of these can undoubtedly be conveyed by means of milk or drinking-water, and it can also, I believe, be conveyed by the breath, though it is a curious fact, as noticed by Dr Harley, that few of these diseases spread much when removed from their real source or origin. The cause of this may be, *to a great extent,* the care which is taken with regard to isolation and disinfection, for if the germs get transported by means of milk or drinking-water, they spread disease just as rapidly as in their original home. In proof of this last statement I give the following instance:—In the year 1882 a case of diphtheria was transported from London to a dairy in Surbiton, and the patient was there nursed by the same person who attended to the milk. In a very short time 40 cases of diphtheria were distinctly traced to this milk supply, and the consequence was the dairy was ordered to be closed. No fresh cases then occurred, and the epidemic ceased.

The propagation of infectious disease by means of a third person I consider extremely rare, and, except in the case of smallpox, typhus, and scarlatina, not clearly proved.

The air can scarcely be held to be a conveyer of septic disease, inasmuch as it is the greatest destructive agent known. There have been plenty of assertions to this effect, but they have had to be one by one abandoned.

It is now pretty well known that enteric fever, cholera, diphtheria, and scarlatina generally diminish with the improvements in drainage and water supply.

One matter intimately connected with the spread and decline of infectious disease is the susceptibility of certain persons to certain diseases and the general state of the public health at the time the outbreak occurs. As regards the former, we have an exemplification in the proneness of the Royal Family generally to contract enteric fever. As regards the latter, we know that several persons may be placed under identically the same conditions, and yet some may suffer severely, some only slightly, and some may altogether escape. Exhaustion of body or brain, no matter how brought about, predisposes to an attack.

There are doubtless many points involved in mystery as regards infectious disease, but great progress has been made of late years towards their elucidation. Let each one try to contribute his quotum of knowledge, however insignificant it may be, and then many points, at present obscure, may have light thrown upon them and become as stepping-stones to further discoveries.

VI.—ON THE ONLY WAY OF RAISING THE TONGUE, THE EPIGLOTTIS, AND THE ARYTENO-EPIGLOTTIC FOLDS AT ONE AND THE SAME TIME.

By JAMES FOULIS, M.D., C.M.

(*Read before the Medico-Chirurgical Society of Edinburgh, 6th March* 1889.)

(*Continued from page* 28.)

WE may be sure that no method of raising the tongue and the epiglottis which demands so much to make it successful can ever become popular. There are very few men who may be called upon to try Howard's method for the first time who will have the courage to bend back the neck to the utmost. And even though this is done, how can we be quite certain that the tongue is at the same time drawn well away from the pharyngeal wall, without which the post-oral airway cannot be free? Is there "no fear of harm" in thus bending back the neck to the "utmost" in young persons and in children?

It is the "fear of harm" that will make men at all times hesitate to bend back the neck to the "utmost." And lastly, in cases of artificial respiration, where it is sometimes necessary for many hours at a time to keep open the air passage into the lungs, it would be a very difficult thing to keep up this "utmost" extension of the neck for such a long period of time.

In connexion with this subject it is right to mention another source of danger. Your President, Dr John Smith, many years ago pointed out that in such extreme bending back of the head the internal jugular veins may be dangerously compressed against the transverse processes of the atlas vertebra.

Enough has been said to show that Howard's method of extension of the neck, while it undoubtedly renders tense the elevators of the hyoid bone, and in this way raises the epiglottis from the pharyngeal wall, is not one that can at all times be relied upon as a means of raising the tongue from its contact with the pharyngeal wall; and if the tongue is not kept free from contact with the posterior pharyngeal wall, the post-oral airway cannot be free.

Now what we require, and what we are looking for, is a simple means by which we can with certainty, under all conditions of apnœa, raise up the whole tongue and the epiglottis from contact with the posterior pharyngeal wall; but not only so, the method must be one which will enable us to keep these parts raised up from contact with the posterior pharyngeal wall, say for hours at a time if need be, during artificial respiration. For it must be clear to all, that the act of raising up the tongue and the epiglottis in these cases of obstructed breathing may not be enough. In the great majority of such cases we may be called upon to perform artificial respiration after we have raised up the tongue and the epiglottis; and therefore it is necessary that our method should be one that not only effectually raises the tongue and the epiglottis but also makes it absolutely certain that these parts shall not fall back, or be sucked downwards by the rush of air into the chest during the movements of artificial respiration.

It is not enough simply to make tense the elevator muscles of the hyoid bone, and to raise the epiglottis, either by Howard's method, or by pushing or pulling forward the lower jaw; for in none of these methods can we be sure that the large, flabby tongue is at the same time drawn away from all danger of contact with the pharyngeal wall. Nor is it enough to draw the tongue violently forwards and upwards, for, as has already been pointed out, though this method undoubtedly stretches the tongue to its fullest extent, and draws it well away from contact with the pharyngeal wall, experiment has proved that this method cannot drag upon the epiglottis, except through the medium of the muscles which pass between the lower jaw and the hyoid bone, and that such an amount of force as is necessary in this way to drag forward the tongue and the epiglottis at one and the same time, by means of the catch forceps, is hardly justifiable. Besides, one would hardly dare to drag with such force on these structures for any lengthened period in artificial respiration; and in the case of infants and young children it would be quite unjustifiable to do so.

Now, the method of raising the tongue, and of raising the epiglottis and stretching the aryteno-epiglottic folds at one and the same time, which for some years past I have advocated, is one that enables us to give instant relief in all forms of obstructed breathing, and in doing so, makes it absolutely certain that there is no risk of the tongue or the epiglottis being sucked downwards or valving against the pharyngeal wall during the movements of artificial respiration.

Bearing in mind the anatomical facts to which I have already referred, namely, that the base of the tongue is so intimately connected with the hyoid bone and the epiglottis that if it, the base, can be tilted or hooked forward, the hyoid bone and the epiglottis will at the same time go forward with it; we then see that if we can pass an instrument, such as our forefinger, down behind the tongue as far as its base, and hook forward the base, the hyoid bone will be carried forward, and at the same time the epiglottis will be raised and the aryteno-epiglottic folds put on the stretch; and as long as such an instrument is in this position, one can keep the body of the tongue and the epiglottis so far removed from the pharyngeal wall that there is not the least danger of these parts acting as valves, or in any way obstructing the ingress of air during the movements of artificial respiration.

The forefinger would be the best of all instruments for this purpose, but there are several objections to it. In the first place, it is sometimes almost impossible to get the forefinger into a patient's mouth owing to the rigidity of the jaws, and there is always the risk of being severely bitten by the half conscious patient. The tongue, too, is a large, slippery, flabby mass, and very difficult to move by means of the forefinger in consequence of its slippery nature. The finger also takes up too much room in the mouth, and may not be sufficiently long to reach to the base of the tongue.

It will be in the recollection of many members of this Society, that early in the year 1880 I read before this Society a paper entitled "The Treatment of Asphyxia during the Administration of Anæsthetics." In that paper I recorded a case of opium poisoning, in the treatment of which it was necessary to carry out artificial respiration for many hours under very trying circumstances. The case, shortly, was as follows:—Late one night I was sent for hurriedly to see a lady who was said to be either dead or dying. When I arrived at the house, at two o'clock in the morning, I found the patient in bed lying on her back and apparently dead. I could get no satisfactory history of the case at the moment. She was pulseless at the wrist, and respiration had ceased. She was deeply cyanosed, and a cold, clammy moisture was on the body. With the stethoscope I detected evidence of slight cardiac action, which proved to me she was still alive. Looking into her eyes by means of a candle-light, I found the pupils contracted to a pin's point. I then threw back her head over a pillow and passed my finger down her throat as far as the base of the tongue, which I hooked forward. Now, mark what occurred immediately I hooked forward the base of the tongue. There was a slight inward rush of air. From this it was quite clear to me that at the end of an expiratory effort the tongue had fallen back, and at the moment of an inspiratory effort it had been sucked downwards and backwards against the pharyngeal wall, where it remained as a valve entirely preventing the passage inwards of air. On removing this valve the natural resilience of the chest wall came into play, and air rushed into the chest through the glottis immediately the chest wall expanded. I soon came to the conclusion that the case was one of opium narcosis, and that artificial respiration must be energetically carried out. As soon as I took my finger out of the mouth, the tongue fell back again, and valved against the pharyngeal wall; and I then adopted a very simple means to prevent this occurring.

Howard's method of chest compression was carried out for twelve hours in this case, and during this time I proved to myself, over and over again, that neither it nor any method of artificial respiration can be of use in such cases as long as the tongue and the epiglottis are allowed to obstruct the patient's breathing. As the patient lay on her back in a deeply insensible state, two pillows were placed under her shoulders, and the head was allowed to fall well back. The handle of an ordinary tablespoon was then passed with the greatest ease down behind the patient's tongue as far as the base, and as the spoon was levered backwards the hyoid bone and the base of the tongue were made to project as a lump in the middle line of the patient's neck, and as long as this lump was seen in the patient's neck I felt quite certain that neither the epiglottis nor the tongue could in any way again obstruct the breathing during the process of artificial respiration.

On removing the spoon, which I did frequently as an experiment, it was found that inspiratory movements caused the tongue and the epiglottis to valve against the pharyngeal wall. As long as the spoon was in the throat and held in the proper way there was no obstruction to the breathing. Hour after hour a servant held the spoon in the proper position, and it was delightful to hear the air rushing in and out of the chest through the glottis during the movements of artificial respiration. On passing a finger into the mouth behind the spoon it was found that a large cavity existed between the spoon and the pharyngeal wall, and that the air passages through the mouth and through the nares were perfectly free.

Owing to the patient's legs having been dreadfully burnt by hot-water bottles which had been placed on them before I arrived at the house, it was impossible to get her out of the bed. So for twelve hours artificial respiration was kept up. At the end of that time it was considered safe to remove the spoon from the throat and to trust to natural breathing. The lady made a good recovery and never made a single complaint as to any strange feelings in her throat, proving that the method—by means of the handle of a tablespoon—of raising the tongue and epiglottis for many hours at a time is quite harmless.

I regret very much I did not publish that case and my paper in full at the time. There were circumstances, however, which made it impossible for me to do so. When the Secretary of this Society asked me to send him my manuscript for publication, instead of doing so I sat down and wrote out carefully an abstract of my paper for publication in the *Edinburgh Medical Journal*, which I knew would embody the important parts of it. Kindly allow me to read that abstract as published, which is as follows:—

"Instead of seizing the tongue with a pair of catch-forceps and dragging it forward, as is usually done when a patient's breathing is arrested during the administration of chloroform, Dr Foulis proposes that one of the curved ends of the glossotilt should be passed down over the dorsum of the patient's tongue as far as the base, and then the instrument should be pressed back in such a way that it acts as a lever. The base of the tongue and the hyoid bone are thus made to project forward. When this takes place, the epiglottis, being closely connected with the hyoid bone, is drawn well away from the rima glottidis, and the aryteno-epiglottidean folds are rendered tense. The instrument is passed into the throat with the greatest ease if the patient's head is allowed to fall well back over the end of the operating table, or the head may be made to fall back by passing an arm under the patient's neck and raising it.

"Dr Foulis maintains that the only effectual way of raising the base of the tongue, of raising the epiglottis, and of making tense the aryteno-epiglottidean folds at one and the same time, is to act on the base of the tongue and the hyoid bone in the manner he has described.

"All are agreed that in any case of impending death during the administration of an anæsthetic artificial respiration is the best means of treatment; but artificial respiration is of little value if air is not made to pass freely in and out of the lungs. The object of the paper was to show that the tongue, the epiglottis, and the aryteno-epiglottidean folds occasionally act as valvular impediments at the orifice of the windpipe to the passage of air in and out of the lungs, and that by the use of a simple instrument, such as the glossotilt, or even the handle of a large spoon, it was possible to raise up the dorsum of the tongue from its contact with the posterior pharyngeal wall, and at the same time to raise the epiglottis, and to render tense the aryteno-epiglottidean folds; and that when this was accomplished there could not possibly be any obstruction to the passage of air in and out of the lungs by those structures during the movements of artificial respiration. It was generally taught by surgeons that dragging on the tip of the tongue with catch-forceps was sufficient to effect all this; but Dr Foulis showed that this was not the case, and that it was not possible to stretch the aryteno-epiglottic folds except by hooking forward the hyoid bone and the base of the tongue; and this could be most easily done by using the simple instrument in the way he had described."

I have read these extracts to-night, just as they were published in the *Edinburgh Medical Journal* nine years ago, in order that you may see that, as far back as 1880, I had devised a simple means of raising the tongue and the epiglottis at one and the same time, and had already tried it, and found it to be thoroughly reliable and trustworthy, in the case of opium poisoning which I have narrated. Dr Howard, therefore, cannot claim to have discovered a new and only way of raising the epiglottis. Up to 1880, I had read all that Dr Howard had published on the subject of artificial respiration, and I saw then that his method of artificial respiration is not a reliable one, because in it there is no certainty that the tongue may not act as a valve by falling against the pharyngeal wall during the inrush of air. Allowing the head to fall well back over the end of a table until the skin of the neck is made tense will undoubtedly raise the epiglottis, but there is no certainty that the tongue will be raised up at the same time, and unless this is done no method of artificial respiration can be considered a reliable one. I say nothing about Dr Howard's method of chest compression in the meantime. Our attention is at present fixed upon the question, what is the best method of raising the tongue and the epiglottis at one and the same time; and I have attempted to show that Howard's method will not answer, for the reasons stated. There is but one way of raising the tongue and the epiglottis at one and the same time, and this one way is perfect, inasmuch as it makes it absolutely certain that neither the tongue nor the epiglottis can fall back and valve against the pharyngeal wall during the movements of artificial respiration, or during the natural breathing of a deeply anæsthetized person.

If you now look at Plate III., Fig. 3, and place it in such a position that it represents a person lying on his back with the head well back, you will see that the large flabby tongue hangs down, suspended, as it were, from the upper genial tubercles, and that a very little relaxation of its muscular supports will allow it to fall against the pharyngeal wall. The hyoid bone at the base of the tongue is also suspended by muscles from the lower genial tubercles of the jaw, and, like the tongue, if its suspensory muscles become relaxed, it also will fall back against the pharyngeal wall, and as the hyoid bone moves so will the epiglottis and base of the tongue move.

As air can pass into the lungs only in the channel which lies behind the tongue and the epiglottis, it must be clear to every one that to keep this channel open we must prevent the tongue, the hyoid bone, and the epiglottis from falling back on the pharyngeal wall, and this can be most easily done by lifting forward the whole tongue and the hyoid bone in exactly the same way as I did in the case of opium poisoning, in which it was necessary to carry out artificial respiration for fully twelve hours.

Suppose, now, we have to do with a case in which, from some cause or other, it is necessary to carry out artificial respiration to bring back the insensible person to life, the first thing to be done is to cause the patient's head to fall well back over the end of the table or bed, on which he may be lying; or two pillows may be placed below his shoulders, and the head allowed to fall back naturally. There are other ways of making the head fall back, but I need not speak of them now.

In the next place, it is the simplest thing in the world to insert an instrument into the air-passage which lies between the tongue and the pharyngeal wall (Plate IV., Fig. 4). A forefinger may be passed in, or any flat instrument, such as the handle of an ordinary tablespoon or a fork, *k*. It is only necessary to insert such an instrument between the teeth, and then push it onwards on the surface of the tongue till it comes in contact with the pharyngeal wall. The end of such an instrument will be now found to be near the base of the tongue. As the head of the patient is bent well back the lower jaw is carried out of the way, and if the handle of the spoon is now pressed back, the whole tongue will be carried forward, and all parts which lie in front of the handle of the spoon may now be lifted forward with great ease; and it will be found that as the instrument is levered backwards its point glides forward on the surface of the tongue until it strikes on the middle glosso-epiglottic ligament, and then fits into a sulcus at the point of junction of the tongue with the hyoid bone. The hyoid bone can now be made to move forwards as far as the thyrohyoid membrane will allow it, and the epiglottis will, of course, move with it. A very slight amount of force is sufficient to do all this, and it can be done in a second or two of time. Now, what is the state of matters after this is done? It is this,—the whole tongue down to its base has now been lifted forward as far as the thyrohyoid membrane will stretch, and as a result the tongue is well removed from all danger of contact with the pharyngeal wall; the epiglottis is also raised, and the aryteno-epiglottic folds are put on the stretch, the mouth is quite open, and air can now pass into the lungs through the mouth as well as by the post-oral channel, and during the movements of artificial respiration none of these parts can possibly act as valvular impediments during the inrush of air. There is no theory in all this. The method has been thoroughly put to the test, and it has never failed. It is applicable in every case where it is necessary to remove those causes of obstruction to breathing such as I have described, and it is applicable in every case where it is necessary to carry out artificial respiration after these causes of obstruction have been removed.

But some one may say we agree with all you have said as regards the mechanical obstruction caused by the tongue and the epiglottis, and as regards the means proposed to relieve this obstruction, but you have not sufficiently considered that form of vital

Plate III.

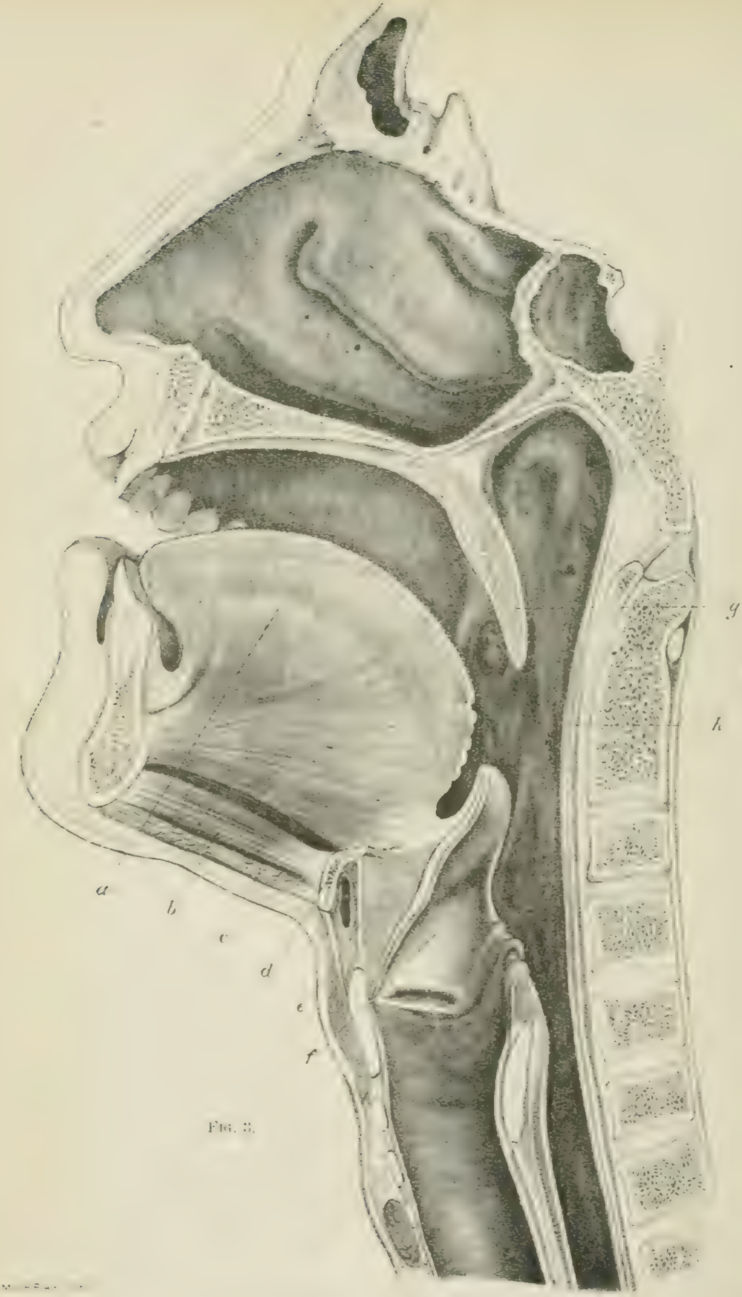


FIG. 3.

Vertical mesial section of head and neck, to show the relation of the tongue and epiglottis to the pharyngeal wall.

a. The tongue and geniohyoglossus muscle.
b. The geniohyoid muscle.
c The mylohyoid muscle.
d The hyoid bone.
e. The thyro-hyoid membrane.
f. The epiglottis and right arytenoepiglottic fold.
g. The soft palate.
h. The post-pharyngeal wall.

(From Luschka.)

Plate IV

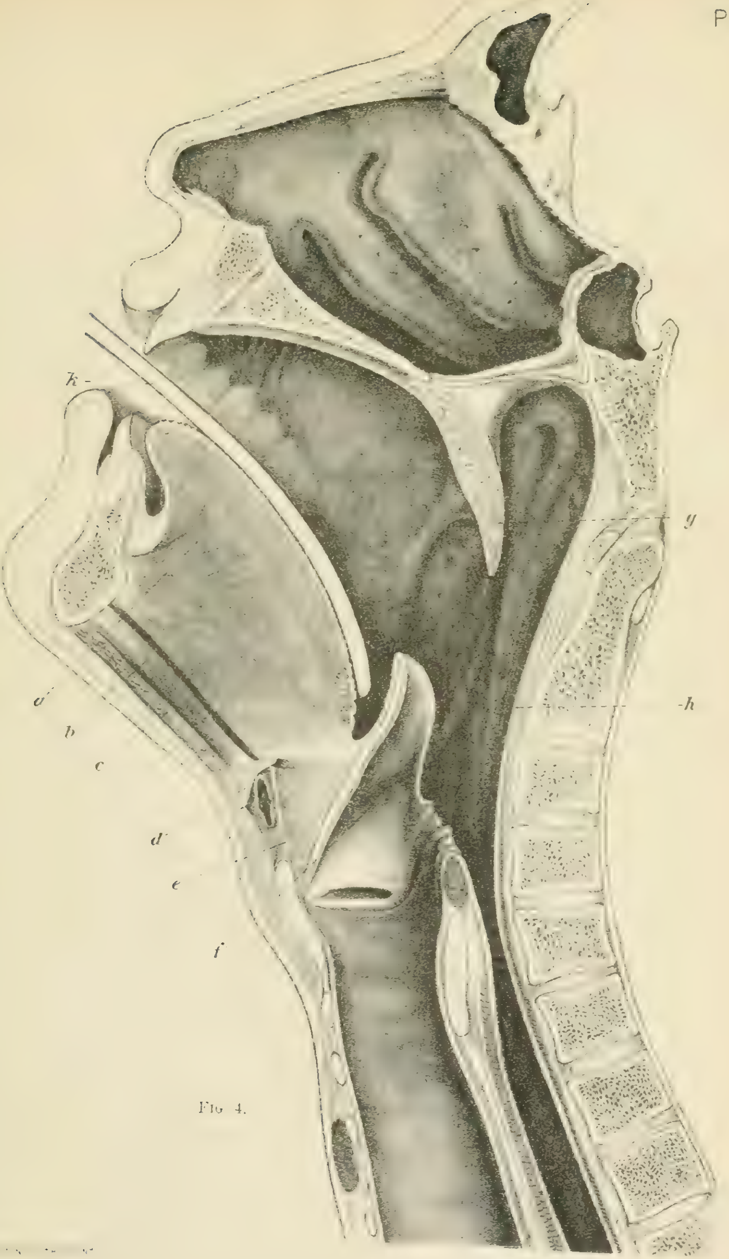


FIG. 4.

Vertical mesial section of head and neck, to show the free breathing-space through the mouth and nose, when the whole tongue is lifted forward by means of the handle of an ordinary table-spoon. On depressing the spoon, the base of the tongue and hyoid bone move forward.

a. The tongue and geniohyoglossus muscle.	*f.* The epiglottis and right arytenoepiglottis fold.
b. The geniohyoid muscle.	*g.* The soft palate.
c. The mylohyoid muscle.	*h.* The post-pharyngeal wall.
d. The hyoid bone.	*k.* The handle of a large spoon, lifting forward the whole tongue with the epiglottis.
e. The thyro-hyoid membrane.	

(Partly from Luschka and from Nature.)

obstruction which seems to occur within the box of the larynx, and which is accompanied by a peculiar inspiratory laryngeal stridor, and is sometimes met with in the early stages of chloroform administration. It is difficult to see how the true or false vocal chords can cause serious obstruction to the breathing during the administration of chloroform; but, undoubtedly, in these cases the noise we hear seems to be vocal. It is a very alarming noise, and the patient may become somewhat cyanosed in such cases. We have often seen that dragging on the tip of the tongue gives instant relief in these cases. How is the relief thus brought about? I suspect it was in these cases that Lister taught that the relief to the breathing was brought about in a reflex manner, though it is not easy to follow the physiology of the process.

There are cases, however, in which dragging on the tip of the tongue with catch forceps does not put a stop to the laryngeal stridor, though it gives relief to the laboured breathing, judging from the accompanying improvement in the patient's face.

In regard to these cases I have always held the view that the obstruction to the breathing is more or less mechanical,—that is to say, the obstruction which makes the patient look cyanosed is not caused by the laryngeal vocal chords, but by the tongue falling back at the same time that the laryngeal stridor is going on. Hence dragging on the tip of the tongue removes the mechanical obstruction, but does not always remove the laryngeal stridor, which, however, will pass off if we can continue chloroform administration until reflex action is abolished. I have found that lifting forward the whole tongue and the epiglottis, and making tense the aryteno-epiglottic folds by means of the glossotilt, removes the obstruction to the breathing in these cases just as effectually as does dragging on the tip of the tongue with catch forceps.

As I have already said, all this can be done with the forefinger, but there are many objections to the forefinger being used, and as long as we can get hold of a tablespoon or a fork, or, in fact, any instrument to pass down into the throat as far as the base of the tongue, we need not use the forefinger. Now, my object in reading this paper is not to recommend any special instrument, but rather the simple method by which, under all conditions of apnœa, we can effectually raise the tongue, raise the epiglottis, and render tense the aryteno-epiglottic folds at one and the same time, all of which parts are liable to interfere with the breathing in an anæsthetized or unconscious patient, and all of which may greatly interfere with a successful result in artificial respiration.

At the same time, it is advisable to point out that there is a form of instrument that experiment on the dead and living body has shown to be extremely easy of application, and therefore I bring it before you this evening (Plate V., Figs. 5 and 6, *k*). The first instrument I used for tilting forward the base of the tongue was very like a double-handled tablespoon with the ends slightly curved; this answered very well; but from numerous experiments made lately, I have found that this instrument, which is called a glossotilt, answers best of all. It is simply a double-handled tablespoon, but the ends are so curved that when the point is inserted between the teeth, the curved end adapts itself to the roof of the patient's mouth, and a very slight amount of levering backwards brings its point into the sulcus which lies at the base of the tongue above the hyoid bone, and the end being well curved the whole body of the tongue lies in the hollow of the curve. The tongue as a mass can in this way be lifted forward with the greatest ease. In inserting it into the patient's mouth we simply keep the point in contact with the roof of the mouth, and push it onwards. It will, in consequence of the curve, find its own way to the base of the tongue, and then the whole tongue can be lifted forward until the thyrohyoid membrane is put on the stretch.

If you look at Plate VI., Fig. 7, *a*, which is a photograph in profile of a dead body, you will see that the line of skin which lies between the symphysis of the lower jaw and the prominent edge of the thyroid cartilage is a curved one. In this case, examination has proved to us that the tongue and the epiglottis have fallen back, and are in contact with the posterior pharyngeal wall. Now, look at Fig. 8, *a*, which is a photograph of the same body, but, in this case, the glossotilt is in position, and you will notice that the part of the neck which lies between the symphysis of the jaw and the thyroid cartilage is now bulged forward, and a finger passed into the mouth behind the glossotilt shows us that the tongue and the epiglottis are well lifted forward away from the pharyngeal wall, and that the breathing channel through the mouth and through the nose is quite free. Air may now be pumped in and out of the chest in this body by Howard's method of compressing the chest; and it is quite striking to notice how freely air passes in and out through the glottis, according as the chest is compressed or allowed to expand, and it will be clear to everybody that it is quite impossible for the tongue and epiglottis now to act as valves as long as the glossotilt is in its proper position. I might tell you of many cases in which I have used the glossotilt during the last 8 or 9 years, but this is not necessary. In the case of young children, and in newly born infants, the handle of a dessertspoon or a teaspoon may be used as a means of raising up the tongue and epiglottis, while artificial respiration by Howard's method is being carried out. In one case of a newly born infant I used the handle of a teaspoon to lift up the tongue and epiglottis, and air was made to pass freely in and out of the chest with one hand, while the other hand held the teaspoon in proper position.

Much might be said in favour of this method in the case of the apparently drowned, when artificial respiration has so often to be carried out energetically. I fear many lives have been lost in all

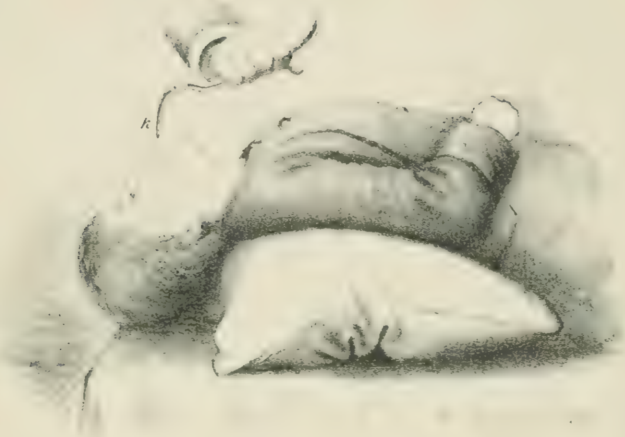


Fig. 5.

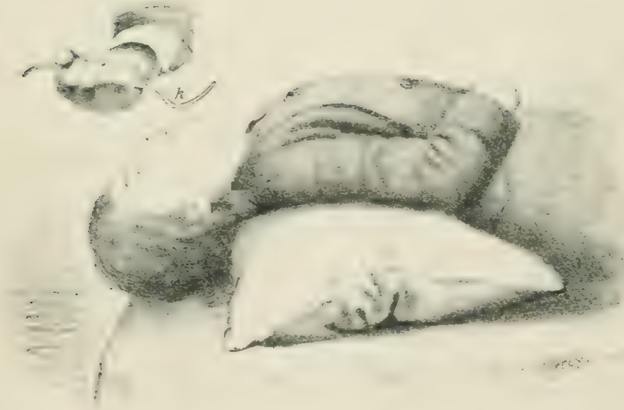


Fig. 6.

Showing the manner of using the glossotilt *k*. The point of the instrument is inserted between the teeth, and as it is pushed onwards, by a simple turn of the wrist (see fig. 6), it is made to lift forward the whole body of the tongue, and, at the same time, the hyoid bone and the epiglottis are carried forward.

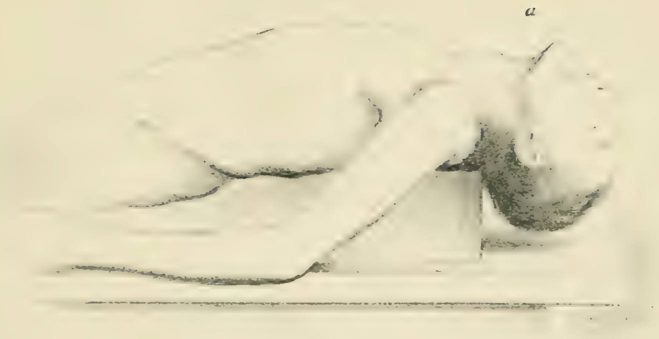


Fig. 7.

From a photograph of a dead body, showing the curved line (*a*) that exists between the chin and the upper border of the thyroid cartilage in the middle line. In this case examination showed that the tongue was lying in contact with the pharyngeal wall.

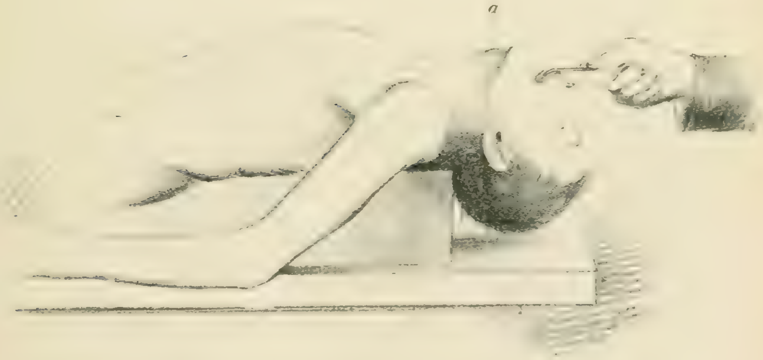


Fig. 8.

A photograph of the same body, showing the appearance of the neck below the chin in the middle line (*a*) when the glossotilt is held in proper position during artificial respiration. In this case examination showed that the whole tongue was well removed from the pharyngeal wall, and that the epiglottis was raised, and the arytenoepiglottic folds were quite tense.

parts of the world through not raising up the tongue and epiglottis properly in such cases of artificial respiration, and yet how simply this can be done when you know how to do it. A coat or two folded up and placed under the sufferer's shoulders would allow the head to fall back. A tablespoon or a fork is generally at hand, and if it is not, any straight instrument could be used in the same way as the handle of a tablespoon. Of course the fingers can be used to lift up the whole body of the tongue in any case when the patient is deeply insensible. In using the handle of a tablespoon or of a fork, if the head is bent back, such an instrument can be passed to the base of the tongue without the slightest difficulty. The sword swallower has no difficulty in passing his sword down his own gullet. When the head is well bent back the passage from the mouth to the gullet is almost a straight tube, and it is very easy to pass a straight instrument down this tube. It is well, also, to point out that in using the glossotilt it is not necessary to bend back the patient's head over the end of a table or over a pillow. If the patient is lying on his side, as in chloroform administration, on a table, or on the bed, the head can be bent back sufficiently to pass the glossotilt down behind the tongue as far as its base, when a very slight amount of backward pressure with the instrument will now cause the tongue to bulge forward in the neck before the lower jaw; and when this is done we may be sure that the tongue is carried forward as far as the thyrohyoid membrane will allow it, and that it is well removed from the pharyngeal wall. This act of carrying forward the whole tongue as far as the thyrohyoid membrane will allow it, also raises up the epiglottis, and makes tense the aryteno-epiglottic folds.

Howard's method of compressing the chest in artificial respiration, combined with this simple way of raising the tongue and the epiglottis, should be taught in all our public schools and in our ambulance classes. One may be called upon at any moment to assist in saving the life of a fellow-creature who has fallen into water, or who is in danger of death from want of oxygen, and if we know how to carry out artificial respiration in a proper manner without delay, many a life may be saved by such knowledge. I might have discussed the respective merits of Howard's, Marshall Hall's, and Silvester's methods of artificial respiration, but from what I have already said you will see that I prefer Howard's method of compressing the chest, combined with the proper method of raising the tongue and the epiglottis, to all others. My own experiments on the human body have convinced me that it is the most easy to carry out, while it is the most effectual. In no case have I ever heard a single complaint of injury being done to the soft parts by the use of the glossotilt. As I have already said, my object is not so much to recommend this instrument as to insist that there is but one safe and reliable way of raising the tongue, of raising the epiglottis, and of making tense the aryteno-epiglottic folds at one and the same time, and you may use any instrument you please to do this.

In one case I used the handle of an ordinary paper cutter, and in another case I could lay my hands on nothing but a gentleman's boot hook. This was passed with the flat side against the tongue as far as the hyoid bone, which was then tilted forward until the thyrohyoid membrane was put on the stretch. The patient was choking at the time, and instant relief was given by thus lifting forward the hyoid bone and the base of the tongue.

The important thing in such cases is not the instrument, but the knowledge of what ought to be done, and how to do it.

Part Second.

REVIEWS.

The Natural History of Cow-pox and Vaccinal Syphilis. By CHARLES CREIGHTON, M.D. London: Cassell & Company: 1887.

"Vaccination." Article in the *Encyclopædia Britannica*, Ninth Edition, Vol. XXIV. By the same Author.

Jenner and Vaccination: A Strange Chapter of Medical History. By the same Author. London: Swan Sonnenschein & Co.: 1889.

> "It is the nature of an hypothesis, when once a man has conceived it, that it assimilates everything to itself as proper nourishment; and from the first moment of your begetting it, it generally grows the stronger by everything you see, hear, read, or understand. This is of great use."—*Tristram Shandy.*

PERHAPS no better instance could be imagined of the occasional correctness of Tristram Shandy's views on "the nature of an hypothesis" than is furnished by Dr Creighton's opinions on vaccination. Three years ago he published an interesting little work on *Unconscious Memory in Disease*,[1] in which he formulated the doctrine that the word "memory" has "a real application to unconscious organic phenomena," and is not merely "a figure of speech." Illustrations of this memory he found in connexion with such maladies as cancer, tubercle, syphilis, pellagra, leprosy, albuminuria, neuralgia, etc. When he began, in the same year, to study the subject of vaccination, in preparation for the article in the *Encyclopædia*, he soon found reason to conclude that, in reading the natural history of cow-pox, he had fallen in with a fresh and very perfect instance of "unconscious memory." The original cow-pox, as it occurred in the bovine animal, was a disease of a very severe character, and when inoculated on man often produced very severe symptoms—deep phagædenic ulcers, erysipelas, axillary abscesses, much constitutional disturbance, and even "secondary" symptoms, as roseola, lichen, pemphigus, and, in one case, an affection of the throat. By careful "management" and selection of mild "strains" of lymph these characters of the "untamed" cow-pox have been gradually got rid of, and the result is the eight-day or "short cycle" pock of ordinary experience. Every now and then, however, we meet with reversions to the original type. Where the crust becomes detached and leaves an ulcer, we have been in the habit of attributing it to accident; where erysipelas has arisen, we have looked on it as entirely due to constitutional debility, or to foul instruments, or to direct infection from other cases; and where, on the Continent mainly, cases of "vaccinal syphilis" have occurred, we have accounted for them by heredity, or by insertion of the syphilitic virus from a specific sore, or by the possibility of inoculation from an infected vaccinifer. In all these suppositions we have been wrong, Dr Creighton tells us. The ulcer and the erysipelas were merely acts of memory on the part of the lymph used—a harking back to the original type of cow-pox as seen on the cow or on those vaccinated with "primary lymph." And the supposed syphilitic disasters were not due to syphilis at all—not even in the well-known case of the public vaccinator, who inoculated himself from a child with open syphilitic sores, and in due course developed the ordinary appearances of the disease. The symptoms, primary and secondary, were merely the result of a return to the first and worst characters of cow-pox. This brings us to the opinion that cow-pox has its analogue, not in small-pox, but in great-pox. It is not syphilis, but it is on all fours with it—it is a parallel disease. This being so, it naturally follows, in Dr Creighton's scheme of things, that cow-pox, having no relationship, either of similarity or of antagonism, to small-pox, has no preventive power against small-pox. It is not the case, and it never was the case, that vaccination had any inherent property of keeping away small-pox, nor of modifying an attack when it did occur. The popular traditions of the dairies, the variolous tests of the early vaccinators, the wide and prompt acceptance of the practice by the medical profession of Europe and elsewhere, the modern statistics both of mortality to population and of case mortality, and all the accumulated experiences of the world since the beginning of the century, form part of a gigantic delusion the like of which perhaps has never been seen before, while the key to the whole problem is to be found in Dr Creighton's theory, that organic phenomena, not merely mental but bodily, are endowed with unconscious memory, and that "memory" used in no metaphorical sense, but in the everyday meaning of the word, is as truly predicable of a disease like cow-pox as it is of the author of an autobiography.

---

[1] H. K. Lewis, 136 Gower Street, London, 1886.

It is with diffidence and misgiving that I have ventured to make this synopsis of Dr Creighton's thesis, for, if I am right in my statement of it, it follows that he sometimes uses English words in a sense which does not ordinarily belong to them. In the *Lancet* of 12th January last he says,—"When I began it [the subject of vaccination] in 1886 I had no other prepossessions than those which nearly all medical men have in favour of an established doctrine and practice. It was not until I had spent some months in a search among the authorities, pathological and other, at first hand, that I felt constrained to modify the opinions which I had hitherto implicitly accepted."

If I am right, or within a measurable distance of being right, in my interpretation of Dr Creighton's opinions, is it possible, by any common stretch of language, to describe these opinions as a "modification" of any previous opinions which were consonant with "the prepossessions which nearly all medical men have in favour of" vaccination? But, at any rate, I am safe in quoting his own words. In his latest book, addressed to the public rather than (as was his letter in the *Lancet*) to the profession, he speaks of vaccination as "a grotesque superstition."[1] Is this a "modification" of any ordinary views on the subject? If so, then a man who formerly held that the earth was a sphere, and now asserts that it is a level plane, may properly express his change of mind by saying that he has "modified" his views as to the shape of the planet.

It is obvious that, in endeavouring to square with his hypothesis all that is known of small-pox and cow-pox and syphilis, Dr Creighton sets himself a gigantic task, and one cannot but admire the courage which characterizes his effort to accomplish it. In attempting to review some of his principal lines of argument, I am met by the initial difficulty that his opinions are nowhere put in the form of definite propositions. I will, therefore, try to state each of them as necessity arises.

(1.) The first question to be taken up refers to the folk-lore of cow-pox. The belief in its protective power against small-pox, he says, was a localized one. It had never been heard of till rather late in the eighteenth century, and it had no foundation in fact. The only bond of connexion between the two diseases was the word *pox*. The "jingling sound of 'cow-pox—small-pox'" led the "officious gossips" of the countryside to make a legend out of it, just as the old herb-books alleged that for a person to carry about with him the herb hound's tongue, protected from mad dogs, and that the root of the dog-rose cured their bite. This, and this alone, originated the idle story.

Such is Dr Creighton's statement in Chapter II. of *Jenner and Vaccination*. Clearly it is of the very greatest importance, for Jenner dates his first thoughts on the subject from hearing in his master's surgery in Sodbury a young woman observe, "I cannot take that disease [small-pox], for I have had cow-pox;"[2] and during

[1] *Jenner and Vaccination*, p. 353.

[2] Baron's *Life of Jenner*, vol. i. p. 122.

the next thirty years much of his attention to the matter was due to the constant reiteration of this opinion by the dairy people of the Vale of Berkeley.

Is, then, Dr Creighton's statement correct? In his last published work he makes constant references to the early volumes of the *Medical and Physical Journal.* He seems to have ransacked them from beginning to end (and well worth the labour they are); but, curiously enough, he has missed their very clear and ample evidence, that he is here entirely wrong. Vols. iii. and iv. (p. 503 and p. 425), published in 1800, contain letters on the subject from Dr John Barry of Cork. Dr Barry shows that the country people of his part of Ireland were well acquainted with the disease, that they knew it by the name of *shinach* or *shinagh*, and that they attributed to it the antivariolous power. One lady had had it about 1760, "and was then informed by some of her neighbours that she never would have the small-pox," which turned out to be true, contrary to her own expectation, and in spite of many opportunities of infection. A gardener "gave himself the disease purposely, by rubbing himself against some one who was affected with it, from a conviction that it would prevent the small-pox." Still more interesting is the following:—

> "Johanna Sullivan, aged 50 (cookmaid at Dr Richard Walsh's of this city), when she was 13 years of age was brought with a number of other children to a dairy, for the purpose of being infected with a disorder of cows called the shinach, which by the general belief of the neighbours would secure for ever such as took it from the small-pox. She and the other children were made to squeeze the cows' teats till their hands and fingers were covered with the fluid matter of the disorder. . . . . When she was 20 years of age she was twice inoculated by Mr Godwin, an apothecary at Bantry, without effect; but on hearing from the mother that she had the disease above mentioned, he declined inoculating her a third time, alleging that there was not the smallest danger of her ever taking the small-pox, as he could aver from experience. She has since resided in Cork, where she was frequently exposed to small-pox, particularly about eleven years ago, when the grandchildren of the late Mr Attewell Hayes, with whom she then resided, were inoculated. In order, as she said, to be sure of herself, she lay with the children four nights in the height of the eruption, but did not take the disease."

Dr Barry gives another bit of evidence, of even more importance. It is an extract from a letter which he had received "from a lady of respectable connexions:"—

> "It is thirty years since my mother had the cow-pock. . . . . She has been inoculated frequently since, and exposed to the infection of small-pox in various ways without taking it, which has been attributed to her having had the cow-pock, universally known among our farmers by the name of shinach. I was last night speaking to my grandmother on the subject. She had the cow-pock fifty years ago. Her account agrees with my mother's. . . . . She has never been inoculated, but was very often exposed to the small-pox without taking it. At the time she had the disease there was scarcely a spring that the cows were not affected with it; and it was so universally believed that those who took it were ever after exempted from the small-pox, that people exposed themselves as much as possible to it. My grandmother, who is about eighty years old, says that the same opinion always prevailed in this country."

These accounts go to show three things, all denied by Dr Creighton—(1), that the disease was known as a preventive of small-pox long before "rather late in the eighteenth century;" (2), that the tradition was not confined to a few "English dairy-farming districts;" and (3), that as "shinach" does not jingle with "small-pox," the "jingle" theory will not account for the facts. It is *not* therefore a mere "case of the river in Macedon and the river in Monmouth." But, indeed, we don't need to go outside the covers of Dr Creighton's own book for proof. In a "note to page 21" (in which it is asserted that the legend is a local one), printed at the end of the work, he says that "there is an authentic record that the protective virtue of cow-pox had been talked of in the country near Gottingen previous to 1769." Here cow-pox was called *kuhpocken*, and small-pox *blattern*, so that again there was no "jingle" to give rise to the legend. But while Dr Creighton adds this note, he calls no attention to its bearing on his theory, and makes no attempt at a reconciliation of the fancy and the fact.

Thus one of the main contentions of the book is shown to be entirely without foundation, and in passing from it I have only to note of how much ingenuity the invention of the "jingle" doctrine shows the author to be possessed.

(2.) Dr Creighton asserts that the great bulk of the medical opinion of the country districts was dead against the antivariolous power of cow-pox. Indeed, in one place he conveys the suggestion (unintentionally, of course) that the jingle theory existed even in their day, and that they were believers in it. His words are that, when Jenner "used to air the popular fancy," "the medical men who had experience to guide them would good-naturedly produce case after case which showed that the popular belief, in so far as it was held even by the vulgar, was a mere verbal illusion."[1] Again, he says (p. 24)," the way was barred by the hard facts of experience, which country doctors, who knew far more of cow-pox than Jenner did, recognised in the way that sensible men always do recognise hard facts." This depreciation of Jenner, and magnifying of all who were opposed to him, or are imagined by Dr Creighton to have been opposed to him, is characteristic of the whole critique. Why the other medical men should have known "far more than Jenner" does not appear; and that they were "sensible men," Dr Creighton concludes simply because he supposes they agreed with his present opinions.

It happens that in 1798, shortly after the publication of Jenner's *Inquiry*, another *Inquiry* saw the light—the *Inquiry into the History of Cow-pox*, by Dr George Pearson, of St George's Hospital (who afterwards had a bitter quarrel with Jenner). Incited by Jenner's publication, Pearson wrote to a number of his medical acquaintances throughout the rural districts, asking what knowledge they had of cow-pox and its relationship to small-pox. The book contains the results of his investigations, and it is surprising to learn how many medical men, within a comparatively small radius of Berkeley, had formed on this point the same opinion as Jenner. The following I have noted:—Mr Bragge, Axminster; Mr Downe, Bridport; Dr Pulteney, Blandford; Mr Henderson, Wendover; Mr Giffard, Gillingham; Prof. Wall, Oxford; Mr Dolling, Blandford; Dr Croft, Staffordshire; Mr Rolph, Peckham (formerly of Thornbury in Gloucestershire); Mr Groves, Thornbury; Mr Wales, Downham (Norfolk); Dr Fowler, Sarum; and Mr Hughes, Stroudwater. Here is a sample of their evidence:—

---

[1] Pp. 162–3, *op. cit.*

> "Mr Rolph says there is not a medical practitioner of even little experience in Gloucestershire, or scarce a dairy-farmer, who does not know from his own experience, or that of others, that persons who have suffered the cow-pox are exempted from the agency of the variolous poison. The late Mr Grove [Mr Rolph's colleague] was a very extensive small-pox inoculator, frequently having 200 to 300 patients at one time, and the fact of exemption now asserted had been long before his death abundantly established, by his experience of many scores of subjects who had previously laboured under the cow-pox, being found insusceptible of the small-pox, either by inoculation or by effluvia. While Mr Rolph practised at Thornbury, he thinks not fewer than threescore instances of failure in attempting to produce the small-pox inoculation occurred in his own practice, all of which were persons who had been previously affected with the cow-pox. In almost all of these cases, the uninfected persons associated with those who took the small-pox, and many were repeatedly inoculated. Although Mr Rolph has not, in his recollection, any instances of people taking the small-pox who gave admissible evidence of their having laboured under the cow-pox, he thinks such cases may, and have indeed occurred to others, where the cow-pox had only been local."

It is noteworthy that this same Mr Rolph, so early as 10th June 1795, or three years before Jenner's *Inquiry*, wrote to Dr Beddoes of Bristol on the same subject, and in a similar though less decided strain.

So, too, Mr Bragge, many years before, had inoculated over 50 persons, of whom three had had cow-pox, and these "he therefore charged with an abundance of matter, but to no purpose." Dr Pulteney stated that "an intelligent and respectable inoculator" had informed him "that of several hundreds whom he had inoculated for the small-pox, who had previously had the cow-pox, very few took the infection; and such as did, he had great room to believe, were themselves deceived in regard to their having had the cow-pox." Mr Downe said, "A few years ago, when I inoculated a great number for the small-pox, I remarked that I could not, by any means, infect one or two of them, and on inquiry I was informed they had previously been infected with the cow-pox. Some few families who had been infected with the cow-pox were repeatedly inoculated with the matter of the small-pox, and without effect."

Evidence of this sort could be largely added to, but enough has been given to show how far from the fact is Dr Creighton's opinion that the legend was believed only by "credulous people," or "officious gossips," and not by those "who had some real practical knowledge of either or both diseases."

It is strange that all this adverse testimony receives no notice from Dr Creighton. But a still stranger thing remains to be stated regarding the evidence which he himself adduces. One man in particular he brings prominently forward as a witness. He says "some of Jenner's professional neighbours knew a good deal about it [cow-pox], particularly Mr Fewster of Thornbury" (p. 19). "The man who knew most about cow-pox sores in milkers was Fewster of Thornbury; and Fewster, as well as others, had unfortunately good reason to scout the milker's protection from small-pox as an old wife's fable" (p. 24). "Fewster, the chief authority on cow-pox" (p. 54). "Fewster and the rest knew there was nothing in it" (p. 55). Thus while Croft, Rolph, Wall, Giffard, etc., are all against him, our author can at least depend on "Fewster of Thornbury." We need not therefore quarrel with him if he tends to exaggerate a little the special knowledge and capacity of his chosen authority.

What, then, says Fewster, "the man who knew"?

Listen: "I can now with truth affirm *that I have not been able to produce the small-pox, in a single instance, among persons who have had the true cow-pox.*"

These are the *ipsissima verba* of "Fewster of Thornbury," italicised by himself! They occur in a letter from him, published in Pearson's *Inquiry*, pp. 102–3. And lest an isolated sentence should misrepresent his meaning, I give the whole passage:—

> "In the spring of the year 1768 I came to live at Thornbury, where I have resided ever since. In that very year, from the following occurrence, I became well acquainted with the disease called cow-pox. The late Mr Grove and myself formed a connexion with Mr Sutton, the celebrated inoculator; and to inoculate for the small-pox we took a house at Buckover. We found in this practice that a great number of patients could not be infected with the small-pox poison, notwithstanding repeated exposure under most favourable circumstances for taking the disease. At length the cause of the failure was discovered from the case of a farmer who was inoculated several times ineffectually, yet he assured us he had never suffered from the small-pox, but, says he, '*I have had the cow-pox lately to a violent degree, if that's any odds.*' We took the hint, and, on inquiry, found that all those who were uninfectable had undergone the cow-pox. I communicated this fact to a medical society, of which I was then a member, and ever afterwards paid particular attention to determine the fact. I can now with truth affirm *that I have not been able to produce the small-pox, in a single instance, among persons who have had the true cow-pox*, except a doubtful case which you are acquainted with. I have, since that, inoculated near two thousand for the small-pox, amongst whom there were a great number who had gone through the cow-pox; the exact number of these I cannot tell, but I know that they all resisted the infection of variolous matter."

Then Fewster goes on to state that he "never knew one mortal or even dangerous case" of cow-pox, but that at the same time he thinks it "a much more severe disease in general than the inoculated small-pox," and that therefore he does not see "any great advantage from inoculation for the cow-pox."

How are we to account for this extraordinary blunder on the part of Dr Creighton? Baron[1] says that Jenner had often told him that at the meetings of the local "Convivio-Medical Society," his brethren looked on the cow-pox rumour as a vague notion, and that most of them had met with cases in which persons "supposed to have had cow-pox had subsequently had small-pox." Dr Creighton quotes the passage. In the same page Baron gives the names of a few of the members, and among them he happens to include Fewster's. So far as I can make out, this seems the whole foundation for the misstatement. In general, Dr Creighton's distrust of Baron is only surpassed by his distrust of Jenner himself. He says (p. 3), "It is at this point that the Jennerian mythus begins in the pages of the biographer Baron," and on page 9 we read of "the biographer Baron, mythological as usual." As a rule, Baron's writings are of course strongly in favour of Jenner. Then, to Jenner's critic Baron is a mythologist. But when Baron makes the above statement, so little calculated even as it is to serve Dr Creighton's purpose, he seems eagerly to seize on it, and out of it to manufacture "the mythus," or "legend," or "old wife's fable" (there is a wealth of such terms in Dr Creighton's writings), that Fewster looked on cow-pox as entirely devoid of any anti-variolous property. So true it is, that "an hypothesis, when once a man has conceived it, grows the stronger by everything you see, hear, read, or understand."

JOHN C. M'VAIL.

(*To be continued.*)

A Handbook of Physical Diagnosis of Diseases of the Organs of Respiration and Heart, and of Aortic Aneurism. By R. C. M. PAGE, M.D., Professor of General Medicine and Diseases of the Chest in the New York Polyclinic, etc. New York: J. H. Vail & Co.: 1889.

THOUGH the number of text-books on physical diagnosis is already sufficiently great to make one rather critical of new competitors for public favour, we are inclined to give this volume by Dr Page a good place among the best works on the subject.

The author divides his book into six chapters. In the first of these he considers the chest in health, and the various methods of physical examination; in the second, those diseases, such as bronchitis, asthma, and emphysema, in which the convection of the breath and voice sounds may be obstructed in the bronchi and refracted in the lungs with corresponding diminution or suppression of fremitus, respiratory murmurs, and pectorophony; in the third, diseases in which the breath and voice sounds are conducted with more force than in health, with corresponding increase of the fremitus, and other signs, and the occurrence of bronchial breathing and bronchophony. Examples of these are found in pneumonia, consumption, and cancer, the physical signs of the different stages of which are discussed at length. In the fourth chapter he deals with the various affections of the pleura, in which the breath and voice sounds are interrupted in their transmission to the chest-walls, and in which there is consequent diminution or absence of the fremitus, respiratory murmur, etc. Chapter five is devoted chiefly to the consideration of adventitious sounds, and of changes produced by disease in the respiratory murmur, and the vocal resonance. In chapter sixth, the longest of all, the physical signs of the various forms of cardiac disease and of aortic aneurism are described.

---

[1] *Op. cit.*, vol. i., p. 48.

Such is a meagre outline of the contents of the book, which is well and clearly written. It cannot be said that there is anything novel in the author's teaching; but he has treated his subject, as a successful teacher should, in a lively and interesting way, and from a logical stand-point. He has not restricted himself to the consideration of signs and symptoms alone, but wherever he has thought it necessary, has discussed points in ætiology and pathology that help to elucidate the meaning of the physical signs. Errors are few in number, and of small importance, but Dr Page ought to know that the Edinburgh School cannot claim the honour of numbering Stokes among its teachers. That belongs to Dublin. Again, we are not acquainted with the details of Lawson Tait's earlier years, and therefore speak under correction, but we hardly think he can have been a pupil of Stokes' in 1835.

The illustrations and printing deserve a word of praise. Altogether we feel we are safe in recommending the volume to the attention of our professional brethren on this side of the Atlantic.

Annual of the Universal Medical Sciences: a Yearly Report of the Progress of the General Sanitary Sciences throughout the World. Edited by CHARLES E. SAJOUS, M.D., and seventy Associate Editors; assisted by over two hundred Corresponding Editors, Collaborators, and Correspondents. Illustrated with Chromo-Lithographs, Engravings, and Maps. Philadelphia, New York, and London: F. A. Davis.

THE Americans are a wonderful people, and their enterprize and industry in every direction are examples to us and to all men. Were it not for the Forth Bridge and Mr Lawson Tait we would be left entirely behind in the race. Their engineers and tourists drive to the first, and their gynæcologists worship the second of our celebrities. This second yearly issue of the *Annual* is a most marvellous example of industry and arrangement governed by common sense, and rendered possible by admirable liberality on the part of the publisher. It contains, in five large well-printed uniform volumes, the cream and kernel of the medical literature of the world for the year 1888. A marvel of ingenuity in arrangement; we have indices in each volume and a general index for all. Each writer gets his due credit; each journal finds its matter which is quoted referred to its source. It speaks most highly for the zeal of the profession in the attainment of recent knowledge that such a splendid work can command a sale sufficient to make it pay, and an immense amount of editorial oversight and sub-editorial work must have been done unselfishly and perseveringly to attain such a splendid result. No public medical library should be without this work, and every medical man who has to write much and to consult works of reference will find his labours lightened by its use.

Fever: A Clinical Study. By T. J. Maclagan, M.D. London: J. & A. Churchill: 1888.

This is certainly a disappointing work. The author's reputation as an authority on the febrile processes naturally led one to look for a philosophic and logical disquisition from his pen. Instead of this we find merely a fairly clear and intelligent statement of the outlines of our present knowledge of fever, with a curious theory in regard to its origin; but nothing throwing any new light on this much-vexed question. This appears to be due to a certain lack of broadness of grasp and of logical clearness, which leads the author to adopt views on altogether insufficient evidence, and to reject others without due examination.

At the outset he makes the grave mistake of not sufficiently recognising that heat is a form of energy, and not merely "a product destined for elimination," to be classified along with urea and carbonic acid. That this is not a mere mode of expression—as we at first thought—employed to emphasize an apparent parallelism of importance to the author's views, is sufficiently indicated by his failure to appreciate the full significance of Dr Ord's most suggestive observations.

Throughout, his criticisms on the work of other investigators is marked by this want of grasp. Especially is this the case in his discussion of Dr Wood's interesting experiments on fever due to the direct application of heat to the cranium. Here, as in many other parts of the book, one might almost believe that Dr Maclagan looked upon heat as the old materialistic "caloric."

The most important part of the work is undoubtedly that concerning the theory of fever.

The neurotic theory is first discussed, and while admitting that in many forms of thermal disturbance the nervous system plays an important part, the author holds that this theory is not universally applicable. His criticism of Dr Macalister's views is most unsatisfactory—in fact, it can hardly be considered a criticism, since it is entirely beside the mark, and has little bearing on the great question at issue—Is all fever due to disturbance, primary or secondary, of the nervous system?

The manner in which the nervous system can induce fever by increasing the metabolic processes is clearly explained; though we do not know upon what evidence the author forms his conclusion that, "Besides dominating heat production, the nervous system also controls heat inhibition, and by so doing keeps the temperature at the normal standard of health, and in disease prevents it from passing due bounds." As far as we are aware our evidence of the existence of anything like a general system of inhibitory nerve structures such as is here postulated is entirely wanting. All our present evidence points to the purely neurotic fevers as being the result of stimulation of discharging centres or augmentor nerve fibres rather than the paralysis of an inhibitory mechanism: witness the effect upon thermogenesis of complete section of the spinal cord in the neck.

In his statement of the metabolic theory of fever, the author again fails to weigh his evidence judicially.

The discussion of the view that the fever poison stimulates heat production leads him to the development of his own theory of fever, which, briefly stated, is that the micro-organisms in their reproduction and increase in the body consume the nitrogen and water destined for the nutrition of the tissues. This, he holds, will account for all the essential phenomena of fever.

The theory is certainly original, and would require for its substantiation strong and convincing arguments. These are wanting. In the first place, the author entirely fails to refute the usually adopted idea that the increased heat production in fever is *always* due simply to an increased metabolism of the tissue elements, whether induced, as most physiologists at present are inclined to hold, through the nervous system, or directly by the action of the fever poison on the active tissues of the body. He early shows how he is—we believe erroneously—led to reject this view. At page 3 he states pretty clearly that if fever is due to increased tissue waste, the febrile disturbance should be proportionate to the extent of the metabolism. This every one knows is not the case. But while he recognises the possible action of a disturbed relationship of heat production to heat dissipation, he states that the proportion between these is *not* upset in fever; and yet at page 5 he clearly points out how the high temperature so characteristic of the early stages of fever is to a large extent the result of such a disturbance.

When we turn, on the other hand, to the explanation of how his theory will elucidate all the phenomena of fever, the number of assumptions which have to be made, and the intimate knowledge of the practically unknown intrinsic metabolic processes of protoplasm which the author assumes, astonish but do not convince us.

We need only refer the reader to the explanation of the increased elimination of urea given on page 49.

To our mind the theory is absurd, and bears on the face of it its own refutation. How any one who has devoted any time to the study of fever could so entirely ignore the action of the *products* of the metabolism of the pathogenic organisms is astonishing. In the present work not a single allusion is made to these most interesting chemical products, which, apart from the organism producing them can exercise so powerful an influence on animal life.

Enough has been said to show the character of the work. Space does not admit of our noticing it further. It is to be regretted that Dr Maclagan has not digested and tested his views more fully before making them public.

A Surgical Handbook for the Use of Students, Practitioners, House-Surgeons, and Dressers. By FRANCIS M. CAIRD, M.D., F.R.C.S. Ed., and CHARLES W. CATHCART, M.B., F.R.C.S. Eng. and Edin., Assistant-Surgeons, Royal Infirmary, Edinburgh. With very numerous Illustrations. London: Charles Griffin & Co.: 1889.

THIS is a neat, compact little book of 262 closely but clearly printed pages, lavishly illustrated, admirably arranged with every luxury of italics, black type, small paragraphs, every arrangement that can catch the eye and give emphasis and point to the instruction; and the matter is as good as the manner. Any one who knows the authors is at once assured that all that painstaking, self-denying enthusiasm, combined with brilliant and experienced teaching power, can do to perfect the work has been lavished on the book. The result is the best practical little work on the handicraft of surgery we have ever seen. There may be faults; they are certainly not on the surface. The teaching has a peculiar savour of the Edinburgh School,—none the worse of that, all Edinburgh men will say. We can see on every page the influence of men still living, and of others (notably that great surgeon Spence) who have joined the majority. Among the best bits we had marked anæsthetics, the mode of administering chloroform, the whole chapter on emergency cases, and on tracheotomy.

One hint might possibly be useful for a future edition. Allusion is made (p. 77) to the occasional necessity of the performance of tracheotomy in cases of cut-throat in which laryngeal swelling and inflammation are either present or imminent. Before this is done, if the case is what is called a "police one"—and many are—be sure to have a consultation with a colleague, as if the case goes wrong the murderer's counsel is sure to say that your operation, not the original one by the murderer's knife or razor, killed the patient. The same rule holds good in trephining for head injury, for laparotomy, for bullet wounds of abdomen, and the like.

To point out errata is always kind. P. 16, line 3, "epithelium" is printed for "epithelioma." In page 54, 13th line from foot, "tampoon" is printed, surely a new spelling of "tampon." If the French word tampon is to be Anglicized, it should be either tampion or tompion.

On the whole, this little book will be a great boon to the student of surgery, and we wish it every success.

Archives of Surgery. Vol. I., No. 1. July 1889. By JONATHAN HUTCHINSON, LL.D., F.R.S. Published Quarterly. London: J. & A. Churchill.

MR HUTCHINSON is an experienced and learned surgeon, of great industry, who has had exceptional opportunities, both in hospital work and private practice, of maturing his opinions on the basis of a vast experience of carefully observed and elaborately noted cases. The profession has already had the benefit of some of his work in his large *Atlas of Clinical Surgery.* This new journal is to be edited, and in the main written by himself. From the first number we are justified in believing that it is to be a most practical and useful collection of experience. The leading article, on Records of Intestinal Obstruction, is a much-needed and most characteristic protest against the tendency, so marked at present, in the direction of operative treatment as early as possible by exploratory laparotomy. Mr Hutchinson pleads the cause of abdominal taxis as a curative measure, and describes it most clearly and dramatically. Forty pages of the volume consist of most excellent, rare, and practical cases, which in the present day are much more likely to fall into the hands of a specialist rather than of a surgeon.

Mr Hutchinson is a surgeon first and a specialist afterwards. He has made his name well known, not only in general surgery, but also in the special subjects of eye, ear, syphilis, skin, and nervous system. In fact, he may be said to be a general surgeon and five specialists rolled into one. In our opinion, his patients have a better chance because treated by one with a general experience of life and disease. An oculist may by doing little else than extract lenses do it better than a man who also has to amputate a leg, but he will certainly not cure a constitutional choroiditis by confining his attention exclusively to the eye. There is an odd little Catechism of Surgery, with cases for diagnosis for students. Possibly many of the answers which are also given might not satisfy all examiners of the present day, but they may be a useful form by which a student may test his own knowledge of his work.

There are some excellent coloured plates of rare diseases, on a smaller scale but on the same lines as the ones in the large *Atlas of Clinical Surgery.*

We wish this new venture every success, and Mr Hutchinson health and strength to continue it.

Masso-therapeutics, or Massage as a Mode of Treatment. By WILLIAM MURRELL, M.D., F.R.C.P. Fourth Edition. Pp. vi., 230, and Index. London: H. K. Lewis, 136 Gower Street, W.C.: 1889.

UNDER another name this little book has been twice before referred to by us (Vol. xxxii., pp. 730 and 1021). Dr Murrell has thought well to change its name and to call it masso-therapeutics. This change is explained in the preface as an attempt to take the subject out of "a means of earning a living or as a modified form of hotel-keeping" into the "scientific aspect of the subject." If reports which reach us from London are correct, the author does well to make the endeavour. Massage is no mystery, and any physician can prescribe it and see it well carried out if he takes the trouble to obtain a proper knowledge of the subject. It is, however, a method of treatment which readily lends itself to quackery. It is a great mistake to suppose that all nurses who call themselves rubbers are anything of the kind, and it is only after good training and considerable practice that the art can be acquired. It will be well for all physicians who wish to prescribe massage to read this book, as they will find in it many useful and serviceable hints both as to when it should be and when it should not be made use of.

The Bombay Materia Medica and their Therapeutics. By RUSTOMJEE NASERWANJEE KHORY, M.D. Brux., M.R.C.P. London, etc., etc. Pp. 600, and Index xxxix. Bombay: Printed at Ráníná's Union Press: 1887.

Dr KHORY must be congratulated upon the result of his painstaking endeavour to place before the profession in India a mass of information little known and still more inaccessible. The book is all the more interesting, because it is printed and published in Bombay, and it forms a very creditable piece of native typography; but in the next edition the author will do well to read the proofs more carefully, as there are a considerable number of errors in spelling, etc., which ought to have been avoided. The book is composed of eight sections—1. A description of the preparation of drugs and of the manner in which they are used in different diseases; 2. Classification and description of inorganic bodies; 3. Zoological classification and description of animals; 4. Botanical classification and description of plants; 5. Classification founded on physiological effects of drugs; 6. An alphabetical classification of diseases with the number referring to the prescriptions given in the body of the work; 7. An alphabetical classification of drugs used as substitutes for other or English drugs; and 8. An alphabetical classification of drugs used as specifics in certain diseases. It will thus be seen that the author has sought to work out his subject thoroughly. One very useful feature of the book consists in the synonyms of the bazaar drugs and the vernacular names in the different languages, for it is often difficult for a physician wishing to use native drugs, either on account of failure of his own stock or for the sake of lessening expense, to obtain readily their accurate designation. Many of the drugs mentioned in this volume will be new to practitioners in this country; many, indeed, are not contained in the Indian Pharmacopœia, and one of the author's objects in publishing this volume is to make a contribution to the history of native drugs, and to form a basis for further researches in indigenous medicines. We do not think, however, that the author is wise or justified in trying to popularize the native Materia Medica for the general public. We cannot help thinking that this book will prove very useful to physicians, both native and European, who are in outlying or inaccessible stations, as they will find in it many hints which will enable them to investigate the effects of drugs which for ages have apparently rendered good service to native hakims.

Headaches: their Nature, Causes, and Treatment. By WILLIAM HENRY DAY, M.D., Member of the Royal College of Physicians of London; Physician to the Samaritan Hospital for Women and Children. Fourth Edition. London: J. & A. Churchill: 1888.

THIS new edition of a standard work has undergone thorough revision, some parts of the book are rewritten, and the whole has been somewhat enlarged. The newest opinions are referred to, and the most recent methods of treatment find a place in this fourth edition. The work may be recommended as entirely deserving of the support of the profession which it has so long enjoyed.

A Guide to the Instruments and Appliances required in Various Operations. By A. W. MAYO ROBSON, F.R.C.S. Eng., etc. London: J. & A. Churchill: 1889.

A VERY handy reference for the guidance of hospital assistants, students, and practitioners. Mr Robson leaves a space at the foot of each list of instruments for each surgeon to add what he thinks is required. The book is small and compact, and is therefore more useful and convenient than the large sheets which we have seen.

The Therapeutics of Kronenquelle Water. By PROSSER JAMES, M.D., M.R.C.P. London: Trubner & Co.: 1889.

THE chemical composition of this mild alkaline water, its physiological action and therapeutic uses, are first considered. Dr James holds that it should be regarded as directed against the uric acid diathesis. Some useful hints as to the best methods of administration and the diet for patients during its course are also given.

Graphic Clinical Charts. Designed by H. HANDFORD, M.D., M.R.C.P., Physician to the Nottingham General Hospital. London: Wodderspoon & Co.: 1888.

THE great feature of these charts is that they show the rate of the respiration and pulse, as well as the height of the temperature, in a graphic manner, the facts being recorded on the chart in different colours: pulse, red; breathing, green; temperature, black. By calculation the phenomena of the circulation and respiration are correlated with the temperature, and from some experience of these charts, we can vouch for the care with which this has been carried out, and the excellence of the result. In other respects the charts are worthy of high commendation, spaces being arranged for noting almost all the usual points of a case. We can cordially recommend these charts as the best with which we are acquainted.

Warren's Table Book, containing Tables of the Weights and Measures used in the British Empire. By Rev. ISAAC WARREN, M.A. London: Longmans, Green, & Co.: 1889.

AN excellent little compilation, and all the more valuable that the metric system of weights and measures used in scientific inquiries is also given, as well as an account of the calendar.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXVIII.—MEETING VII.

Wednesday, 6th *March* 1889.—Prof. CHIENE *in the Chair.*

I. ELECTION OF NEW MEMBERS.

The following gentlemen were elected Ordinary Members of the Society:—John Berry Haycraft, M.D., D.Sc., F.R.S.E., 20 Ann Street; Henry Harvey Littlejohn, M.B., C.M., B.Sc., 24 Royal Circus; Albert Edward Morison, M.B., C.M., Hartlepool; William George Sym, M.B., C.M., 58 Queen Street.

II. EXHIBITION OF PATIENTS.

1. *Dr W. A. Finlay* showed a boy, æt. 12 years, on whose left foot he had performed MICKULICZ'S OPERATION. It was done for disease of the tarsus, and the result was a success. He was debarred from performing Syme's amputation by the presence of an ulcer on what would have been the heel flap. The operation

was performed seven months and a half ago, on 16th July 1888. The union of the bones is now firm, and the boy can walk well and with ease. The limb operated on is half an inch longer than the other, measuring to the heel in the right leg and to the ball of the great toe in the left.

2. *Mr A. G. Miller* showed a case of FRACTURE OF THE BASE OF THE SKULL, and read the following notes:—

Case of Fracture of Base of Skull.—A. H., female, æt. 19. Admitted to Ward XVII. on 6th Feb. 1889.

History, obtained from others, for patient has no recollection of accident. Patient was hanging out clothes at window when the pole on which she was leaning broke, and she fell a distance of three stories.

On admission patient was insensible, and suffering from condition of so-called concussion. There was a scalp wound, about 2 inches long, on *left* side posteriorly. There was bleeding from the right ear, and ecchymosis of right eyelid. No subconjunctival hæmorrhage. (Temperature was persistently subnormal.)

Diagnosis.—Fracture of base of skull oblique from behind forwards towards the right.

On the day after admission patient's respiration became sighing. On suspicion that the origin of the vagus nerve might be irritated the urine was examined, and the usual reactions indicating the presence of sugar were noticed. The glycosuria and sighing respiration disappeared after twenty-four hours. Patient recovered rapidly.

3. *Dr James Carmichael* exhibited two patients who had recovered from PURULENT PLEURITIC EFFUSION, treated by aspiration.

CASE I.—That of a girl, aged 8, who was admitted to the Royal Hospital for Sick Children 24th October 1888, and dismissed 14th January 1889. On admission the signs of fluid at the base of the right chest were well marked. She weighed 34 lbs. on admission, and 41½ lbs. on dismissal. Two days after examination five ounces of thick creamy pus were drawn off from the posterior part of the chest below the scapular angle. She experienced some, but only partial, relief, and for several days was much troubled with paroxysmal cough. On the 30th inst., or four days after tapping, she expectorated a quantity of muco-purulent matter, with manifest alleviation to her symptoms. The chest was subsequently explored with the hypodermic needle at several points, and no pus found. She did not improve during the next fortnight, the symptoms pointing to reaccumulation of fluid in the pleura. Exploration failing to find fluid anywhere posteriorly, the needle was introduced anteriorly in the fourth intercostal space and pus found, about 2 ounces being drawn off. She still expectorated at intervals small quantities of muco-purulent matter during the paroxysmal fits of coughing. The subsequent progress of the case showed gradual and steady improvement as regards general symptoms and physical signs, and she may now be seen to have made a perfect recovery. The diameter of her right chest is two-thirds of an inch less than the left.

CASE II.—A boy, aged 8, admitted 28th November, dismissed 14th January. He had been subject to bronchial cold in the winter for several years, but was otherwise well, not having suffered from any of the acute diseases. A fortnight before admission he returned home from school soaking wet, and thereafter suffered from cough and debility. On admission he was ill nourished, and weighed 38 lbs. There were well-marked signs of limited effusion quite at the base of the right lung posteriorly, and on introducing a needle some thick curdy-looking pus was drawn off. He suffered from slight cough and some sweating at night, but was otherwise well. He was put on maltine with cod-liver oil and generous diet. On 4th December the physical signs having only slightly improved, a needle was introduced in the same situation, and a similar quantity of pus came away. A week afterwards exploration was tried, and no pus discovered. From this time the physical signs steadily improved, and the patient was dismissed, having increased in weight 4 lbs.

These two cases, it appears to me, are of interest, not in showing anything new in the clinical features of this disease, but in demonstrating the fact that purulent collections in the pleura can be got rid of effectually, although exceptionally, by aspiration alone. In Case I. the occasional complication of pleural perforation and spontaneous discharge of pus per orem is demonstrated, and it may be questioned whether in the absence of Nature's assistance in this way, aspiration would have been successful. Such spontaneous discharges of pus in empyema are not in my experience very uncommon. As Trousseau long ago pointed out, the occurrence is generally due to ulceration of the pleura over small localized pneumonic areas. In this case also the cavity was bilocular.

III. EXHIBITION OF SPECIMENS.

1. *Mr A. G. Miller* showed—

(*a*.) A CAST of a dislocated knee-joint and parts removed by operation. The patient from whom these were taken was a male, æt. 19, from Aberdeen.

Admitted to Ward XVIII., 21st February 1889.

History.—Four years ago patient, when jumping into a cart, struck his left knee against the side of the cart. The limb at once assumed the appearance it presented on admission. Patient was able to walk and bear his weight on limb. Very little seems to have been attempted in the way of treatment for two years, during which time the lad was able to do work. During the last two years several attempts have been made to improve the form and mobility of the joint.

On Admission.—The left knee-joint is flexed and almost completely anchylosed. There is some swelling and pain on movement. There is a displacement of the tibia outwards, with outward rotation, so that the fibula lies behind. The patella is fixed.

Diagnosis.—Old dislocation of tibia outwards, with possibly fracture of the external condyle of femur. Any attempt at reduction being, of course, out of the question, excision of the joint was advised, and agreed to by the patient.

On 23rd February the knee-joint was excised, when the dislocation was made very plain, and evidence of commencing tubercular disease discovered. There is a mark on the external condyle, which looks very like a partial fracture.

(*b*.) A PORTION OF INVAGINATED BOWEL removed after two days' obstruction in a gangrenous condition. This was from a boy æt. 12.

Admitted 27th February 1889.

History.—On 25th, patient was preparing to go to school when he was suddenly seized with pain in the abdomen on the right side, immediately after which he passed a loose motion in the w.-c. (patient cannot say whether there was any blood.) There has been no motion of the bowels since. Patient then began to vomit. Vomiting became fæcal on second day, and continued up to time of admission. He has had an anxious expression ever since illness commenced.

On Admission.—Abdomen moderately distended and slightly tender. Tympanitis not marked. A firm elongated mass is easily felt in right iliac fossa extending upwards into the lumbar region, and about four or five inches in length. Nothing abnormal felt by rectum. Patient very weak and collapsed.

Diagnosis.—Intussusception into ascending colon. Immediate operation was recommended, and at once agreed to by parents, who accompanied patient.

On opening abdomen intussusception was at once discovered. Attempts at undoing the invagination failed completely. The portion of invaginated bowel was at once excised and removed. As the patient was in a very feeble condition, the bowel openings were secured to the external opening in the abdominal walls for the formation of a fæcal fistula. Patient sank, and died in the course of the evening.

A very fine cast by Mr Cathcart showed the condition of parts when removed.

2. *Mr Cathcart* showed a BLADDER taken that day from the body of an old woman who was admitted in a moribund state to Mr Miller's ward, supposed to be suffering from a fracture of the neck of the femur, which it was found, however, that she did not have. At the post-mortem the interior of the bladder was found to be simply a clot of blood. Her only bladder symptom

during life was incontinence of urine. She had not passed any blood. It was evident that there was an infiltration of blood into the walls of that viscus. The explanation of this remarkable condition was not clear.

IV. ORIGINAL COMMUNICATION.

Dr James Foulis read his paper, entitled, THE ONLY WAY OF RAISING THE TONGUE, OF RAISING THE EPIGLOTTIS, AND OF STRETCHING THE ARYTENO-EPIGLOTTIC FOLDS AT ONE AND THE SAME TIME, which will be found at pages 19 and 132 of this Journal.

Mr John Duncan said the subject of this really admirable paper was one on which those who had experience in the giving of chloroform might dilate for hours. When Mr Howard did his spread-eagle business in London the other day, they should all have been prepared to accept his paper as a well-reasoned exposition of the anatomical facts connected with the subject of respiration under chloroform; but some of them would take very strong exception to his "new and only," and he (Mr Duncan) rather regretted Dr Foulis's use of the word only. Mr Howard's method was not new, nor was it the only one, nor was it very good. The practical application of his well-reasoned observations was not satisfactory. He supposed that under chloroform the risks came solely from the respiration, and they came in various ways. There was a way in which the respiratory act simply died out without any obstruction to the respiration. With that method of death (it was very important to distinguish the various methods) they had not then to deal. Another method with which also they had little to do was the risk of solids and fluids getting into the air-passages, even thin fluids lying in the back of the pharynx having a great influence. The modes of death with which they were concerned that evening were those in which obstruction took place from a paralysis causing an obliteration of the upper air-passages. He supposed this occurred in two ways. One was paralysis of the intrinsic folds at the opening of the larynx, in which a sort of valvular action took place, and the other, to which Dr Foulis drew their attention, was the falling back of the tongue and blocking up of the pharynx. Now these two conditions might be easily distinguished in the experience of any one who had seen risk arising from them, and who had adopted the method of pulling up the tongue. In the one case the arrestment of respiration was relieved by the tongue being pulled slightly forwards. But in the other class of cases, in which there was more complete paralysis of the tongue and of the orifice of the larynx, they required to pull very forcibly on the tongue to relieve it. They now came to the question of how best to overcome those difficulties. He was prepared to maintain that they were both capable of being overcome by pulling on the tongue from the tip; and they were also capable of being overcome, though not so completely, by pushing forwards the angles of the jaw; and he was prepared to admit that Howard's method, if carried out as he described it, was also generally effectual in doing the same. How the pulling on the tip of the tongue acted he was not prepared to say, but he had suggested to Dr Foulis that it might be by putting the fibres of the genio-hyo-glossus and other muscles on the stretch, and so, both mechanically through traction on the jaw and reflexly by causing them to contract, raising the hyoid bone. From all he had seen and heard, he was strongly inclined to adhere to the old method, which was extremely effective, and required no instrument beyond what was in their pocket-cases, and really produced very little injury to the tongue. He had, at the same time, no doubt Dr Foulis's instrument was perfectly effective. As to the modes of artificial respiration, he immensely preferred Sylvester's method to Howard's. He believed the air entered in greater volume and was changed more rapidly in the former than in the latter method.

Dr M'Bride failed to see how in any narcosis the ary-epiglottic folds could be paralyzed, and how even if they were they could affect the respiration. The chief obstacle lay in the base of the tongue, and the epiglottis falling against the posterior pharyngeal wall. Theoretically it seemed to him that any of the methods they had discussed would be efficacious in getting these apart.

Mr Cathcart pointed out that in addition to the deep guttural sound emitted by patients under chloroform there was another of a crowing character, which seemed to support Sir Joseph Lister's view that the obstruction was sometimes in the larynx. He ventured to disagree with Mr Duncan's opinion that pulling on the tip of the tongue was in every case successful. He had seen cases in which the patient was in imminent danger, in which efforts to pull forward the tongue had failed to give relief. He observed this in one instance in Mr Joseph Bell's hospital practice, in which after tongue-pulling failed the finger passed down to the epiglottis and hooking forward the tongue was of immediate benefit. If the advantage of pulling forward the tongue were that of exciting reflex action, one could imagine that in deep narcosis the reflexes might be abolished, and in such a case the advantage of Dr Foulis's mechanical method would be apparent. With regard to the methods of artificial respiration, he was inclined to think Sylvester's superior to Howard's. In the latter expiration alone appeared to be produced, whereas in Sylvester's the raising of the arms above the head produced inspiration and the lowering of them to compress the chest-walls expiration.

Dr James Ritchie had seen cases like those referred to by Mr Cathcart, in which pulling the tongue forward failed to give relief, but pulling up the jaw did. In contrasting the methods of artificial respiration, he thought Mr Cathcart had overlooked the fact, that in the Howard method the arms were fixed above the head in such a way as to place the chest in the condition of full inspiration. One of the great advantages of the Howard method was that, if it had to be undertaken single-handed, it could be kept up without fatigue for a very much longer period than Sylvester's, and therefore it was a suitable method to teach the ambulance and swimming classes.

Mr A. G. Miller said the practice of pulling forward the tongue had originated with a house-surgeon of his father. A slight operation was being performed, when the respiration suddenly became embarrassed. The house-surgeon surmising that the tongue had something to do with it, seized the tip with a pair of Liston forceps, and the breathing was at once relieved.

Dr Symington showed, in connexion with Dr Foulis's paper, two horizontal sections through the head of a male subject, made while the body was frozen and lying on its back. These showed the parts referred to in the paper. They showed that after death the tongue was very apt to fall back just as in chloro-narcosis. A considerable part of the dorsum of the tongue was lying in contact with the posterior pharyngeal wall, and the pharynx itself for about an inch above the epiglottis was little more than a transverse slit. It seemed to him that even a comparatively slight pull on the tip of the tongue would move it forward sufficiently to turn this transverse slit into a considerable cavity. So far as Dr Foulis's method went, it seemed to him an admirable one, not only by drawing forward the tongue, but by keeping it there. He did not think Dr Howard's experiments with water in the fossæ between the tongue and the epiglottis established anything. He agreed with Mr Cathcart as to the obstruction being sometimes in the larynx, and the experiments of Dr Wyllie helped them to understand that. In the majority of cases he believed the obstruction was due to the tongue falling back.

Dr P. A. Young said he was interested in Dr Foulis's paper from an ambulance point of view. Ambulance students were generally taught the Sylvester method of artificial respiration, although they were also sometimes instructed how to employ Howard's method. He thought that while the glossotilt might be an admirable instrument in skilled hands, it would prove dangerous if used by ambulance pupils. He did not think that Dr Foulis had correctly represented the Howard method of artificial respiration. The arms ought to be raised and fixed above the head.

Drs Mackenzie Johnston and *A. D. Webster* also made remarks on the paper.

Dr Foulis, in reply, said,—With regard to any supposed injury to soft parts in using the glossotilt or handle of a tablespoon, he had not met with an instance of such during the last nine years, and did not see how such could occur, as only the slightest amount of force was necessary to raise up both the tongue and epiglottis. Far less injury was done to the soft parts with the glossotilt than with the catch forceps, for in using this last instrument the tongue must be dragged upon violently if the method is to be effectual, and, according to some gentlemen who had spoken to-night, it was not always effectual in relieving the breathing even then, but it injured the tongue. As regards artificial respiration, Dr Foulis would again say that Howard's method was the best of all; and as to the question of the quantity of air pumped in and out of the chest in that method, he would like to point out to Dr Cathcart that as much air rushed in as was pumped out by the act of compressing the chest. All other methods were very exhausting to the operator, while Howard's method was not so, and could be carried out with ease for hours at a time. It was surprising how large a volume of air could be made to rush in and out of the chest by Howard's method when the tongue and the epiglottis were kept out of the way by means of the glossotilt; and he would ask gentlemen present to give this method a trial before condemning it. He would also point out that in artificial respiration air should be pumped in and out of the chest as rapidly as possible at first, but that as soon as signs of natural breathing were established, the artificial movements should accompany the natural movements of the chest. In conclusion, Dr Foulis said that from all he had heard from the speakers that evening, he thought he was quite right in describing his method as the only way of raising the tongue, of raising the epiglottis, and of stretching the aryteno-epiglottic folds at one and the same time.

SESSION LXVIII.—MEETING VIII.

Wednesday, 20th March 1889.—Dr JOHN SMITH, *President, in the Chair.*

I. EXHIBITION OF PATIENT.

Dr Caverhill showed a case of ADDISON'S DISEASE in a young German commercial traveller. Within the last eighteen months he had developed a bronzing of the skin, which was well marked on the face, gums, epigastrium, and genital organs. There was a history of a severe wrenching of the body while skating some sixteen months ago. He had no special feeling of weakness, led an active life, and his weight, which was rather small, was increasing.

II. DISCUSSION ON THE PATHOLOGY AND TREATMENT OF STRUMOUS GLANDS.

The President.—The business to-night is a discussion on the pathology and treatment of strumous glands, and I daresay it will be admitted that this is an affection of sufficient interest, frequency, and importance to warrant a special meeting of the Society such as has been called for this evening. I hope that as many gentlemen as possible will give their opinions or their experience in the matter. Many different views have been entertained of the pathology of these lesions,—an inherited instability of tissue in certain organs prone to be attacked, and among these especially the glands and lymphoid structures; the inability of such weakened structures to resist the effects of inflammatory processes, coupled with low recuperative power; the influence of what is called scrofula, with or without the agency of tuberculous elements; the action of bacilli,—all these and their effects upon the gland tissues present themselves for consideration on the pathological side of the question, while the treatment of a malady leaving not only disastrous constitutional changes as its after-effects in some cases, but the extremely unsightly cicatrices so often visible, equally render it an interesting subject for attention. I call upon Professor Annandale to open the discussion.

Professor Annandale.—Mr President and Gentlemen, you have done me the honour to ask me to be one of the leaders in this important discussion. I feel placed in a rather responsible position. It is, I think, the duty of one placed in my position to-night to endeavour, as far as possible, to indicate and limit the lines of discussion; but in connexion with this subject I feel it may be somewhat difficult to do so. There are so many side issues which are tempting to pass into; but I shall do my best to keep to the main lines. Necessarily I shall have to refer to the minute or microscopic anatomy and pathology of this subject. I do not wish to say very much about this, for there are gentlemen here who have a much more complete knowledge than I have. There are two favours I should ask. The first favour is, that if in my remarks I say anything inconsistent with recent pathology, they will at once correct me. The second favour I have to ask is, that in their remarks they will endeavour as much as possible to summarize or arrange their facts, so as to be useful in connexion with the diagnosis and treatment of this disease. At the very commencement of this discussion a difficulty arises which will occur to most of you, the definition of the term "strumous glands." I believe, and hope I am correct in stating, that the word "struma" is now considered synonymous with tubercle or tubercular disease, but unfortunately this term is still used in a wider and more indefinite sense, and this constitutes one difficulty. Another difficulty in my mind is, that even if we take what is termed tubercular by pathologists, it does not include a certain class of case, known to observers, which resembles the tubercular affections in its behaviour and yet is not tubercular. I will refer, in the first place, to what is considered now as tubercular affection of the glands. Clinically, I take it that a tubercular condition of the glands is considered to be that in which glands in certain situations become enlarged. The glands in the neck, the glands in the thorax and abdomen, are from experience known to be much more frequently affected than glands in other situations. One explanation has been given, that in connexion with the naso-pharynx, lungs, and intestines, there is a greater amount of adenoid tissue in these parts, and it is in connexion with irritations of these tissues that the glands are more often enlarged. It is quite possible there is another explanation; that we may find that some of these micro-organisms are more commonly connected with the naso-pharynx, lungs, and intestines, and may have an influence in producing this tubercular condition.

In the next place we have the fact that one or more glands may be affected—sometimes one, sometimes several. Sometimes one gland appears to be a source of infection to others, the infection being carried along the small arteries, the lymphatics producing a more or less general tuberculosis. Then, again, it is a known fact, acknowledged even by pathologists, that the tubercular condition is not unfrequently simply local; a well-marked tubercular condition, examined microscopically and also observed clinically, may remain local; whereas, in other cases, the condition gradually develops into a general tuberculosis from infection. In connexion with the microscopic pathology of tubercle, I understand that the diagnostic and distinctive symptoms, pathologically, of tuberculosis are,—Firstly, the presence of a certain grouping of cells—giant cells; secondly, we may have the presence of the micro-organism or bacillus; lastly, the condition of caseation. These are the facts pathologically in regard to tuberculosis. When we come to consider the affections of the glands clinically—and I do not refer to special affections such as cancer or syphilis—we certainly meet with conditions which very much resemble those conditions usually termed tubercular, but are certainly not tubercular; and therefore it occurs to me that any term, such as scrofula, struma, or tubercular affection of the glands, is a matter of confusion. I cannot help thinking it would be better simply to look upon all these affections of the glands as inflammatory, and I believe most authorities are agreed that all these processes—even tubercular—are of the nature of an inflammation. Well, then, I would suggest that instead of using the word "tubercular" gland or "scrofulous" gland, we use the general word adenitis, and by careful observation and pathological research we endeavour to specify or classify the different forms of adenitis. It will probably come to be that we shall have a simple suppurative adenitis, tubercular adenitis, scarlatinal adenitis, etc. Well, then, I should prefer, in the present state of our knowledge—certainly by no means perfect—that we should class these affections of the glands as simply "inflammatory," and by careful research, both clinically and pathologically, endeavour to classify the different forms of adenitis.

Next, the important question of causation. I prefer to consider the question not as simply tubercular, but that we meet with a certain group of inflammatory affections of the glands modified by certain circumstances. And these circumstances are—(1), The conditions of the patient (and we do not know yet what all those conditions are); (2), local irritation of various kinds; and (3), the special tissue or organ, and, in connexion with the glands, the special glandular texture in which the disease originates must be taken into consideration. If we do this we are in a better position to gain light by further observation. In regard to the causation, the causation of these conditions may be divided, in the first place, into local causes. These local causes are very much more numerous than is generally supposed. All forms of irritation will produce the inflammatory conditions of glands as we know, but still there are special forms of irritation that undoubtedly produce some of those causes in the group to which I have referred. We have always to remember that in addition to local causes, we have the general or constitutional cause —the condition of the patient. There are many of these local causes which in a healthy patient would have no effect, but in certain conditions of health with which we are not thoroughly acquainted, the various local irritations which, perhaps, would cause very little effect in a healthy patient, will cause a glandular enlargement when that special constitutional condition is present. Then in regard to the organ or tissue in which the affection arises or exists, we must always take into consideration, I think, in studying the disease, the organ or tissue in which this disease exists; because every organ or tissue has its function in the human body, and we can quite understand in the case of a gland, which has certain functions to perform, and is liable to certain special forms of irritation, the glands may be affected by some general condition of health affecting the function of that gland.

Now I pass to a matter in which I feel more at home, and that is the treatment; but in connexion with the treatment one has to remember the causation, and the first point in connexion with the treatment of these affections—and, mind, I do not limit my remarks to simple tubercular affections, I rather prefer to look upon the subject as consisting of a group of cases not always tubercular. The first remark I shall make is, that every case ought to be carefully studied by itself, because I believe there has been far too much routine in practice. We all know what the usual practice is in the waiting-rooms of the Infirmary or elsewhere. The patient presents himself with some enlargement of the glands of the neck, and, without much study, it is declared to be a case of strumous affection of the glands, and cod-liver oil internally with iodine externally ordered. But every case ought to be considered by itself. Every case has usually a special local cause and a general cause. The first thing to be done in treating a case of affection of the glands (I prefer to call it inflammatory affection) is to carefully study the case, and more particularly to endeavour to find out whether or not there is any local irritation which can be removed. I have, over and over again, seen in private practice delicate young children, or even adults, suffering from enlargement of the glands of the neck; and these enlargements would at first sight be very naturally pronounced strumous or scrofulous or tubercular, but on further examination I have found that these were cases which depended upon a local cause. Quite true, the condition of the patient may have made the affection more liable to occur; but the proof that it was the local cause was that whenever that was removed the glandular affection got better. I will mention one cause, and that is irritation connected with the teeth—not necessarily decayed or bad teeth, but "jammed" teeth, teeth too crowded together. I have seen one or two cases in which the glands have remained enlarged for months and months, and have been treated as strumous or scrofulous, and yet the glands have gradually got well after teeth have been extracted and tension relieved.

I think it also important in connexion with the disease of the cervical glands that the whole naso-pharynx and the ear should be carefully examined, because these local conditions are probably the cause of the irritation of the glands. Then, having got rid of this question of local irritation, we come to the question of general treatment. This must not in all cases be cod-liver oil or even maltine; but the patient's general health should be investigated, and treated according to its condition. When the surgeon is called upon to see a case of enlarged or diseased glands, one very important thing is to find out whether or not there is suppuration, and if there is suppuration, I have no hesitation in saying that that should be at once relieved. But even before the stage of suppuration is reached, I have also no hesitation in saying that, if a case of enlargement or disease of the glands presents itself, local treatment, more especially soothing treatment, should in the first place be tried, provided there are no signs of suppuration; then counter irritation should be used. If that does not relieve—more especially if there should be tension—puncture, done with a tenotomy knife, is often very valuable. In our present state of knowledge, I have not much faith in injections, simply for this reason, that, as far as pathological and clinical knowledge go, I do not think we can diagnose whether a gland is in a tubercular state or in one of mere simple inflammation by simple external examination. If a gland is well marked with tubercular disease, I think no injection of iodine, or no injection of anything else, will be useful. So that if soothing treatment, if the removal of the cause, if the treatment of the constitution and general health, if the application of soothing means locally, or counter irritation fail, I think puncture should be tried by introducing a Graefe knife at two or three points. At the same time, in all these cases I should be careful in watching for suppuration; and as soon as suppuration is detected an incision should be made—in the first instance limited—and this allows you to examine the gland; and in the majority of cases you will find the gland is diseased. In other cases you will find the gland itself suppurating; and if this is present, the sooner you get rid of the gland the better, and it should be got rid of entirely. You can sometimes scrape the diseased gland away, and I would say the scraping must be thorough. The ordinary Volkmann spoon is not always the best instrument, because in many of these cases you will find that the gland is situated in some depth; so that you will require an instrument more or less curved, not simply a straight one, but a curve which will allow you to get thoroughly in and scrape away at the entire gland. There are other cases in which excision of the gland is advisable, and I think that it is so in cases where the gland is not very adherent or has resisted all treatment, or in cases in which the gland has remained for a long time without undergoing any change. In all these cases I believe, provided the gland can be excised safely, that the complete excision of the gland is the best thing that can be done to the patient, so that the treatment which I advise is as follows:—In the first place, remove any local cause; in the second place, treat the general health not simply by cod-liver oil, but by remedies suitable to the particular case; in the third place, apply soothing treatment to the gland itself. If that does not relieve, try puncture, always examining for suppuration; and if that takes place, the entire removal of the gland by scraping. The last point in connexion with the treatment I would refer to is the reparative treatment—that is to say, the removal of cicatrices or the removal of deformity the result of cicatrices. I have had to operate on a very large number of these cases. There are two methods of operating (and I would remark that it is important that the whole of the diseased gland should have disappeared. There is no use attempting to remove cicatrices or deformity until all inflammatory action has subsided). There are two methods of treatment—the first, the method of Mr Adams, who suggested that you should introduce a tenotomy knife and separate the cicatrix from its adhesion to the subjacent textures. But I do not think that this plan is successful when the cicatrix is extensive. I have frequently cut out the cicatrix. It is very fortunate that many of them are in the direction of the lines of the neck. What I advise and practise is to cut out the cicatrix, and then to carefully bring together, by small sutures of horsehair or other substances, the edges of the wound.

Mr Duncan.—I am in an unfortunate position, because I was unable to hear the earlier parts of Mr Annandale's speech. I am sure the Society will excuse me when I tell them that I was called to the Infirmary, and have been detained there. The subject is one which cannot of course, in the time which one may legitimately occupy, be at all exhaustively considered. I shall merely endeavour to refer to one or two points on which Mr Annandale has not dilated to any extent. Taking first the pathological aspect of strumous glands, I suppose that we may nowadays safely enough consider that the disease is really due to the action of the tubercular bacillus, and that the difficulty to which Mr Annandale referred is truly one of diagnosis. Necessarily, therefore, the pathology of strumous glands includes the whole pathology of tubercle. I shall leave that to the pathologist; but there is one point in the etiology which seems to me interesting and important, viz., supposing, as I have supposed, that they are due in every instance to the presence of the tubercular bacillus, how did the bacillus get there? There are plainly only two ways in which the disease can possibly have entered the glands—one is by the lymph path, and the other by the blood path. Mr Annandale, from what I caught of his remarks, seemed rather to favour that which is the more general view, the entrance of the disease by means of the lymph path. This view is more generally entertained because it appears adequately to explain the frequency of tuberculosis in glandular texture, as well as the frequency of tuberculosis in the glands of the neck more especially, to which it may so easily enter by the teeth, the tonsils, and other parts. I am strongly impressed with the idea that this is erroneous, and that the entrance of the disease into the glands is frequently by the blood path. And the reasons which make me think that it is so are briefly these:—First, we have the analogy of tuberculosis of other organs, we have tubercle occurring in many other organs to which it can only arrive by the blood path—such organs as the joints, for instance, the bones, the testicle. I leave again to the pathologist the question as to how it does enter the blood, and how, having got into the blood, it reaches these particular organs, but I picture to myself hypothetically a condition of affairs in which the tubercular bacillus, having arrived at the capillary portion of the circulation, makes a diapedesis like the white corpuscles, and if it then perhaps find a particularly suitable soil, breeds there; or, if not, is destroyed by the cells of the tissue in which it finds itself. Another reason then for believing, from the analogy of other organs, in its arrival at the glands by the blood path lies in the curious fact that there is most undoubtedly a selective property in tubercle—it has affinities for particular organs. It would be detaining you too long to adduce many instances of this. Let me merely refer to one or two. For instance, it is plain that the joints afford a particularly suitable soil for the multiplication of the tubercle bacillus, and that in them alone, under many conditions, the tubercle will breed. Thus you have a condition of affairs which I may venture to call strumous or tubercular arthritic fever, a disease which is not described in the books, but which I have been able to verify, and which very closely resembles rheumatic fever, differing from it in the somewhat shorter duration of the intensely feverish condition, and being characterized by a considerable swelling, comparatively slight pain in the joints, and high fever, subsiding in a week or fortnight, but leaving behind one or two joints to run the particular course pathognomonic of strumous joints. Then another variety of strumous arthritis occurs—a variety, again, not very definitely described in the books —a variety which follows the surgeon's interference, cases in which you perform an operation here or there, generally upon joints, and the bacilli getting into the vessels you have opened descend upon a joint elsewhere. Twice my interference was followed by a very distinct attack in other joints; and in joints only arthritis is not alone. Many tissues of the body are affected to the exclusion of others. The analogy of these various effects of tubercle upon other organs would lead one, *à priori*, to suppose that it was exceedingly likely that in the glandular textures such might also occur, and this is confirmed by various facts in connexion with strumous or tuberculous adenitis. For example, there is undoubtedly a considerable number of cases in which strumous adenitis is not confined to the glands of the neck, but is found in the axilla also. It is frequently observed that tubercular or strumous glands of the neck have a symmetrical tendency, in chronology appearing about the same time. Strumous glands in the neck are very frequently unaccompanied by any discoverable breach of surface in the parts with which they are connected. Lastly, strumous glands are very rarely (and this is an interesting point) caused by local strumous disease. I have seen three cases of that rare disease, tubercular ulcer of the tongue, two of which were verified by microscopic examination. In these the glands were not affected. Strumous diseases of the joints are not prone to produce disease of the glands. All these facts, I think, point to the probability that strumous glands, even in the neck, are often affected through the blood. I do not mean to deny, on the contrary I am sure that it is true, as Mr Annandale pointed out, that local irritation plays a part—it plays an important part—in the production of strumous glands, but this part, that it forms the soil. A Russian lady doctor has made a most important examination on that particular point, showing that the scarlatina organism has this property, that it prepares, in a very remarkable way, the tonsils and glands of the neck for the action of the ordinary pyogenic organisms in a very virulent form. So it is with pyogenic organisms in connexion with tuberculosis; I am certain they are adjuvant the one to the other; and the surgeon has very frequent opportunities of verifying that fact when he sees the rapid progress of tuberculosis after the occurrence of septicity. In this way it may be found that strumous glands in the neck are actually connected with local sources of irritation, because soil is prepared for the strumous disease. I am rather, I am afraid, wandering from the true subject of discussion, and detaining you too long. Let me pass to the question of treatment, leaving it for others to consider the diagnosis to which Mr Annandale has referred. And it seems to me a by no means difficult diagnosis, except in early cases, where the difficulty of diagnosing between strumous glands and lymphadenoma is often great, and must largely depend on the history of the patient. Let me proceed to the matter of treatment. Here, I think, we ought always to comfort ourselves and increase our activity by remembering that the warfare between the bacillus and the tissue of the body is an exceedingly close one, in which very little interference on our part may determine the victory to one side or the other. With that view, you go with greater comfort and decision to the treatment of the case. You may lay down two general principles which are applicable to all inflammatory and tuberculous disease. First, that the treatment must be etiological, must consist in the removal of the cause; and, second, what has been recognised from all time, but more especially since Mr Hilton's book was published, that rest is a universal treatment in all inflammatory affections. Well, then, as to the removal of the cause, How may you remove the cause? First, you will, of course, remove the subsidiary or local cause, and, in this respect, you will bear in mind the extreme importance of removing or preventing septicity. Next in the etiological treatment of the case comes the removal of the tissues affected. And this may be done in two ways—either by excision or by scraping, as Mr Annandale pointed out. The method by excision appears to me applicable to a comparatively small number of cases—to those, perhaps, in which a single gland is affected, is easily removable, and which, without progressing rapidly or being stationary, has yet resisted other methods of treatment. It is right, I have no doubt, in these cases to remove it as a possible source of future general infection. When the glands are more numerous, the scar that you produce, the matting that generally exists, the impossibility of tracing out the many glands that you did not recognise from the outside make excision a comparatively rare resource. Scraping, however, is undoubtedly most useful, always to be employed when you open a gland, and most valuable when the gland is already open, with great certainty enabling you to remove the septicity. I do not think you remove the whole tubercle, but it has to be remembered that quantity is an element of great importance in the warfare, and that, Providence being here, as elsewhere, on the side of the biggest battalions, if you take away so many bacilli, you may place the victory on the side of the tissues. Third, in the etiological treatment you have the possibility of destroying the bacillus; and I think Mr Annandale has been rather contemptuous with reference to injection. The injection of iodine, I am sure, has given good results before suppuration. I have tried the bichloride injection. I have certainly seen glands that resisted other treatment improve or disappear; and this result would be remarkable if they were simply adenitis. I am quite sure that the iodine internally is a great deal better than externally. In adenitis of a chronic character, the result of local irritation, counter-irritation may do good; but when you have a tubercle bacillus, how can you expect by means of blistering or iodine to cure it? Blistering, iodine, and so on, may, however, do good after the glands have opened, by the painting of iodine well over the interior or pushing the blistering fluid under the skin; and I believe it does so by proving inimical to the bacillus. Passing from these various means of treating etiologically, we come to the treatment by rest, and this is, I think, a method of treatment which has been too much neglected. I have the greatest possible faith in it. It must be complete, absolute, just as you would make it for the spine, the knee-joint, or any other case of tuberculous disease. You must put the neck and the head at rest; and in order to do that I have tried many things, Mr Fleming's bags for the treatment of cervical caries, and an invention of my own. But I have come to the conclusion that the only thing that really is very nice, and does what you want, is the use of some sort of cuirass, with a good stock running up from it and catching by the jaw and the occiput so as to hold the head steady. Mr Bryant the other day had a communication on the subject in the *Lancet*, with an elaborate drawing of what we have been using for years. The helmet and cuirass form an exceedingly useful apparatus for the treatment of these glands in every stage, and in the earlier stages they will frequently cure with rapidity. If the patient is not a good soil, rest will do; even if he is, it will have a remarkably good effect. Rest, then, is a matter of great importance. Let me sum up the methods of treatment which I think ought to be recommended. First, the glands being in the early stage, you apply all those internal hygienic remedies which the physicians present will point out to you. This is most important. You apply also rest by means of a well-padded stock and cuirass. Supposing it has further advanced but not suppurated, then I should use the injection of iodine—two, three, or four drops injected into each gland, according to circumstances. I think the iodine is better than the bichloride. I tried igni-puncture; and I should recommend that, if any one does try igni-puncture, they use that which I used long ago, putting in the platinum wire through the gland, and threading upon it hollow needles, taking care not to heat the hollow needles and hurt the skin. You can then destroy large portions by electric heat without making a skin wound. I must say that igni-puncture has not been so satisfactory as I should have expected, although I have seen cases in which considerable improvement followed, whether because I at the same time put at rest or not I am not quite sure. Suppose the iodine injection fail, then opening and scraping. I should add to the scraping the injection of parasiticides. I myself use salicylic or boracic acid for the purpose of destroying the bacilli, whether pyogenic or tubercular. And, lastly, in a few and limited number of cases, excision of the glands.

(*To be continued*).

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION L.—MEETING VII.

Wednesday, 12th June 1889.—Dr UNDERHILL, *President, in the Chair.*

I. *Dr Felkin* read his paper, DOES THE INCREASE OF CIVILISATION RENDER THE OCCURRENCE OF SEPSIS MORE COMMON? which appears at page 113 of this Journal.

Prof. Simpson thought Dr Felkin hardly needed to make any apology for bringing before the Obstetrical Society a paper which bears in so many ways on the subjects that specially interest its members, in the course of which he had been able to throw light on distinctly obstetric matters, and in which he had embodied some more of the important observations he had been able to make in the course of his travels in Africa. The facts he had adduced as to the power of uncivilized races to resist the effect of injuries were quite in the line of the observations that had often been made by our army surgeons in India, who found the natives recovering from injuries and surviving operations under which British soldiers almost inevitably died, and Dr Felkin's explanations had no doubt a large element of truth. He (Prof. Simpson) was afraid that Dr Felkin was inclined to lay too much stress on the difference in the nervous irritability attendant on the greater cerebral development of civilized communities, but was more satisfied when later in the paper he pointed out the condition of the tissues generally, and of the blood in particular. It would be important to have observations made on the constitution of the blood, now that the importance of the function of the white corpuscles had been recognised in the way of meeting and making away with some of the morbific elements that find their way into the system through wounds and otherwise. In the course of his interesting paper Dr Felkin had referred to the relative facility of parturition among the natives of Africa, and had rightly adduced as a cause the good muscular development of the women and healthy condition of the pelvis. But he (Prof. Simpson) thought that perhaps an even more important element was the small size of the fœtal head, associated with the less perfect development of the brain. He thought the Society and the profession were much indebted to Dr Felkin for his valuable and interesting communication.

Dr Foulis thought it would have been easier to have given an answer to this question had Dr Felkin, in the first instance, defined the term *civilisation*, as he had done the term *sepsis*. For if we went to the large towns in so-called civilized countries, as London, Paris, Berlin, etc., in each we found countless thousands of poor, wretched, half-starved creatures, full of hereditary disease, filling our numerous charitable institutions, who were quite unable to resist the deadly effects of blood-poisoning. In each city were human beings living crowded together, in bad atmospheres, badly fed, badly clothed, in ignorance, utterly disregarding the commonest sanitary laws, some of them educated in a sense, many of them very religious, very superstitious, and with a strong belief in the supernatural. There was no sympathy in *Nature,* and what could these poor creatures do in the great struggle for existence but die like flies. Such people were not civilized. They were not civilized who neglected the development of their bodies. They were not civilized who did not follow closely the unchangeable laws of Nature, but were content to live in ignorance of those laws, trusting for their happiness to the comforts of their religion, to superstition, and to a belief in the supernatural. Only the truly scientific man was civilized, and he alone had the greatest power of resisting the onslaughts of disease. He alone had the power, too, of helping his suffering human brother. And how did he do it? Not by prayer and fasting, but by giving him a knowledge of the truth as regards the unchangeable laws of Nature. The highly civilized man was the truly scientific man, who followed, without swerving to the right or to the left, the teachings of these unchangeable laws of Nature; and who, by a knowledge of such laws, developed his mind and body to the highest degree of human excellence. And such a man, because of this development, could not be otherwise than moral, noble, and good, in the best sense. Now, if Dr Felkin would accept Dr Foulis' definition of the civilized man, the answer to the question would be as follows,—there is no comparison, and in course of time most of the septic and zymotic diseases among civilized nations will be abolished.

Dr Matheson thought the thanks of the Fellows present were due to Dr Felkin for his most interesting and instructive paper. To his mind it seemed in all probability true, from what Dr Felkin had said, that among uncivilized peoples there was a greater resisting power to septic influences. But, at the same time, it was possible another element came into play in explanation of the freedom of these races from septic diseases, namely, that the germs causing these diseases did not exist in such abundance in the surroundings as in those of civilized peoples. Dr Felkin had stated that dirt and filth were often present about wounds without giving rise to septic poisoning, unless in exceptional cases; but this was perhaps often due to pathogenic organisms being absent, though the non-pathogenic organisms of putrefaction abounded.

The President said Dr Felkin's paper was an interesting and instructive one, but not quite convincing to his mind. He did not think the evidence brought forward sufficiently exact and authentic, excepting what depended on Dr Felkin's personal experience, to found any strong opinions upon. You could not get certain conclusions from vague premisses. He had always thought, with Professor Simpson, that the ease and rapidity with which labour was passed through by the savage races depended primarily on the smaller size of head. Dr

Felkin, he thought, laid too much stress on the nervous system as a factor in puerperal disease. The fact that, since the introduction of a strenuous antiseptic treatment into the management of lying-in women, death from septic causes had almost entirely disappeared from certain large maternity hospitals in Paris, Copenhagen, and elsewhere, was to him proof positive that the question of sepsis in midwifery had to do with external conditions, and was independent of the influences of race. And if this were true of midwifery, it was likely to be equally true, though not so easily proved, in relation to wounds and injuries. It was a pity that Dr Felkin had excluded zymotic diseases from the present investigation, because experience seemed to show that, although savage and semi-civilized races might recover more quickly and certainly from severe injuries than those more civilized, the reverse was true of zymotic disease; the records, for instance, of cholera and measles showed a very high mortality among barbarous tribes.

Dr Felkin, in reply to Professor Simpson, said he was afraid that he had enlarged rather too fully upon the nervous susceptibility of different races; yet he felt that it must be admitted that Europeans and Negroes do vary in their susceptibility to suffer from the diseases of the nervous system. The mortality per thousand at Sierra Leone has been found to be 43· amongst whites as against 16· in Negroes.[1] With regard to the action of the white corpuscles, he thought that valuable information might be obtained which would probably explain many differences met with amongst various races. He agreed with Professor Simpson in thinking that the size of the fœtal head in relation to parturition was of great importance, and this point is illustrated by the greater frequency of difficult labours which occur in native women who have crossed with men of other races. As is well known, difficult labours occur where Arab and Egyptian men marry Negro women. The latter have dolicho-cephalic heads (73·40), the Arabs' heads are dolicho-cephalic (74·06), and the Egyptian heads sub-dolicho-cephalic (76·39). The pelvis in the Negro is wedge-like.[2] Certainly the proportion of the fœtal head to the pelvis in a Negro is less than it is in this country; at the same time, it must be borne in mind that the African child is exceedingly well developed when born. In answer to Dr Foulis, Dr Felkin said that in using the term civilisation he had certainly intended to include many of the points referred to by Dr Foulis. He considered that the dregs of society as seen in the Cowgate were quite as much the outcome of civilisation as were the wisest of our scientific men; and at the end of his paper he had ventured to hope that the advance of science due to civilisation would in time counteract the increased liability to septic diseases, which he considered to be also the result of civilisation. In reply to Dr Matheson, Dr Felkin was ready to admit that, although uncivilized races were possessed of a greater resisting power to septic influences than civilized peoples, yet in all probability many other factors were at work in decreasing their liability to septic disease. It is quite possible, for instance, that they may possess a heightened and quicker metabolism, thereby having the power of more quickly eliminating morbific material from their bodies; and possibly also the practice of copious but harmless libations may wash out, so to speak, the morbific material. In reply to the President, Dr Felkin explained that he was obliged to put some limit to his paper, and that, therefore, he had had to generalize to some extent on the observations of others instead of always giving definite authorities and quotations. As the President would allow, it was impossible to obtain statistics with regard to many of the races mentioned; but he believed that where statistics were obtainable, as, for instance, to some extent they were in India and America, they supported the points in his paper. In conclusion, Dr Felkin begged to thank the Fellows of the Society for the way in which they had received his paper.

---

[1] *The Human Species*, p. 424.

[2] Topinard's *Anthropology*, pp. 242, 305.

ROYAL MEDICAL SOCIETY.

May 30.—*J. R. Ratcliffe, M.B.*, showed an Eye which had been enucleated for glaucoma with cataract. The cause of the condition was a melanotic sarcoma, which grew from the back part of the choroid, and displaced the retina forwards as far as the ciliary processes.

G. L. Gulland, M.B., opened a discussion on the Ætiology of Gastric Ulcer, which was taken part in by H. H. Littlejohn, M.B., A. L. Gillespie, M.B., J. R. Ratcliffe, M.B., T. W. Eden, M.B., R. Muir, M.B., W. C. Sillar, M.B., and E. C. Carter, M.B.

R. A. Fleming, M.B., related a case of Labour he had attended, in which the patient was delirious, the temperature 101°·5 F., and the pains accompanied by maniacal excitement. The history was indistinct, but the woman had been quite conscious the previous day, although complaining of slight malaise. The breath and skin exhaled a very offensive odour. Over the chest, and subsequently over the back, petechial spots were seen. The temperature after delivery rose to 103°·6 F., and patient was removed to the Fever Hospital, where the diagnosis of typhus fever was confirmed. Several other cases of typhus were also admitted from the same close.

James Ritchie, M.B., related the case of a man who was found lying at the foot of a flight of stairs with a small scalp wound. Patient was unconscious, and the right arm was paralysed. Slight drooping of the right corner of the mouth was observed, and conjugate deviation of the eyes to the right. There were no signs nor symptoms of fracture of the skull, and no cardiac nor renal disease discoverable. Patient, in reply to questions, could only say "yes" or "no." On the fifth day after admission the temperature rose, and he died with œdema of the lungs. The post-mortem examination revealed nothing whatsoever to account for the symptoms.

June 6.—*A. L. Gillespie, M.B.*, gave a communication on Tabes Mesenterica, and narrated four cases. The first, a boy, aged 6, was admitted to the Royal Hospital for Sick Children on December 1, 1888, suffering from enlarged peritoneal glands with effusion. He weighed 36½ lbs., and had shortly before recovered from measles. There was dulness over the upper part of the left lung, but no morbid accompaniments to the breath sounds. *Treatment.*—Patient ordered cod-liver oil, and oleate of mercury ointment to be rubbed into the abdomen. By *Dec.* 19 a regular hectic temperature had set in, and the base of the right lung was dull. Ordered to take tr. cinchonæ, liq. hydrarg. perchlor. āā ♏ xv. *Jan.* 16.—Abdomen hard and resistent, and patient losing weight. *Feb.* 5.—A hard mass felt in left iliac fossa, reaching the splenic dulness above, and extending 1½″ to the right of the umbilicus. Liq. calcii chloridi substituted for the mercury, and maltine added to the cod-liver oil. *Feb.* 13.—Patient began to gain weight and improved steadily. He was shown to the meeting, and looked remarkably healthy.

The second and third cases were a girl and boy, aged respectively 12 and 4, both suffering from enlarged peritoneal glands, but without effusion or pyrexia. Great improvement took place under the exhibition of iodoform in pill and inunction of iodide of potassium ointment, with the appropriate regimen.

In the fourth case similar masses of glands were found in the abdomen, but in spite of treatment the patient was carried off by intractable diarrhœa, greatly emaciated, after seven weeks' illness.

H. H. Littlejohn, M.B., showed Four Calvaria exhibiting fractures, the result of violence or accident.

W. C. Sillar, M.B., communicated the case of a professional man who for long had been troubled by an Offensive Smell in the Left Nostril, perceptible only to himself. He was treated at first by insufflations and other local applications, but without result. On consulting a specialist in throat and nose diseases, the probe revealed the presence of an abscess in the frontal sinus. This was opened, and the discharge, at first sweet, subsequently became septic. Shortly afterwards a rigor occurred suddenly, and patient became rapidly comatose. From the history of the case, the physicians in attendance diagnosed an abscess in the left frontal lobe, although there were no symptoms to help in localization of the lesion. It was decided to trephine. On operation, thirty-six hours after the onset of the rigor, an abscess was found in the tip of the left frontal lobe and evacuated, but without affording relief to the patient, who sank twenty-four hours later.

R. Muir, M.B., gave a communication on Blood Plaques, describing their nature and appearance in health, and the methods necessary to be employed for their microscopic study.

June 20.—*H. H. Littlejohn, M.B.*, showed a card specimen of Rupture of the Aorta into the Pericardium causing great distension. He also related the case of a man, aged 64, who had died suddenly while raising himself in bed. A week previously he had complained of pain in the region of the heart, and had been ordered to keep perfectly quiet in bed. On post-mortem examination the pericardium was found to contain blood which had proceeded from a rupture in the wall of the left ventricle on its anterior surface.

R. A. Fleming, M.B., communicated the case of a tablemaid, aged 22, admitted to Ward XXV., Edinburgh Royal Infirmary, on 21st May 1889. Since the beginning of the year patient had been anæmic and out of health from overwork, and latterly had suffered from headaches and lowness of spirits. On 12th May, after a long walk, she became violently sick, fainted, and lapsed into a semiconscious condition, occasionally passing her urine in bed. On admission there were no distinct morbid appearances. Patient was very obstinate about answering questions, but on being asked if she felt pain pointed to her forehead and behind her left ear. All the systems appeared normal save the nervous. On P.V. examination, however, there was slight tenderness in the left lateral fornix. *Nervous System.*—All the sensory functions were normal, as were all reflexes, except for the loss of control over micturition, and occasionally over defæcation. The ophthalmoscope revealed nothing abnormal. *May* 25.—Slight paresis of the left arm and leg were noticed. This passed away completely by 5th June. Though apparently understanding what went on around her, patient could only say "pain," "head," "yes," or "no." *May* 30.—Patient had a peculiar fit which lasted five minutes. Both upper and lower limbs, especially the left arm, exhibited large tremors; the face was turned upwards to the right. There was no screaming. *June* 2.—Passed a very troubled night, rolling her head from side to side and screaming. *June* 3.—Commenced to take fits with great frequency. Clonic spasm began in the right thumb, accompanied by very fine tremor in the upper lip. The spasm then affected in order the fingers, wrist, elbow, and at the same time would appear retraction of the right corner of the mouth and elevation of the upper lip and right ala nasi. These ill-developed fits continued with great frequency, and the temperature rose to 101° F. Pulse, weak, 130 per minute. *June* 5.—At 2 A.M. trephining was performed over the left ear with no relief as regards the fits, of which patient had more than 150 after operation. On or about 7th June the wound became septic, and patient died on 8th June, the temperature having risen steadily, till after death it reached 106°·4 F. *Post-mortem Examination.*—Besides evidences of operation were found—Ante-mortem clot in superior longitudinal sinus; numerous minute hæmorrhages in cortex of convolutions near left fissure of Rolando; clots in the superficial veins of the same region, and most markedly in one running along the parallel fissure of the temporo-sphenoidal lobe. The left hemisphere was considerably larger than the right, and in its frontal and parietal convolutions at their upper and middle part was a growth the size of a walnut, which proved subsequently to be an angioma. A small parovarian cyst was found on the left side.

Surgeon W. E. Home, R.N., communicated a case in which an attack of dyspnœa and dysphagia had followed immediately after a fall from a height, the patient striking the back of his neck. Several subsequent attacks were experienced at irregular intervals, and ultimately the condition appeared to merge into ordinary asthma, from which patient had never suffered previous to the accident.

J. Ritchie, M.B., communicated the case of a boy, aged 12, who was admitted to the Royal Infirmary, Edinburgh, on 19th May, suffering from symptoms of Tetanus. Seventeen days previously he had by falling on the pavement, received a severe cut on the top of the head to the left of the middle line. Three days later the cut had entirely healed, except at one spot, where a small abscess had formed. At this time slight stiffness of the jaw and paresis of the left side of the face were observed. These symptoms continued till two days previous to admission, when spasms of the neck and trunk were added. On admission patient's jaws were found firmly approximated, his head bent back, and on the slightest movement arching of the back, rigidity of the abdominal parietes, and contortion of the right side of the face took place. There was complete paralysis of the lower part of the left side of the face and paresis of the upper. There was no paralysis of the limbs. Sensation was perfect. Patient was free from pain, except on attempting to swallow, when severe pain was felt in the epigastrium. Mental functions normal. Patient was kept quiet, fed by the rectum, and medicinally had potassium bromide and chloral. On the evening of the 19th the cicatrix was excised, after which only three spasms occurred. On the 20th the mouth could be opened. In the evening the pulse and respirations increased, and patient got weaker. On the 22nd the temperature, which had been subnormal, rose to 102°·2 F., and patient died on the 23rd. Before death the temperature was 103°·6 F., the pulse 140, and the respirations 96. Post-mortem examination revealed two patches of purulent exudation over each supramarginal convolution, slight congestion of the gray matter of the cortex, great congestion of the medulla and pons, and, to a less extent, congestion of the gray matter of the spinal cord.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

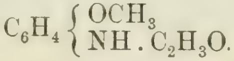
By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

URAL, A NEW HYPNOTIC.—Dr Gustavo Poppi describes a product under the name of ural, formed by the combination of chloral hydrate with urethan, and recommends it as an excellent hypnotic, which in ordinary doses produces a physiologically quiet sleep more rapidly and of longer duration than any of the other known hypnotics. He claims that its use produces no disagreeable accidents, and in therapeutic doses no disturbance of blood-pressure; if, however, poisonous doses are given to dogs under these circumstances, reduction of blood-pressure is the principal mode through which its toxic action is expressed. No discomfort or distress is produced on awakening from the sleep produced by ural; and Dr Poppi states that its use may be persisted in for several days without inconvenience. He claims that he has given it in different cases of heart disease, hysteria, and other nervous affections, in all cases with the best results, even when other hypnotics had proved ineffective (*Wiener Medizinische Wochenschrift*, No. 22, 1889).—*Therapeutic Gazette*, May 1889.

EXALGINE, A NEW ANALGESIC.—Under the name of exalgine, or orthomethylacetanilide, is described a new analgesic, a derivative of the aromatic series, which was recently discovered by Brigonnet, and which formed the subject of an interesting communication to the French Academy of Sciences by Drs Dujardin-Beaumetz and Bardet. Exalgine (*ex*, privative, and *algos*, pain) has the formula of $C_9H_{11}NO$, and is one of the three isomeric methyl derivatives of acetanilide (*Compt.-Rend.*, cviii. 571). It occurs in needles or in long tablet-like crystals, according as it is obtained by crystallization or from the mass after fusion. It is sparingly soluble in cold water, quite soluble in hot water, and readily soluble in very dilute alcohol or in water to which a small amount of alcohol has been added; it fuses at 101° C. As regards its physiological action, it seems closely allied to antipyrin, although its effects upon the sensorium are more marked than the latter, while it exerts a less marked action as an antipyretic. The characteristic results are said to be obtained after an administration of a dose of from 4 to 6 grains given at once, or from 6 to 12 grains taken in two doses in the course of twenty-four hours, the analgesic effect being claimed to be very marked in all forms of neuralgia, including visceral neuralgia, even being superior to that of antipyrin (*Pharmaceutical*

Journal and Transactions, March 30, 1889). No gastro-intestinal irritation, rash, or cyanosis has been noticed after its administration, and it therefore would appear to be free from the disagreeable after-effects which appear in certain cases after the use of antipyrin and acetanilide. The toxic dose is stated to be 7½ grains for every two pounds of body weight, death apparently resulting from paralysis of the respiratory centre. It is noted that after the physiological effect of the drug has been developed, while painful sensations are destroyed, the tactile sense remains unaltered. Exalgine is eliminated by the urine, upon the quantity of which it exercises a marked effect, acting like the antipyretics of the same group, diminishing the quantity of the secretion (*National Druggist,* April 15, 1889). In diabetes it also reduces the quantity of urine and of sugar eliminated. Like all the derivatives of the aromatic series, it is analgesic, though the latter property is the more marked, and may be developed in doses of less than half as great as antipyrin.—*Therapeutic Gazette,* May 1889.

METHACETIN, A NEW ANTIPYRETIC.—Dr Franz Makuert, assistant to Professor von Jaksch, describes in the *Pharmaceutische Post* for 7th April 1889 the most recent addition to our rapidly-growing list of synthetic medicinal compounds. This time it is an antipyretic, and in composition might be described under the name of para-acetanisidin, its formula being—



$$C_6H_4\begin{cases}OCH_3\\NH\,.\,C_2H_3O.\end{cases}$$

This substance is a combination of the radical acetyl with anisidine, and is therefore an acetyl combination of the methyl ether of para-amidophenol. From the fact that para-acetphenetidin is commonly described under the name of phenacetin, the precedent is formed of describing this compound under the name of methacetin. This substance occurs in the form of a powder, formed of slightly reddish scales, without odour; slightly bitter to the taste; soluble in warm water, to a less degree in cold water; while it is very soluble in cold and warm alcohol. It is not more soluble in acid and alkaline liquids than when neutral; its points of fusion is 127° C. From experiments made on animals, it was seen that methacetin, as might be expected from its analogy in combination to phenacetin, possesses the power of reducing temperature, three, four, or five degrees being lost within a few hours after its administration. Doses of 45 grains given to rabbits are markedly poisonous, producing convulsions in the posterior limbs, then in the anterior members, as in the case of poisoning with antipyrin. Its action is principally exerted on the central nervous system, death being produced in rabbits by administration of only 45 grains. In doses of from 2 to 3 grains, given to children, it exerts a marked antithermic action, the reduction of the temperature being gradually produced, remaining for several hours at the lowest point, and then gradually increasing. Frequently marked perspiration is produced by its use within an hour after its administration. If the administration of the drug corresponds to the day of remission of the fever, the thermal fall is produced more rapidly, and has a longer duration than under other circumstances. The author has employed it in three cases of pulmonary tuberculosis, one of tubercular meningitis, and one of pneumonia, the patients in all the cases being between 6 and 13 years of age. In one instance it produced slight collapse, but never vomiting, ringing in the ears, vertigo, or erythema. The urine during the administration of this drug reduces the potasso-cupric test for sugar, but does not itself contain sugar, and there is no hæmoglobinuria. Methacetin in one per cent. solution prevents coagulation in milk and ferment. It seems worthy of further study. —*Therapeutic Gazette,* May 1889.

Camphorated Naphthol in the Treatment of Tubercular Ulcerations.—At the meeting of the Société de Thérapeutique, held 27th February 1889, Dr Fernet read a short communication as to the properties of camphorated naphthol, and indications for its use in the treatment of tubercular ulcerations (*Le Progrès Médical,* 9th March 1889). This substance is formed of a mixture of one part naphthol and two parts of camphor. It forms a greasy liquid, readily applied to skin ulcerations and accessible mucous surfaces. Dr Fernet recommends its use in the treatment of boils, ulcerations of the skin of the mouth, and in diphtheria. He reported numerous cases of buccal tuberculosis which, though not cured, were greatly ameliorated by this local treatment. In the discussion which this statement provoked, several other members of the Society confirmed the statements of Dr Fernet as to the use of camphorated naphthol in such cases.—*Therapeutic Gazette,* May 1889.

MEDICAL PERISCOPE.

By Francis Troup, M.D.

Berlin. Klin. Wochensch., No. 24, 1889.—The opening paper of this number is one entitled "Observations and Remarks on the Repetition of Diseased Processes, and specially Relapses," by Prof. H. Senator. The little that is to be found in medical literature about renewals and relapses of disease is not free from obscurities and contradictions, because the several forms have not been sufficiently contradistinguished. There are recrudescences, consecutive or after fevers, relapses, and genuine second or more attacks. The recrudescence appears before the original sickness has reached its normal end, and therefore not after, but during, the illness, and really represents only a stadium, an exaltation or repetition of isolated morbid phenomena (for example, in the acute exanthemata the rash is renewed) which have not at once reached their full development, but unfold themselves in a fragmentary manner, and with short pauses, as it were, probably because the morbid poison has entered the circulation, not in one dose but in several.

The "Nachfieber," on the other hand, appears *after* the normal course of the original disease has been reached. It is a transitory feverish attack, which does not present the characteristic phenomena of the original illness; more or less of fever and feverish disturbances, but without specific symptoms. Such are not uncommon in typhoid and scarlet fever. It is very likely that such consecutive fevers owe their origin to the operation of certain (in themselves) unimportant noxæ, faulty diet, constipation, psychical excitements, slight bodily exertions—in short, trifling noxious agents which would not disturb the equipoise of a sound man, but in a reconvalescent are capable of causing such feverish disturbances. Possibly also the thermal centres have not recovered their normal condition. This nachfieber is comparable to the ephemera or febricula of older authors, namely, feverish condition without exact localization, which attack young or older persons with small power of resistance.

Relapse or recidive and a second attack have not even yet been sufficiently contradistinguished. We commonly say that every renewal of the disease is owing to the renewed operation of the noxious agent which caused it at first, and in the case of infectious disease we think that a new infection has taken place from without. Even Griesinger, who belongs to our time, and was at the apex of science, considered that relapses of intermittent fever or abdominal typhus arose only through new infection from without. This view is now quite given up, save with perhaps one exception. We now generally say that if a sickness appears in a shorter or longer period after a sickness of the same nature has been passed through, and if it can be demonstrated that this second attack is the result of the penetration of the morbid germs from without, we speak of a repetition of the disease, not of a relapse.

Gerhardt has given us a clear view and a rule by which to go in the acute infectious diseases at least. True relapses, he says, appear within a time which, reckoned from the first outbreak, does not much exceed the incubation period plus the duration of the illness; they behave thus as if the infection from which they have sprung had been acquired during the course of the first illness: as if this had given no immunity, and as if the morbid germs again ran through their developmental phases in the same body. Now, in measles the incubation period is pretty constant—ten days—that is, till the outbreak of the initial phenomena. These usually last three days, and in the other four the rash is out and over, and desquamation beginning; so we have seventeen days for incubation and duration of illness. In Gerhardt's view we could only speak of a measle relapse if the disease recurred about seventeen days from the beginning of the first attack. If one sticks to this it is astonishing how narrowly limited is the occurrence of relapse, and how cases which are thoughtlessly so called are not so in reality, but actually new attacks, standing in no other relation to the first illness.

But it is questionable if even relapses, so limited, are caused by new infection from without, and whether this is their only mode of origin. Most infectious diseases, and specially the acute exanthemata, afford a certain amount of protection from a second attack, and so much the surer the shorter the time after the first illness. One may therefore reasonably doubt whether during the progress of such a disease a new infection in Gerhardt's sense could take place. Surely relapses would be much more frequent than they actually are. Every sick man could reinfect himself from his own atmosphere, which certainly infects others.

Just as in the infected organism at a very early period, and before the disease has run its course, the poison can be efficacious as regards infection, so it can be efficient as regards its conferring of immunity. It is not impossible, but happens seldom, that during the course of an infectious disease a new infection may take place from without, but essentially this is not different from a repetition of the disease, it may be after years; but, at the same time, in common speech we call the reinfection happening after a short time a relapse, and later on a renewal of the disease.

But there is another mode of origin of relapses: it is this,—the infective germ is not incorporated anew, but a part of the original poison remains latent in the body, and only after the original sickness has run its course does it become efficacious for harm. This idea has lately received substantial confirmation from the fact that Alb. Fränkel found, in an abdominal abscess of a man who had gone through typhoid with relapses, five months after the first outbreak of the disease, typhoid bacilli living and capable of development. Ebermaier has made a similar observation in a tibial periosteal abscess of a typhoid convalescent who had been free of fever for several weeks. Valentini found the same in two cases—one of periosteal abscess and one of empyema. May not the inflammatory foci which frequently "complicate" typhoid not be of the same nature? On the ground of those observations, it is not difficult to suppose that the bacilli may again find their way into the circulation and anew excite typhoid symptoms, even abdominal ones, were they to arrive into the lymph apparatus of the bowel.

Among other infectious diseases analogous observations and researches have been made in pneumonia, and many of its mysterious complications and accompaniments become comprehensible on the supposition that its pathogenic parasites remain garnered up in some quarter of the body long after the disease has run its typical course. How often, for instance, is a pneumonia ushered in in children by cerebral symptoms? A similar origin of relapses of syphilis may arise in a similar way, and is also thinkable for many other diseases.

Relapses may also occur from a general constitutional disposition, some casual circumstances calling forth the diseased process in a very ready manner, as in gout and several functional neuroses. There may be also a merely *local* disposition to disease, the "locus minoris resistantiæ" of the older authors. Its existence gives easy occasion for repetitions of the same disease—to local relapses, in fact—and this local recidive may be the starting-point of a distant affection or a generalized illness.

Ibidem.—Professor Bernhardt speaks favourably of the results of suspension in the treatment of tabes dorsalis. Although it does not cure, yet many troublesome and painful symptoms are bettered, at least temporarily, by it.

Ibidem, No. 25.—Dr Konrad Alt of Halle gives his researches on the elimination by the stomach of morphia injected subcutaneously. Hitzig had noticed that a dog which had licked up the vomit of another which had been narcotized by morphia injection also vomited a short time thereafter. This led Hitzig to believe that, notwithstanding the very short time between the injection and the vomiting, a considerable portion of the injected morphia had been eliminated. Men do not all react in the same way to morphia—some vomit, others do not; and authors explain this by saying that a part of the morphia is converted, in the circulating blood, into apomorphia, or that the preparation used is impure. None of them seem to think that the vomiting may be a reflex act setting out from the gastric nerves.

Dr Alt made many experiments, and found that in dogs morphia injection caused, in a few minutes, vomiting, and that a considerable portion was eliminated *as morphia* by the stomach. Before the injection the stomach was carefully washed out, and at short periods after it samples were taken and tested. The conclusions at which he has arrived are the following:—

1. After subcutaneous application of morphia, it is eliminated by the stomach.
2. This elimination begins in two and a half minutes, lasts for half an hour, and ceases in one hour.
3. The inclination to vomit begins when the morphia appears in the stomach, and may be prevented by washing it out.
4. The quantity thus eliminated amounts to about half what is injected.
5. Poisonous results are much lessened by washing out the stomach, and deadly doses were thus borne without harm.

These propositions are true of dogs, and to a great degree also in man.

The practical importance of these researches lies in the fact that washing the stomach will be found of the greatest use in opium poisoning, and that the medical jurist should in future search for the poison in the stomach instead of the blood, even when it has been given subcutaneously.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. Allan Jamieson, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

Extracts from the Reports of the Weekly Clinical Conferences of the Physicians of the St Louis Hospital, Paris. —It has been the custom for a long time for the physicians at St Louis to exhibit to one another important cases of cutaneous or syphilitic affections. Of such many curious, rare, difficult, or doubtful, flock to this special establishment. In order to give these clinical conversations the necessary development, and with the object of benefiting the pupils at the Hospital as well as the physicians who frequent it, it has been decided that the exhibitions shall take place every Thursday, at half past nine, during the session. The reports will appear in the *Annales de dermatologie et de syphiligraphie* monthly, and as a special volume published separately at the end of the year. Selections of the more interesting cases will also appear in this Periscope.

1. *Maculæ Ceruleæ, or Taches Bleues.*—M. Besnier showed a man, aged 26, about to be married, who came complaining of itching on the body of a moderate degree of intensity. He was very hairy, and on examination presented a generalized eruption of spots or macules of a bluish colour. He seemed to have a macular roseola. The hairs, particularly of the genital region, were covered with nits, though the eyelashes were free from these. He was, in fact, affected with the pediculus pubis in great abundance, and exhibited the characteristic eruption of blue spots which these parasites provoke. This was specially well marked on the abdomen, the front of the thorax, and back of the arms and axillæ. The spots seen by oblique light were depressed in the centre. The researches of Mourson and Duguet have proved the relation which exists between these spots and the pediculus pubis. Notwithstanding the number of the insects, there was an almost complete absence of itching. This is not always the case, yet there is no proportional relation between the pruritus and the number of spots. The parasites may have been there for a considerable time before the itching commences. The parasite is rare at St Louis, since, in Besnier's opinion, it lives by preference on persons of a rank rather above that of those who come there.

2. *Acute Nodular Disseminated Lupus Tuberculosus.*—M. Besnier showed a girl, aged 4 years, who looked well and was well nourished, born of parents free from syphilis, but not so certainly free from tuberculosis. She exhibited, scattered all over the body, neoplasms which infiltrated the derma in the form of nodules which rose but little above the surface. These were softish, of a colour difficult to represent in words, but presenting a yellowish red, somewhat coppery aspect, with a special gummy translucency, which he

regarded as characteristic. The eruption had existed for a year, and the first spots were seen two months after an attack of measles, as minute nodules the size of a pinhead, on the face and other parts of the body. There are about forty in all, arranged without regard to symmetry; the largest are on the face. Although the diagnosis of lupus was not regarded as doubtful, a rigid antisyphilitic treatment was carried out without effect. Sections of a portion removed showed a great deposit of embryonic cells in the upper layers of the disease, but neither giant cells nor bacilli. Yet inoculations into a healthy guinea-pig caused an abscess at the point of insertion and enlarged glands, in the pus from one of which bacilli were found in small number.—*Annales de dermatologie et de syphiligraphie*, January 1889.

Destructive Folliculitis of Hairy Regions.—Quinquand has made an interesting communication to the Society of the Hospitals of Paris on a special form of folliculitis which is followed by an incurable alopecia, which simulates alopecia areata. The usual site of the affection is the scalp, but it may affect the beard, the pubis, and the axillary regions. The bald patches are irregular, nearly smooth, polished; the skin is decolorized, white, as though atrophied, but may present in some points a slight redness. The patch is depressed, and has a pseudo-cicatricial appearance. The morbid process is essentially constituted by follicular lesions of various aspects, which exist at the periphery of the plaques of alopecia, themselves consecutive to the evolution of these lesions. The lesions consist in purulent points like miliary abscesses of the size of the head of a pin or still smaller. They are punctiform, and from the centre emerges a hair, which soon falls spontaneously; or else there are simply little crusts resting on a red base, which is slightly moist; or there may be a red follicular elevation. There are neither tubercles, nor favus cups, nor seborrhœal alterations. The evolution of the disease is altogether peculiar. An alopecia is first noticed, but in studying it, it is seen that it begins in isolated points. Situated round the hair-follicles a red elevation is noted, and often, though not always, the folliculitis is suppurative. Crops of small pustules are produced at the base of the hairs, but the number of these purulent points is relatively small. There is thus an acute epilating folliculitis, proceeding by successive crops, and lasting for a very long time. A special micrococcus has been found by Quinquand in the hair-follicle and blood of the inflamed region. This can be cultivated, and if frictions are made with the culture fluid over hairy parts, it will produce lesions of the follicles, and loss of hair in the rat, the rabbit, and man. The treatment consists —1. In cleansing with care the hairy part with soap and water. 2. In applying to the diseased region and its neighbourhood tincture of iodine every ten days. 3. In applying the following lotion each morning:—℞. Hyd. biniodidi, gr. j.; hyd. perchloridi, gr. xv.; sp. vini rect. at 90°, ℥ij.; aquam, ℥xj. After about a month of this treatment the folliculitis ceases, but the baldness persists. Brocq, in commenting on this, includes at least two other morbid conditions. One is the form of sycosis called lupoid, which affects the beard especially. The inflammatory lesions are characterized by large follicular and perifollicular pustules, by redness and thickening of the skin, by the production of crusts and scales, and confluence of the adjacent lesions. It differs from a sycosis of the non-parasitic variety by the constant tendency to extend in a regular centrifugal manner, while all the hairs of the affected part are radically destroyed and a central cicatrix ensues. There is an altogether characteristic appearance of a central absolutely bare cicatrix, surrounded by a zone of activity of a red, inflamed, sycosiform aspect. It is very rebellious to treatment, but when an energetic course is pursued, it finally is arrested in its extensive march, and recovery takes place. Another variety simulates alopecia areata, and has probably been confounded with it. The inflammatory process is slight, there is little tumefaction, and the scalp has only a light rosy tint surrounding the affected hair. The hair soon falls spontaneously, and the inflammatory action is calmed, but it has produced a complete destruction of the hair papilla, and only a white, smooth, ivory-like portion of scalp is seen, which seems to be atrophied, without a vestige of hair or lanugo.—*Journal of Cutaneous and Genito-Urinary Diseases*, January 1889.

Ulerythema Ophryogenes.—Under this name (meaning occurring erythema of the eyebrows) Taenzer relates the histories of six cases which have been seen at Unna's clinique in Hamburg. The affection begins in early childhood with redness of the skin of the eyebrows (a situation which remains affected throughout life), and spreads at a later time to some neighbouring parts, in particular the face, the scalp, more rarely the upper arm. At first resembling a so-called lichen pilaris on a reddened base, the disease becomes in some localities—in bad cases in all—a severe though very superficial inflammation of the skin. On the part of the hair follicles there are symptoms of a non-purulent folliculitis; the hairs stand here and there in clusters of two or three, the space between being the seat of an atrophic process. Only in one case did he see the termination of this slow inflammatory process, which consisted in total alopecia and atrophy of the scalp as extreme as results from extensive favus. There is a milder and a more intense form. In the former it stretches from the outer portion of the eyebrows over the zygomatic region in front of the ear down the neck and on to the upper arm. It invariably begins as a hyperkeratosis with associated erythema; the follicles become stopped by a covering of horny substance; the lanugo hair is always, the stronger hair sometimes, prevented from emerging, so that the appearance of a peculiar hyperkeratosis pilaris on an erythematous base arises. In the more severe cases, besides the regions mentioned above, the inner part of the eyebrows, the upper lip, the scalp, and the extensor aspect of the upper limb in a greater or less degree are attacked. In such the gravity of the complaint is shown by the subsequent scarring, which is only visible in the milder cases on isolated spots. The disease shows in general a very indolent character, and only occasionally is there suppurative folliculitis. This latter occurs especially when stimulating treatment of the scalp has been adopted, while feebly reducing remedies—as sulphur and resorcin—ameliorate the severe and cure the milder forms. The complaint is related to lupus erythematosus and to that described above by Quinquand and Brocq.—*Monatshefte für praktische Dermatologie*, No. 5, 1889.

TREATMENT OF SCALY ECZEMA OF THE BACK OF THE HAND.—A physician engaged in obstetric practice has consulted Unna as to a scaly eczema limited to the back of the hands, which has interfered much with his practice, and has lasted a long time. The skin is reddened and infiltrated, there are no pustules, but numerous fissures which heal with difficulty. The hands often seem as if frost-bitten, yet he scarcely can believe that the complaint is induced by frost, since it was equally bad in summer. Unna regards the case as one of eczema seborrhœicum. He thinks also that it should be classed in this instance as a trade or professional disease, and that it is at least aggravated by contact of the back of the hand with antiseptic substances used in midwifery practice. Yet such eczemas only occur, in his opinion, in persons suffering from seborrhœal affections of other parts, as alopecia pityrodes, pityriasis barbae vel capitis, seborrhœa oleosa of the face, or sicca of the trunk or axilla, eczema intertrigo, etc. Not only in treatment but in future prophylaxis is this to be borne in mind. In the treatment he recommends that the backs of the hands should each night be covered with a thin layer of absorbent wool well soaked in the following lotion, to which at the time as much water is added:—℞ Resorcini, glycerini, āā 10·0; sp. tenuior, 180·0. The wool is to be covered with guttapercha tissue, so that in the morning the hand is still moist. On the moist surface such a paste as the following is to be spread in the morning, and repeated once or more times during the day:—℞ Ung. zinci, 25·0; kaolini, 2·5; resorcini, 0·5. At night the wet compress should be renewed. This has the advantage of healing the cracks which unavoidably form during the day. The over-fatty basic soap and warm water are exclusively to be employed to wash with. All seborrhœal ailments, even simple scurfiness of the head, must be simultaneously completely cured, else the eczema of the hands will continually recur.—*Monatshefte für praktische Dermatologie*, No. 4, 1889.

TREATMENT OF EPITHELIOMA BY CHLORATE OF POTASS.—Lemoine concludes an interesting article as follows:—"We possess in the chlorate of potass a substance of much efficacy in the treatment of epithelioma, and which can quite cure it. Although in the majority of cases the knife is preferable, yet it is the sole remedy from which in cases unsuited for operation we can expect results. It may not be suitable for epithelioma of the mucous membranes, since in such cases the disease penetrates deeply, yet it gives better results in mixed forms, such as attack the skin and mucous membrane at the same time. Its employment is most successful in epithelioma of the skin. The treatment by its means is relatively slow, yet it uniformly checks progress, should the growth tend to advance rapidly, if there seems danger of a general infection."—*Revue générale de clinique et de Thérapeutique*, No. 27, 1888.

SYCOSIS.—Jackson of New York, after relating and critically considering fourteen cases, formulates the treatment of this obstinate complaint as follows:—In acute cases, where there is much pustulation, epilate or curette, and apply boric acid ointment or Lassar's paste with salicylic acid. Give one-tenth of a grain of calcium sulphide in fresh tablet triturates every one or two hours. If an acute outbreak of pustules occurs under it, stop it until a subsidence of the eruption takes place, and then begin again. In sub-acute cases, where there is not so much pustulation but more redness, and the disease is more patchy, epilate or curette, and use Bronson's ointment (℞ Hyd. ammon., gr. xx.; hyd. subchloridi, gr. xl.; vaselini, ℥j. M.), or one of sulphur, or tar, or other mild stimulant, or use soap frictions followed by protective ointments. In chronic cases epilate or curette, or apply a solution of caustic potash carefully to diseased parts. Locally employ strong ointments or solutions of tar, provided caustic potash has not been used. If it has been, then apply a simple soothing dressing. The use of tar in alcohol, as proposed by Pick of Prague, has given brilliant results in his hands in some cases of chronic eczema, and has greatly benefited one of the most obstinate of Jackson's cases of sycosis. Should any case take on the acute form, soothing methods are to be adopted. The patient as well as his skin must be suitably treated. —*Journal of Cutaneous and Genito-Urinary Diseases*, January 1889.

ON THE PARASITIC ORIGIN OF PSORIASIS.—Professor Lang in a series of articles has endeavoured to prove that psoriasis is due to a parasite, which he described and figured under the name of epidermidophyton, and Eklunnd in like manner thought it was occasioned by another parasite, named by him lepocolla repens. Ries has set himself to examine into the question, and as the result of very carefully executed observations has come to the conclusion that neither of these organisms is the specific organism of psoriasis. And more, that both are artificial products of the modes of preparation of the scales, and are not fungi at all. This does not, perhaps, entirely disprove the parasitic theory of psoriasis, but it shows that so far at least there is no scientific proof of such hypothesis. The arguments will be found in full in the *Vierteljahresschrift für Dermatologie und Syphilis*, Heft 6, 1888.

OCCASIONAL PERISCOPE OF TUBERCULOSIS.

By R. W. PHILIP, M.A., M.D., F.R.C.P., Physician to the Victoria Dispensary for Consumption.

CONGENITAL TUBERCULOSIS.—Cases of undoubted transmission of tuberculosis from mother to fœtus by way of the placenta are still sufficiently rare to make all trustworthy records valuable. MM. Malvoz and L. Brouwier give a detailed account of two cases, presumably of this nature, observed at Liege. The first was an eight months' fœtus, found in the uterus of a cow affected with general tuberculosis. In the liver, and particularly in the hepatic and bronchial glands of the fœtus, there were unmistakable indications of tuberculosis. The bacillus tuberculosis was abundantly present. (The maternal genital apparatus appeared healthy.) In the second instance, where the evidence of tubercular disease was similarly established, an element of doubt was included, inasmuch as the observations were made on a six weeks' old calf. The authors claim that the similarity of the seat of the lesions and the state of advancement of the process in the latter case, and the absence of pulmonary and intestinal tuberculosis, justify their contention that the affection in this instance, as in the former, was strictly congenital.—*Annales de l'Institut. Pasteur,* tom. iii., No. 4, April 25, 1889.

INDIGESTION IN PHTHISIS.—With a view to explain the meaning of the frequent occurrence of loss of appetite and other symptoms of indigestion in the course of phthisis, Schetty investigated twenty-five cases of that disease in different stages. In twenty-three of these the presence of the tubercle bacillus had been ascertained. By a careful examination (after the methods of Kahn, Leube, and V. Mering) of the contents of the stomach at different periods of the day, and at different times in relation to food, he discovered no marked diminution of gastric secretion nor delay in gastric digestion, and he concludes that such symptoms as occurring in connexion with phthisis are certainly not always attributable to dyspepsia produced by gastric catarrh and diminished secretion of juice. Each case should be carefully analysed *per se;* in many, it will be found advisable to push alimentation even against the will of the patient. It is to be noted, however, that, in some of the cases observed by Schetty, there were no marked gastric symptoms, and the appetite was good; that, in most cases, the symptoms were limited to a dislike to food; and that in no instance was there evidence of the presence of waxy degeneration of the gastro-intestinal tract.—*Deutsches Archiv für Klinische Medicin,* Vol. xliv., Nos. 2 and 3, 14th March 1889.

TREATMENT OF PULMONARY PHTHISIS BY CREASOTE.—In publishing the results of his extended observations on the value of creasote in pulmonary phthisis, Dr Beverley Robinson dwells with emphasis on the necessity of obtaining a perfectly pure drug. He states, " that much of the creasote which is dispensed in New York city, at least, is simply crude carbolic acid." This latter is not possessed of the virtues of creasote, and is distinctly injurious. The best creasote is that distilled from beechwood tar. Dr Robinson corroborates many of the statements made by Jaccoud and later observers. He has found creasote superior to any other medicinal agent in the treatment of phthisis. His observations, extending over some 160 patients in hospital and private, go to show that, in addition to relieving the more conspicuous symptoms, creasote undoubtedly increases appetite, promotes nutrition, and arrests night-sweats. There are no counter-disadvantages of importance. " It does *not* occasion hæmoptysis, and rarely causes disturbance of the stomach or bowels, except in cases in which it is given in *too large* doses." The formula employed by Dr Robinson is the same as that of Professor Jaccoud, except that brandy, recommended by the latter, has been replaced by whisky:—

℞ Creasoti (beechwood), . . ♏vj.
Glycerini, ... ℥j.
Spts. frumenti, ... ℥ij. M.
Sig.—As directed.

The dose varies with different cases, but Dr Robinson usually prescribes half a minim of creasote every two or three hours, and this he has continued frequently for many months without increase or interruption, or any evidence of intolerance.—*International Journal of the Medical Sciences,* January 1889.

The Pathology of Joint and Bone Tuberculosis.—The etiology of tubercular synovitis and of para-articular abscess is discussed in the light of recent experimental work on the tubercle bacillus. Attention is drawn to the form of synovitis which has been observed in the joints of those who have died of general miliary tuberculosis. Dr Van Hook recognises four varieties of bone tuberculosis: 1. Miliary tuberculosis of bone, occurring in cases of general tuberculosis; 2. Tubercular osteo-myelitis; 3. Local intra-osseous tuberculosis, with wedge-formed sequestrum; 4. Local intra-osseous tuberculosis, in the form of a tubercular granulation mass.—*Chicago Medical Journal and Examiner*, December 1888.

Tubercular Peritonitis successfully treated by Abdominal Section and Washing of the Peritoneal Cavity.—Mr O'Callaghan has communicated to the Royal Academy of Medicine in Ireland the record of three cases of tubercular peritonitis in his practice successfully treated by abdominal section and subsequent flushing with blood-warm water. In one of these cases, the diagnosis had been comparatively easy, and was confirmed, on opening the abdomen, by the discovery of diffusely scattered tuberculosis. The peritoneum and intestines were covered with little nodules like millet seeds, and the mesentery was like the roe of a fish. The abdominal cavity was continuously flushed out with blood-warm water until the water flowed away clear. During the process, a kind of massage was practised on the abdomen. Mr O'Callaghan expressed the opinion, that tubercular peritonitis was frequently a localized affection similar to articular tuberculosis, and if dealt with sufficiently early in this manner, would be perfectly cured.—*Transactions of Royal Academy of Medicine in Ireland,* 22nd February 1889.

Consumption on Shipboard.—Under the title "Consumptive Travellers," a suggestive letter has appeared in the *Australasian Medical Gazette* (November 1888), from the pen of a Melbourne barrister, drawing attention to the possibility of healthy persons contracting phthisis from consumptive fellow-passengers. Attention is drawn to the necessarily very limited number of cubic feet, even in the best of cabins, and to the liability which exists of obtaining for one's mate a person of phthisical taint. The evil has undoubtedly increased by reason of the comparative frequency of the sea voyage as a part of treatment. Though the letter is professedly addressed by a layman to laymen, extracts are given from the works of Koch and others in proof of the contagiousness of the disease, especially by way of the sputum; and the proposal is advanced, that all intending passengers should, on booking, be made to produce to the shipping agents a medical certificate of health, and that medical officers should have the power to remove a passenger into the ship's hospital, which should be properly fitted for their reception.

SURGICAL PERISCOPE.

By A. G. Miller, F.R.C.S.E.

Ossifluent Abscess.—This term is applied by M. Terrillon of Paris to chronic collections of pus connected with diseased bone, more especially with the vertebræ and pelvis. These abscesses are often of large size, greatly in excess of the amount of caries that seems to have given origin to them. They have, when connected with disease of the pelvic bones or joints, prolongations in various directions, which M. Terrillon calls "pus-burrows or glove-fingers." These may persist or find their way to the surface, and cause numerous sinuses. In treating such cases, M. Terrillon says, the surgeon must remember these prolongations or burrows, and scrape them thoroughly and drain them. He believes that these abscesses are tubercular, and contain tubercle bacilli in every part, and therefore that, to effect a cure, every portion of the abscess must be thoroughly scraped and treated directly with an antiseptic. He employs iodoform gauze for stuffing the cavity.—*Philadelphia Medical and Surgical Reporter.*

The Technique of Multiple Amputations (John Ashhurst, Junr., M.D., etc., *Philadelphia Medical and Surgical Reporter*).—In reporting a case of successful triple amputation after railway injury, Prof. Ashhurst says: "With regard to what I have termed the technique of multiple amputations, there are some points which my experience justifies me in urging upon surgeons as of importance in promoting success. In the first place, it is very important that the time occupied by the operations should be brief; that the operations should be done systematically, so as to keep the patient under the anæsthetic as short a time as possible. The next point, perhaps of even more importance, is to keep up the temperature of the patient during the operations. I have been led to think that this is, perhaps, of more importance than anything else. Of course, loss of blood must be scrupulously guarded against, and loss of blood directly causes loss of temperature. In this case hot cans were kept around the patient during the entire operation; and, in order to save time, I operated systematically, the tourniquet and Esmarch bandage being both employed to prevent any loss of blood. I began with the most serious injury, and this is, I think, a point of importance. It may happen that, after the removal of one limb, it will be found that further operation must be postponed on account of the patient's condition, and then it is, of course, better to leave him with the less severe imjuries. In this case I began with the thigh. After amputating the limb, I secured the main vessels, which were readily found. I attempted to tie the arteries with catgut, but as the ligatures broke, I substituted silk, and, in order to save time, left both ends uncut. I next amputated the left leg, securing the vessels in the same manner, and then passed to the forearm. I then came back to the right thigh, screwed up the tourniquet and removed the Esmarch bandage, and secured all the vessels that required ligature, then passing to the other limbs in the same order as before. After the vessels had been secured in each case, a towel dipped in a hot antiseptic solution was placed between the flaps. The wounds were then dressed in the same order, and in this way the operation was completed in a comparatively short time. The points which I have mentioned I believe to be of great importance, and I think that much of the disappointment of surgeons from these operations is due to a want of attention to these matters. I should also say that, in order to preserve the bodily heat I did not use irrigation during the amputations. I think that this often seriously reduces the temperature; and even in comparatively slight operations where it has been used, I have seen the temperature fall to 97° F., and even 95° F. I think that in any grave case it is better to omit it, and to rely upon washing with hot antiseptic solutions before and after the operation. Also, the packing of wet towels around the seat of operation, as is very commonly done, tends to depress the temperature, and in grave cases should be omitted. I think that it is to an observance of these precautions that I have owed success in this case, and in many other serious operations of various character."

LYMPHADENOMA OF THE NECK (Prof. Terrillon of Paris, reported in *Philadelphia Medical and Surgical Reporter*).—"In cases of lymphomata or of lymphadenomata, the glands are augmented in volume, and this hypertrophy is constituted by a modification of their structure, which may take on two forms: (*a*) The soft form, in which the cellular element predominates and undergoes hyperplasia; the glands are then soft and almost fluctuating; (*b*) the hard form, in which the connective tissue is thickened and undergoes hyperplasia, transforming the glands into firm tumours of fibrous consistency. Whatever may be the histological modification, it is necessary to bear in mind that this affection, which takes on at the onset the characteristics of a benign tumour, may afterwards behave like the most malignant growths. The special clinical character of this kind of tumour is to begin in a single gland and to grow by the adjunction of neighbouring glands, which become agglomerated with the first one, and so on. Thus constituted, this glandular tumefaction ends by acquiring a large volume; but it presents a special characteristic, it presses back the neighbouring organs and the skin without invading them, without incorporating them into itself, a property which belongs to malignant tumours. Lymphadenoma possesses already a malignant character when there exist concurrently secondary tumours, which are easy to detect when they are cutaneous (nodules described by Prof. Trélat), difficult or impossible to diagnosticate when they are seated in the liver, the lungs, or the viscera. It is probably in cases of this kind that surgeons have witnessed speedy relapses and a rapid spread of the disease after the ablation of the primitive growth. Note in passing, that the cervical region, which is affected in the case of our patient, is a place of election for the localization of these glandular lymphomata. In lympho-sarcoma, which is only sarcoma or carcinoma of the glands, the envelope of the gland is quickly invaded and traversed, and the peripheral tissues are incorporated in the morbid mass. Lympho-sarcoma evolves rapidly; the skin is affected and ulcerated in less than ten or twelve months. Its principal character, then, is to be spread rapidly, and as it invades the neighbouring tissues, it early provokes pain, which is often very intense and persistent. It will not do, however, for you to deceive yourself by supposing that pathological anatomy knows any absolute difference between lymphadenoma and lympho-sarcoma, for I have many times intimated to you that these two affections may at a given moment of their evolution present a common clinical character, namely, malignity."

ANÆSTHESIA IN DANGEROUS CASES (Frederic Hewitt, M.A., M.D., etc., *Lancet*).—In cases of *collapse* Dr Hewitt recommends the administration of ether, not by inhaler, but on a towel, so that plenty of air may enter the lungs as well as the anæsthetic. In *heart disease* of any kind he recommends the A. C. E. mixture or chloroform. He considers that the anæsthetic power of the mixture is due to the chloroform in it, but he considers the addition of a little ether and alcohol an advantage. In *brain affections* he gives chloroform after a hypodermic of morphia, except in cases where the respiration is slow and feeble. Ether should be avoided in *chest affections* and chloroform given, and likewise when *kidney disease* is present chloroform should be given or the A. C. E. mixture. The ultimate finding is strongly in favour, therefore, of chloroform.

TREATMENT OF TRAUMATIC TETANUS (Dr J. C. Minor, *Boston Med. and Surg. Journal,* 17th Jany. 1889).—Dr Minor assumed "that tetanus was due to the formation of ptomaines of germ origin. He admitted that we could neither eliminate nor destroy the poison, and that the disease, once started, was beyond the reach of direct methods of antagonism. But when we considered the limitation of the disease and the fact that its activity was expended upon the cord and medulla, we were led to the inference that if we could so act upon the central nervous system that the force of the disease would be spent upon a benumbed tissue, if we could so modify these functions of the cord as to materially diminish its reflex excitability until the nerve-storm had passed, we should accomplish the most essential end of treatment. The problem before us, therefore, was how to put the cord and medulla "in splints" physiologically, to do this in the shortest possible time, and to maintain this condition of modified function for at least two weeks. He did not believe it possible, except by profound anæsthesia, to obtain complete relaxation of all the muscles in the first period of an acute attack of tetanus without incurring as much danger from the remedies used as from the disease. The object of treatment was, not to abolish the manifestations of the disease, but to control them to such a degree as to maintain a relatively safe condition long enough for the disease to expend itself. The effect of the disease upon the muscular system was the main point of clinical significance. Assuming that all remedies useful in the treatment of tetanus must possess the power of depressing the functional activity of the cord, the choice of remedies was not of more importance than the method of using them; and a poor remedy well used was better than the best of remedies inefficiently given. The main object being to intoxicate the spinal cord, so as to depress its normal excitability, it was evident that quite a wide range of drugs was at our service. The ones to which Dr Minor gave the preference were Calabar bean, chloral, and the bromides, with an occasional resort to chloroform. Chloroform, Calabar bean, and chloral were the most efficient in the first period of the disease and its crisis or climax; while the bromides, and also the salicylates, came into use during the declining stage in such a way as to anticipate the condition of muscular tension, and thus prevent the recurrence of spasms. Under the use of Calabar bean the periodicity naturally incident to the disease became more marked, because the remedy contributed to a more pronounced remission. Two grains might be sufficient to obtain this relief for two hours at a time during the first week of the disease, but three or four grains were usually required before the end of the second week; and the increasing violence of the disease would bring these doses closer together. As the crisis approached, the patient was in great danger from both the disease and the remedy, as both were cumulative in their toxic action; and at this stage both these dangers would be materially lessened by bringing chloral into use, and rapidly diminishing the amount of Calabar bean. During the period of increase of the disease, no matter what remedy was given, it was necessary to increase the dose in proportion to the work it had to do; and as the crisis came on we had to choose between the danger of an excessive dose of our remedy and an excessive dose of tetanine. We chose the first, because if toxic symptoms appeared we might antidote then, with the probability that the disease had spent its force at this point. To pass the crisis successfully signified recovery. Having referred to the toxic symptoms of Calabar bean, and their treatment by atropia and morphia, Dr Minor said, in conclusion, that to use our remedies timidly was to permit the disease to kill the patient; to use them recklessly was to perform that office ourselves. The responsibilities assumed by the physician in the treatment of traumatic tetanus were precisely the same as those belonging to the surgeon who undertook a dangerous and difficult capital operation. As to the general management of these cases, rest and nourishment were so important in their relation to the successful issue of treatment, that no matter how careful and thorough the therapeutic methods might be, they would certainly fail unless the strength of the patient was properly supported, and his nervous system protected against undue excitement."

TREATMENT OF FRACTURES AT THE ELBOW (*Philadelphia Med. and Surg. Reporter*, 8th Dec. 1888, Editorial).—"One point, which is often, we fear, overlooked, must be constantly borne in mind, namely, that in the normal arm the axis of the forearm is placed at an obtuse angle to the axis of the upper arm. The rectangular splint generally employed in the treatment of fractures at the elbow-joint almost of necessity brings the axis of the forearm into the same plane with that of the upper arm, and this can only be done by forcing the lower fragment into an unnatural position. The result is sure to be a permanent deformity, and more or less functional disability."

LATERAL DISLOCATION OF UNGUAL PHALANX OF THUMB (*The Dublin Journal of Medical Science*, Jan. 1889).—Mr J. Dallas

Pratt read a paper before the Royal Academy of Medicine in Ireland, Section of Surgery, on lateral luxation of the ungual phalanx of the thumb, the dislocation being complete and the direction of it inwards. He stated he believed that the case was so rare as to be almost unique. The patient, a man about forty years of age, had slipped in the street, and putting out his hand to save himself, came down on the point of his thumb. The injury, a cast of which was exhibited, was very difficult to reduce, the ordinary methods—Clove pitch, Hay's knot, Levi's apparatus, etc.—having failed. Finally, by bending the joint, so as to increase the deformity and bring the phalanx at right angles to the proximal one, and then making traction in the axis of the thumb, the bone went back into its normal position. There was no fracture, and the dislocation had not recurred a year after reduction.

Part Fifth.

MEDICAL NEWS.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—The following gentlemen passed their final examination for the degree in Medicine at the sittings held in May and June 1889, and were admitted L.R.C.P. Ed. :—Frederick William Kirkham, Norfolk; Alfred Carrol Otway, Lincolnshire.

ROYAL COLLEGES OF PHYSICIANS AND SURGEONS, EDINBURGH, AND FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW.—The following candidates passed their final examination for the triple qualification in Medicine and Surgery at the sittings held in July 1889, and were admitted L.R.C.P. & S. Ed., and L.F.P. & S. Glasg. :—William Herbert Fawcett, Weston-Super-Mare; John Edward Sang, Edinburgh; Arthur Ernest Taylor, Cardiff; James Duncan, London; Francis George Heard, Edinburgh; Lewis Vaughan Parry, Liverpool; Arthur Dowling, County Cork; Percy Charles Evans, Edinburgh; John Cowe, Edinburgh; Robert Marr, Belfast; Edmund Christopher Foston, Edinburgh; Herbert Blackburn, Manchester; George Ashton Wolfendale, Edinburgh; Bertie Stuart Johnson, Tunbridge Wells; Daniel Bartholomew Foley, Cork; Robert Fawcitt Granger, Whitby; Robert Vincent Sutton, Cork; Henry Edwin Scott, Edinburgh; Grace Clementina Charlotte Mackinnon, London; George Hargreaves, Edinburgh; Charles Edward Harold West, Edinburgh; Malcolm Edward Henry Wale, Chiswick; William Henry Bulger, Wolverhampton; Thomas Charles Duffin Johnston Cathcart, County Antrim; Arthur Chandler Wilson, Liverpool; George Tocher, Pollockshields; Peter Campbell Smith, County Durham; George Sidley, Stockport; Thomas M‘Elwaine, County Cavan; Gordon Trafforde Tuke, Edinburgh; Patrick O'Donoghue, County Cork; Alexander Geddes Vernor, Musselburgh; Edmund Raymond Carroll, County Cork; John Duff, Edinburgh; Samuel William Wolfe, County Cork; John Taylor Connell Johnson, Edinburgh; William Henry Rankin, Edinburgh; George Frederick Emery, Edinburgh; John Brown, Edinburgh; Waitman Henry Hillyard, County Down.

SIR WILLIAM JENNER has advised the Queen to give up champagne and claret for the present, and to drink whisky and Apollinaris water.—*Truth.*

LEPROSY IN INDIA.—It appears from *The Calcutta Englishman*, of the dates 14th and 22nd June, that there are many lepers employed in India in such a manner as to bring them into very close contact with European residents. An instance is mentioned in which a native of about 35 years of age, a Calcutta khitmutgar, had been in the service of European families since he was a boy, and was finally compelled to relinquish his vocation because of the advanced stage at which the disease had arrived. This man, for over a quarter of a century, had been in almost daily contact with the dishes and food of Europeans, and the dreadful nature of his disease was not known. In one of the largest and best known bazaars of Calcutta there is, the same paper states, at the present moment a native maker and seller of sweetmeats, the greater portion of whose hands have dropped off from leprosy. A Bill is to be introduced dealing with leprosy in India, but complaint is made that it is of too permissive a nature. On this it may be remarked that, considering the vast number of lepers known or believed to exist in India, it is remarkable, considering the facts above stated, that so very few Europeans do contract the disease. Before any drastic measures of segregation are compulsorily enforced—a procedure which would entail great expense—it would be well that further inquiries should be made as to the mode of propagation of leprosy, and the bearing of diet and habits on its spread. There is at present a leprosy scare, or a danger of one, and at such times legislation is difficult, and, unless guarded, is apt to be unjust. We should have liked had *The Englishman* told us whether any Europeans had acquired leprosy from those natives who served or waited on them. From the silence maintained we would infer that they did not. The risk to Europeans, though it incontestibly exists, is proportionately small. We should say that the most dangerous form would be that of a leper ayah, as there is reason to believe that children are more liable to become affected than adults; yet it is the rarest occurrence possible for European children born in India to become leprous.

PUBLICATIONS RECEIVED.

H. Ashby, M.D., etc., and G. A. Wright, B.A., M.B.,—The Diseases of Children, Medical and Surgical. Longmans, Green, & Co., Lond., 1889.

J. B. Berkart, M.D.,—On Bronchial Asthma. J. & A. Churchill, Lond., 1889.

James Braithwaite, M.D.,—The Retrospect of Medicine. Vol. 99. Simpkin, Marshall, & Co., Lond., 1889.

Edward Clifford,—Father Damien: A Journey from Cashmere to his Home in Hawaii. Macmillan & Co., Lond., 1889.

Thos. S. Ellis,—The Human Foot. J. & A. Churchill, Lond., 1889.

B. W. James, A.M., M.D.,—American Resorts, with Notes upon their Climate. F. A. Davis, Philadelphia, 1889.

John M. Keating, M.D.,—Cyclopædia of the Diseases of Children. Vol. I. Young J. Pentland, Edin. and Lond., 1889.

Ernest E. Maddox, M.B.,—The Clinical Use of Prisms. John Wright & Co., Bristol; Hamilton, Adams, & Co., Lond., 1889.

James E. Pollock, M.D., etc., and James Chisholm,—Medical Handbook of Life Assurance. Cassell & Company, Lond., 1889.

Charles E. Sajous, M.D.,—Annual of the Universal Medical Sciences. 5 vols. F. A. Davis, Philadelphia, 1889.

John Struthers, M.D.,—Memoir on the Anatomy of the Humpback Whale. Maclachlan & Stewart, Edin., 1889.

John Struthers, M.D.,—References to Papers in Anatomy: Human and Comparative. Maclachlan & Stewart, Edin., 1889.

E. Paget Thurstan, M.D., etc.,—The Canaries for Consumptives. W. H. Allen & Co., Lond., 1889.

Twenty-fifth Report of the Trustees of the City Hospital, Boston. Rockwell & Churchill, Boston.

W. Hale White, M.D., etc.,—A Text-Book of General Therapeutics. Macmillan & Co., Lond., 1889.

Part Sirst.

ORIGINAL COMMUNICATIONS.

I.—THE SYMPTOMS AND SEQUELÆ OF THE SENILE HEART.

By GEORGE W. BALFOUR, M.D., LL.D., Physician to Chalmers's Hospital.

"One of the most efficient factors in shortening the duration of life is the neglect of signs of coming disease which nature furnishes."—*Registrar-General's Reports.*

I FORMERLY[1] pointed out that all those modifications of structure which mark man's progress from the cradle to the grave are initiated by variations in the course and distribution of the blood, which in their turn are brought about by alterations in the blood-pressure induced by changes in the vascular system itself. It is to changes in the vascular system that we owe the remarkable way in which growth passes into development, and development into decay, for the mere oozing of a nutritive fluid from cell to cell, though sufficient for nutrition and for growth, has yet within it no such magnificent possibilities of development and of decay as we find when that fluid circulates within a vascular system, and in which creative design is so distinctly to be traced. Theoretically the first half of man's progress through life is, or rather, perhaps, ought to be, a series of successive triumphs and enjoyments, and the second half a very gradual cessation of all that has made life enjoyable,[2] until it finds its appropriate termination in a welcome and painless extinction.[3] Unfortunately, we all know too well how widely the reality of life differs from this ideal, except in the case of some favoured few. It is, indeed, to this divergence between the ideal and the reality of life that the medical profession owes its existence. It is the object of this profession to teach mankind those laws, the due observance of which promotes a healthy life, and to help them to ward off those dangers which threaten its premature extinction. It is appointed unto all men once to die, and it has been somewhat sarcastically assumed that the object of medical science is to falsify this dogma, and equally sarcastically suggested that a science that has this object in view is not likely to have much to boast of in the way of triumphs. This would be a fair hit if true, and would naturally be most applicable to the treatment of senile diseases, whose very name is redolent of decay. But the science of Medicine makes no vain attempt to confer an immortality on man; its sole aim is to relieve human suffering, to prolong human life, and, as far as possible, to provide a euthanasia for man when the inevitable time arrives. Indeed, the great object of medical science may be said to be to counteract the evils of civilisation, and to restore to man that state of healthy enjoyment, followed by that gradual and painless effacement which seems to have been his original lot. I know of no condition in which this is more possible than it is in connexion with what I have termed the senile heart. As a rule, the discomforts and sufferings connected with the gradual degeneration of this organ, often amounting to excruciating agony, are relieved, life is often considerably prolonged, and the end when it comes is usually sudden[1] and painless. To achieve such results in the face of a gradually increasing tendency to degenerate is surely no mean triumph for the medical art, and ought to be a sufficient consolation to its practitioners for the unaspiring character of their profession.[2]

---

[1] *Edin. Medical Journal*, vol. xxxii. p. 769; and vol. xxxiii. p. 681.

[2] "I am this day fourscore years old, and can I discern between good and evil? can thy servant taste what I eat or what I drink? can I hear any more the voice of singing men or singing women?"—2 Sam. xix. 35.

[3] *Ed. Med. Jour.*, vol. xxxiii. p. 774.

The senile heart may be regarded from two points of view: first, that of the morbid anatomist; and, second, that of the physician, while both deal with the pathology of the organ. The morbid anatomist, however, has before him the results of a completed process, while the physician sees only a series of symptoms which he connects, more or less theoretically, with a progressive movement which it is his duty to endeavour to counteract.

The affections of the myocardium found after death in aged people, and usually ascribed to senile degeneration, are pigmentary involution,[3] fatty degeneration, aneurism, and rupture of the heart.[4] These conditions are all commonly associated with atherosis of the coronary arteries, and, occasionally, with thrombosis of one or other of their branches. Other conditions have sometimes been included with these as also senile, such as sclerosis and waxy degeneration of the myocardium.[1] But sclerosis and waxy degeneration, though occurring for the most part after middle life, are not truly senile changes: the one is the result of a myocarditis,[2] and the other of a general blood disorder, both of which, like tubercle or cancer, may occur at any age. All these alterations of the myocardium belong mainly to the morbid anatomist, however, and not to the physician,—that is to say, they have no pathognomonic symptoms by which we can distinguish them during life; and this is of the less consequence, because there is not any treatment by which we could hope to remedy them. Atrophy of the heart is, indeed, not difficult to detect, but it is not common except when associated with wasting diseases, and any symptoms which may be present are only those referable to debility of the organ. Oppression of the heart by fat and occurring in obese persons may be guessed at, but cannot be diagnosed; and the symptoms ascribed to fatty degeneration of the myocardium are not only often entirely absent, even when that degeneration is extreme,[3] but they are just as frequently well marked where the heart is only feeble, flabby, and dilated, and quite amenable to treatment.[4] Angina pectoris is the symptom most usually associated with aneurism and thrombosis of the heart, but it in no respect differs from that arising from other causes, and it is only after death that the cause is discovered. With a larger experience and more accurate clinical histories I believe it will be possible to differentiate sclerotic and waxy hearts, just as even now we can determine syphilitic affections of the heart, but it is not probable that the diagnosis will ever be so profitable to the former class of cases as to the latter. Such cases, however, as those just mentioned are of comparatively rare occurrence in any practice, though, from the difficulty of diagnosis and the obstacles in the way of post-mortem examinations in private, many of those which occur doubtless pass away quite unrecognised. The senile hearts of which I am now about to speak belong to quite a different category. They are of extremely frequent occurrence, they present well-defined symptoms, often very serious in character, and they are largely amenable to treatment. I may also add that the pathology of those senile hearts seems very clear and distinct, and is of a most interesting character. The diagnosis is also, fortunately, easy, as it is of the utmost importance that this should be made early, as treatment to be of value must be commenced betimes.

---

[1] Realizing Mrs Barbauld's wish—

> "Then steal away, give little warning;
> Choose thine own time:
> Say not, Good night, but in some brighter clime
> Bid me, Good morning."—*Lines on Life.*

[2] "Are we not all, like Iapis, doomed 'mutas agitare inglorias artes'?"—*Æneid*, xii. 398.

[3] *Textbook of Pathology*, by D. J. Hamilton, M.B., etc. London, 1889. P. 587.

[4] Hamilton, *op. cit.*, p. 582, etc.

Rheumatism is usually credited with being the great source of disease of the heart. Rheumatism is a disease of youth, the heart is often incurably affected by it, and it is sad to see young men and maidens scotched, but not killed, and condemned to lead henceforth a lingering life in death. But, though rheumatism is undoubtedly a great curse, it is a mistake to suppose that it is by any means the chief offender in producing affections of the heart even in youth, and in age its influence is *nil*, saving only in the most exceptional circumstances. Confining myself solely to those cases of affections of the heart and aorta who have come to my own house for consultation, and of which I have notes, I find that during the last ten years only I have seen and made notes of over 1100 affections of the heart and aorta. Of these, 580 cases were senile hearts, 270 were young spanæmic hearts, and only 57 were cases of heart affection distinctly traceable to rheumatism. This relative proportion of rheumatic hearts to heart affections arising from other causes was to me quite an unexpected revelation; but it must be remembered that in hospitals, where as a rule the patients are probably not younger on the average, but certainly of a lower class and leading a more exposed life, rheumatic hearts will probably be found to bulk more largely; nevertheless these figures serve to show how large a proportion of human suffering depends upon premature senility.

---

[1] Hamilton, *op. cit.*, p. 588, etc. *Vide* also, *Étude sur le cœur senile*, par le Dr Ernesto Odriozola. Paris, 1888.

[2] Huber, however, is inclined to trace sclerosis of the myocardium purely to arterio-sclerosis, *vide Archiv f. Patholog. Anatomie*, Bd. lxxxix., 1882, S. 236.

[3] *Clinical Lectures on Diseases of the Heart and Aorta*, by George W. Balfour, M.D., 2nd ed., p. 309. London, 1882.

[4] Balfour, *op. cit.*, p. 348.

The PATHOLOGY of the senile heart follows as a corollary its Physiology. In early life the arteries are large in comparison to the size of the heart and the length of the body; hence there is a low blood-pressure and a rapid pulse rate. These conditions, coupled with the large amount of water in the tissues and the abundant supply of nutriment, favour the diffusion of the blood plasma and the rapid growth of the body. As growth advances it is found that the arteries do not keep pace either with the growth of the body in length or with that of the heart in amplitude; the result of this is a gradual rise of the blood-pressure and a slowing of the pulse rate, till in early manhood the blood-pressure has attained its highest norm.[1] At and often before middle life the arteries begin to lose their elasticity, and as age advances the supple, elastic arteries tend to become rigid tubes.[2] Loss of elasticity prevents the arteries from aiding in the onward flow of the blood, a greater strain is thus thrown upon the heart, the aorta and large arteries tending at the same time to become gradually dilated. In a perfectly normal declension there is probably but little of this dilatation, but under the conditions in which life is carried on in a state of civilisation, dilatability of the aorta is probably always present after middle life, and greater or less dilatation is of quite common occurrence. The loss of arterial elasticity, while it increases the blood-pressure within the aorta and cavities of the heart, makes the outflow into the capillaries approximately intermittent instead of continuous as hitherto, and thus lowers the blood-pressure within the capillary area, and this, together with the cessation of active growth, permits the obsolescence of many of the capillaries. But this withering of the capillaries acts like putting a stopcock on the arterioles, and slowly and gradually increases the intra-arterial blood-pressure, thus still further helping to embarrass the heart. About middle life, also, the relative proportion between the aorta and pulmonary artery undergoes a change : hitherto the pulmonary artery has been the larger of the two, now the aorta has the best of it. But the effect of the pulmonary artery being larger than the *aorta ascendens* is to keep the blood within the lungs at a much higher pressure than when the reverse is the case ; and, as the blood gets rid of its carbonic acid much more quickly and perfectly when circulating through the lungs at a high pressure than at a low one, the effect of this altered relation between the pulmonary artery and the aorta is to start a gradually increasing venosity of the blood, which bears its part in all the subsequent phenomena of senile life.[1] The strength of a chain is to be measured by that of its weakest link ; and man's longevity, barring accidents, among which we may include infectious diseases, depends upon his weakest organ, whether that is weak through heredity or conquest. Rheumatism with all its dire results is often hereditary ; it may also be acquired, but it undoubtedly can in either case be largely warded off by the exercise of due care in regard to environment and diet. But the senile heart with all its troubles is more often purely hereditary ; for though the arteriosclerosis upon which it depends may be acquired, or have its development hastened by injurious habits of life, and though the phenomena of the senile heart itself may be precipitated by similar causes, yet there seems to be in certain families a much stronger tendency to early loss of arterial elasticity than in others. Longevity and arterial elasticity go hand in hand, and are hereditary gifts which may be thrown away but can never be acquired, though we may often very successfully fight the tendency to decay, especially when that has been precipitated by accident or imprudence.

---

[1] *Die altersdisposition*, von Dr F. W. Beneke. Marburg, 1879. S. 14.

[2] Foster's *Physiology*, 1883, p. 690,—"One common feature of old age is the conversion by such a change of the supple, elastic arteries into rigid tubes;" and 5th edition, 1888, p. 343,—"As the body grows older the once supple and elastic arteries become more and more stiff and rigid; and often in middle life, or it may be earlier, a lessening of arterial resilience, which proportionately impairs the value of the vascular mechanism as an agent of nutrition, marks a step towards the grave."

---

[1] Beneke, *Die altersdisposition*, S. 18 ; also,
über das volumen des Herzens und die Weite der arteria pulmonalis und Aorta ascendens in den Verschiedenen Lebensaltern.
Marburg, 1879, in *Schriften der Gesellschaft zur Beförderung der Gesammten Naturwissenschaften*, S. 5,—"Gleichzeitig ergab sich aus meinen untersuchungen, dass während in den jugendlichen Altern bis nach Vollendung der Pubertät die Pulmonalis in der grossen mehrzahl der Fälle weiter ist als die Aorta ascendens, im 'gereiften alter' dies Verhältniss sich umkehrt, d. h. also, die Pulmonalis enger wird als die Aorta."

The result of an increase of the intra-arterial blood-pressure is to throw a greater strain on the walls of the left ventricle, and gradually, though very slowly to dilate it.[1] This dilatation is naturally promoted by anything that interferes with the nutrition of the myocardium, and thus diminishes its power of resistance. Hence it is helped even by the ordinary and normal loss of elasticity of the coats of the coronary arteries, and still more by the development of sclerosis within them, as well as by that venosity of the blood always more or less present after middle life. It is also hastened by any other interference with the cardiac nutrition, by the abuse of stimulants and narcotics, by any cause of weakness of the circulation or impoverishment of the blood, such as any illness, even a common cold often precipitating this affection, sexual excess, long-continued dyspepsia (often a result, but still occasionally a cause), or by any drain upon the system such as bleeding piles, etc. Moreover, senile dilatation, long prepared for by the vascular changes already alluded to, may be suddenly and seriously precipitated by some violent exertion,[2] some sudden hæmorrhage, or some intense emotion.[3] Inhibition of the heart's action from violent emotion is a well-known cause of sudden death ; contrary to expectation, joyous emotions proving even more fatal than grief or sorrow.[4] When the emotion is less intense and more persistent, it keeps up a more or less continuous inhibition in a milder form, which renders the systole less complete, producing that choking sensation we are all familiar with in these circumstances, and impairing the nutrition of the myocardium. Thus there is maintained a continued imperfect contraction with a tendency to dilate, coupled with a constantly diminishing power of resistance to this tendency; and these, coupled with the ordinary vascular conditions which after middle life favour cardiac dilatation, often precipitate this dilatation in circumstances where otherwise it might not have been either so immediate or so marked. This is revealed by all the ordinary symptoms—first by a gradually increasing cardiac erethism, followed by all the ordinary signs and symptoms of cardiac dilatation. Few of us who have been in practice for even a few years but have seen the emotion arising out of untoward family affairs, the worry of an unsuccessful business, or even the wear and tear of a too successful business, too great for the physical powers of its manager, followed by the precipitate development of serious cardiac symptoms.

---

[1] "Dilatation of the heart is in part due to increased resistance opposed to the heart's contraction, and in part to textural change in its walls."—Ziegler's *Pathological Anatomy*, by Macalister. London, 1884. Part ii. p. 49.

[2] The influence of overwork and strain in producing dilatation is now well known, and has been well described by Dr Thomas Clifford Allbutt in vol. v. *St George's Hospital Reports*, 1870 ; Dr J. M. Da Costa, *American Journal of Medical Sciences*, January 1871, p. 17 ; *Etiology and Prevalence of Disease of the Heart among Soldiers*, by A. B. R. Myers, surgeon, London, 1870 ; *Zur Lehre von der Ueberanstrengung des Herzens*, von Johannes Seitz, M.D., Berlin, 1875 ; *Die Herzkrankheiten in Folge von ueberanstrengung*, von E. Leyden, Berlin, 1886.

[3] E. Leyden, *op. cit.*, p. 47,—"Die alten aerzte wussten es sehr wohl, dass Gemüthsbewegungen und Leidenschaften, Zorn, besonders aber Gram und Schmerz Herzkrankheiten zu erzeugen im Stande sind. Die neuere Medizin hat deise Erfahrung ziemlich Vernachlässigt, doch wird jeder erfahrene Praktiker Beispiele davon anführen können. Ich selbst habe eine Anzahl solcher Fälle beobachtet. Sie haben in ihren symptomen und ihrem Verlaufe eine auffalige uebereinstimmung mit den Fällen von körperlicher ueberanstrengung des Herzens und Man könnte versucht sein sie als psychische ueberanstrengung jenen an die Seite zu stellen. Die Analogie besteht sowohl darin, dass das auffäligste symptom, nähmlich die Arythmie und die Herzdilatation sich ebenfalls in Folge von psychischen einflüssen entwickeln, als auch darin, dass zwei stadien der Krankheit unterscheiden werden können, das erste der Herzerethismus, das zweite die organische Herzdilatation."

[4] For many instances of death from emotion, *vide A Treatise on Experience in Physic*, London, 1772, vol. ii. p. 268. This is an anonymous translation of a work by Johannes Georgius Zimmerman.

The earliest symptoms of the development of the senile heart are those depending upon weakness of the myocardium, coupled with debile irritability of the nerves and ganglia connected with it, and they may all be comprised in the comprehensive term Cardiac Erethism. This CARDIAC ERETHISM has, as a rule, for its most striking exponent a rapid or irregular cardiac action. In one case only of all that I have seen *intermission* seemed to have been the earliest observed symptom, though doubtless this is no infrequent occurrence. In this case intermission was the prominent symptom to the last. The old gentleman was over 70 when I first saw him professionally, and complained of what he called an occasional "dunt." This was always associated with an intermission, and seemed to be caused by the immediately following somewhat forcible cardiac impulse, which gave him an uneasy and almost painful sensation, as it does to so many. When I first saw him his heart was already considerably dilated, and had been so for some time, as I learnt from his medical attendant. But the old gentleman was quite distinct in his statement, that many years previously, when seen professionally by a distinguished consultant in the west of Scotland, the doctor had told him that his heart presented no signs of disease, but only of nervous derangement. I have no doubt that the intermissions existed then, but there appear to have been no signs of dilatation present. There was always a strong suspicion that this patient indulged in some narcotic, but it was never brought clearly home to him, and he would never confess. He was somewhat relieved by treatment, but there was no marked improvement, and he was found dead in bed very shortly after the death of his wife, no other symptoms having developed. At times an occasional *irregularity* is the earliest and only symptom of the senile heart. A few faltering beats disturbs the regularity of the pulsation, accompanied by a slight choking sensation referred to midsternum, a momentary pallor of the face, with a slight tendency to faint, or a momentary loss of power in the lower limbs, which compels the individual to sit or lie down for a time, and sometimes makes the gait halting and uncertain for a few hours. More usually, however, *rapid action—Tachycardia*—alone is the earliest symptom of that cardiac erethism which so usually precedes and accompanies dilatation. At first the heart's action is normal as to rate, but the apex beat is feeble and the pulse shabby, but on the slightest exertion, or the most trifling emotional excitement, the heart's action becomes rapid, unequal, and irregular. By-and-by in an erethistic, dilating heart the heart beat and pulse rate become persistently rapid, and this condition may either pass away with improving health, continue indefinitely, or diverge into one of two separate directions. In a small number of cases the pulse is continually small, rapid, and feeble, and to this we have superadded a state in which the heart's impulse is feeble, and the sounds empty like the tic-tac of the fœtal heart, to which the term *Embryocardia* has been applied.[1] In the other and more common class of cases, the heart beat and pulse continuing rapid, become characterized by *extreme irregularity*, a combination to which the term *Delirium cordis* has been given, while in both classes of cases sudden attacks of the *Tremor cordis* of Senac and the earlier authors are liable to occur without warning, and passing off in a second or two apparently without detriment to the patient. These attacks are commonly spoken of as a fluttering of the heart. They vary from three or four systoles, sharp, short, and apparently incomplete, rapidly succeeding one another, and running off without warning from a heart beating steadily and regularly, up to a whole series of extraordinarily rapid, short, and unequal cardiac systoles, which convey a tremulous sensation to the hand, and are accompanied by a small, fluttering, and not always perceptible radial pulse. This tremor suddenly ends like an intermission with an unusually forcible beat, and from the same cause. The ventricle has been gradually getting overfilled during all these imperfect systoles, and forcibly exerts itself to get rid of its contents, which escape freely, and gradually distend arteries which have had time to become unusually empty.[2] The heart then settles into its ordinary rhythm.

Tremor cordis is not confined to the senile heart, it may occur at any age, and may happen in a healthy but feeble heart, or it may accompany any form of heart affection, though it is most common after middle life, and in hearts feeble and dilated. I have known a young man of 19 to be suddenly, and without warning, attacked with a sharp fit of *tremor cordis;* he still lives, over 60 years of age now, and has only recently been subject to similar attacks. All his life his heart has been somewhat feeble, apt to become readily intermittent, and always liable to palpitation on the slightest provocation, though much less so of late years. His heart has been seemingly always under the dominion of his nerves, yet he has always been the picture of health, seldom ailed, and then only in some casual way. The only severe illness he ever had was an attack of relapsing fever, for which he was sickening when he had his first, and for many years his only attack of *tremor cordis.* His chest, however, is long and somewhat narrow, and I have observed that those having this conformation have invariably irritable hearts. Moreover, he is hereditarily liable to heart affection on the mother's side. His maternal grandfather died of hydrothorax, originating in what appears to have been an advanced senile heart. One maternal uncle died from syncope over 70 years of age, having long laboured under a senile heart; another maternal uncle died from aortic regurgitation about 50; and a maternal aunt died from syncope from a senile dilated heart about 60 years of age, while his mother had an intermittent heart for many years, dying about 60 from iliac passion. Rheumatism is unknown in the family. On the father's side the ages at death are for generations in the direct line all over 80, though collaterals have from various causes dropped at earlier periods. The fatal diseases have even at that age been usually acute and inflammatory, the hereditary tendency being to death from cerebral apoplexy, but at a very advanced age.

---

[1] *De L'embryocardie ou rhythme fœtal des bruits du cœur.* Par le Docteur Henri Gillet. Paris, 1888. A condition of heart in which the first sound and short silence and the second sound and long silence are equal in duration, both sounds possessing the same timbre and intensity. It occurs in infectious diseases and affections of the heart, it is always of serious import, and often fatal.

[2] This is indicated by the large returning leaps of the mercury when a rabbit's heart has been temporarily inhibited. *Vide* Foster's *Physiology*, 5th edition, 1888, p. 296, diagram and text. Also p. 344, "mere distension of the ventricle. . . . . brings about a more forcible beat."

In a few cases the dilated senile heart is accompanied by a very slow pulse; and of this there are two forms: one in which the heart beats at double the rate of the pulse, every alternate pulsation being too feeble to reach the periphery;[1] and a second form, in which both pulse and heart beat abnormally slow, a veritable *Bradycardia.*[2]

Every senile heart has as its originating cause the loss of arterial elasticity. Hence in every such case we have an accentuated aortic sound, as an indication that the first part of the aorta is more or less passively dilated with each blood-wave,[3] according to the capacity of the ventricle, the amount of resistance to the onward flow of the blood, and the actual loss of resilience, although the aortic lumen itself does not always or necessarily remain permanently dilated. This condition calls for permanently increased exertion on the part of the myocardium, and as this always works much below its power,[1] and is extremely well nourished by two large arteries,[2] it sometimes happens that this increased call for exertion is followed by gradual hypertrophy of the left ventricle, a condition which will be referred to presently. But if the normal loss of arterial elasticity and the tendency to capillary obsolescence has markedly invaded the region of the coronaries, or if from any cause the blood has been impoverished,[3] or the nutrition of the myocardium impaired, then the call for increased exertion is accompanied by increasing inability, and the enfeebled myocardium becomes irritable and dilatable, and at last it dilates. The residual accumulation, which accompanies and induces this dilatation, appears to take place *guttatim* and synchronously in all the cavities of the heart, as well as in the closed circuit of the pulmonary circulation. Percussion is unable to detect any alteration in the heart until it has attained some considerable size; but with a knowledge of all the circumstances before him, a skilled observer is very well able by palpation and auscultation to gather enough of information in regard to the progressive changes to enable him to foresee their tendencies and to guide his line of treatment.

---

[1] Hope, *On Diseases of the Heart*, London, 1839, p. 377. Stokes, *The Diseases of the Heart and the Aorta*, Dublin, 1854, pp. 362, etc. Balfour, *op. cit.*, 2nd edition, London, 1882, p. 258.

[2] Hope, *loc. cit.*; Stokes, *loc. cit.;* and *Dublin Quarterly Journal of Medicine*, 1846, vol. ii. p. 73; also Leyden, *op. cit.*, S. 48.

[3] Balfour, *op. cit.*, p. 31.

When the aorta has lost its elasticity, it expands helplessly and abnormally before each succeeding blood-wave, and the weight of the excess of blood in its ascending portion closes the aortic valves with unusual force, and to this the term accentuation has been applied.[4] This accentuated sound is sometimes merely a louder and more distinct sound than usual, but at other times it has a booming quality, from the greater state of tension into which the valves are thrown,—a state of tension which always tends to terminate, and occasionally does terminate in separation of the segments of the valve with regurgitation between them;[5] a regurgitation which occasionally disappears, and is apparently curable, at least in its early stages.[1] It often happens, however, that the aorta is not only dilatable, but actually dilated, because its more or less sclerosed or rigid coats do not collapse as the blood-wave passes onwards, but remain permanently dilated,—a condition which may either be gradually developed *ab initio*, or which may become engrafted on a primary state of mere dilatability. So soon as the aorta becomes permanently dilated, we have a systolic murmur developed at the aortic orifice, propagated along the course of the arteries, and with a position of maximum intensity, either in the aortic area or about mid sternum, and immediately followed by an accentuated aortic second.

---

[1] Rosenbach has shown that when aortic regurgitation is artificially produced, the heart at once, by its reserve of energy, adapts itself to its altered circumstances without a trace of any fall in the blood-pressure (*Archiv f. experimentelle Pathologie und Pharmacologie*, Leipzig, 1878, S. 1); and Panum and von Bezold have shown that this reserve energy depends upon the fact that the cardiac ganglia work well within their powers, and can carry on their function for a considerable time after they are deprived of their blood supply (for hours, Panum, for many minutes, von Bezold), *vide* Virchow's *Archiv*, Bd. xxv., 1862, S. 308, etc., and *Untersuchungen ans dem Physiologischen Laboratorium in Wurzburg*, Erster Theil, 1867, S. 256, etc.

[2] Occasionally there is only one coronary, as in the case of the celebrated Dr Arnold; sometimes there are as many as three supplementary arteries (Budor, *Obliterations des artères cardiaques et lésions du myocarde*, thèse, Paris, janvier 1888). Wickham Legg has found that the coronaries anastomose through the branches at the apex (*Lancet*, 1883), so that the two coronaries may be regarded as one terminal artery. The size of these arteries is quite disproportionate to the mass of muscle to be fed, so that the heart may be regarded as having an excessive supply of arterial blood compared with other muscles (*vide* Odriozola, *op. cit.*, p. 5).

[3] *Vide antea*, p. 206.

[4] Balfour, *op. cit.*, p. 31.

[5] In the ordinary forms of dilated aorta, a systolic murmur often precedes for an indefinite period the development of regurgitation, *vide* Stokes, *op. cit.*, p. 227.

Naturally a mitral systolic murmur is also audible in the aortic area, but it is only propagated thither, its position of maximum intensity is in the mitral area. When the aortic valves, however, are themselves diseased and thickened, so as to form a permanent obstacle to the onflow of the blood, then we have a systolic murmur loudest in the aortic area, but such a murmur may usually be differentiated from one purely indicative of aortic dilatation by a history of rheumatism, and it is almost invariably accompanied by more or less regurgitation indicated by a diastolic murmur. Besides, it is comparatively rarely that we have diseased aortic valves in an aorta of normal calibre. The dilated aorta may be easily mapped out by percussion, and if there is any booming quality in the aortic second, we may be certain that the valves are healthy enough to take no share in the production of the murmur.

As regards the heart itself, the left ventricle is naturally the first part to become oppressed and dilated, yet there are many cases of heart trouble due to weak walls and failing force, in which none of the ordinary signs of dilatation are to be detected. Yet in every such heart an intelligent observer will always be able to discover some one sign or other sufficient to indicate that its cavities are dilating; and this, coupled with his knowledge of the permanency of the cause, and the importance of the ordinary sequence of events in such a case, is not only a sufficient warning to him of what he has to fear, but also a sufficient indication of the line of treatment to be adopted.

Presuming that our own heart is healthy, if we put a finger point upon the apex beat and hold the breath for a few seconds, the apex beat will be found gradually to disappear, because the left ventricle is pushed backwards into the chest by the dilatation of the right ventricle lying in front of it.[2] If, however, instead of a finger we place one end of a differential stethoscope over the left apex, and the other over the right apex at the lower end of the sternum, then, though we hear no murmur, yet we perceive a gradual transference of the force of the impulse from the left to the right apex. This arises from a temporary cause, and is at once

[1] Balfour, *op. cit.*, p. 236.

[2] Balfour, *op. cit.*, p. 191.

altered by one or two deep inspirations. But when we find a similar condition, that is, when we find by palpation or auscultation that there is a more forcible impulse at the lower end of the sternum than in the normal position of the apex beat, in an individual whose arteries are atherosed,[1] or who being over middle life has an accentuated aortic second, a firm, tense pulse, or a pulse tracing indicative of high intra-arterial blood-pressure,[2] then we know that we have to do with a heart dilating under the influence of a permanent cause. It is of the utmost importance for the patient that this condition should be recognised and duly considered in our treatment, because it is the earliest indication of a decadence which taken in time may often be successfully checked.

Next in the sequence of events we have always some alteration of the first sound; this may be prolonged, blunt, feeble, or impure; now and then we have it loud, clear, and booming; over the right apex the first sound is always more distinct than over the left apex, except the booming sound, which is a left side phenomenon, and best heard just below and to the left of the nipple. Distinctness of sound is probably as much due to the resonating qualities (thinness and flexibility) of the chest wall as to any particular state of the ventricle; the booming quality conveys to the mind the idea of tension.[3] Often accompanying one or other of the alterations of the first sound, and certainly speedily following it in orderly sequence; the educated ear has no difficulty in detecting a systolic murmur in the auricular area, in appropriate cases. This auricular murmur is a murmur audible between the second and third ribs to the left of the sternum, just outside the pulmonary area. The pulmonary artery comes to the front between the second and third ribs on the left side, one-half of its breadth lying beneath the sternum, and the other in the interspace. If we put a finger tip in this interspace close to the edge of the sternum we cover the pulmonary artery, and just to the left of the finger tip the left auricular appendix in most hearts reaches the chest wall, and if large and dilated, passes round the base of the ventricle at the root of the pulmonary artery.[4] This murmur is not always present, because the *appendix auriculi* is not always long enough to reach the surface, but if audible it may be accepted as an early and certain sign of commencing ventricular dilatation. Failing this auricular murmur, and sometimes accompanying it, we have, as an early sign of ventricular dilatation, an occasional systolic whiff over the left ventricle. For a time this whiff is transitory, more audible at one time than at another, and sometimes entirely absent, replaced by a more or less altered first sound. These variations depend on the state of the circulation, and the murmur is always louder and more distinct after exertion than when the patient has been resting. But a systolic mitral murmur, originating in progressive dilatation of the left ventricle, does not long remain trifling or evanescent. Erelong it is always to be found when listened for; it is speedily followed by a systolic tricuspid murmur, and then we have a systolic murmur in all the areas, partly propagated into the aortic and pulmonary areas, and partly produced at these orifices by the reinforcement due to the dilated condition of the aorta, and at times to that of the pulmonary artery also. By the time these murmurs have developed there is no difficulty in determining from its enlarged percussion area, and from its more forcible impulse, that the heart has become not only dilated, but hypertrophied.

---

[1] Loss of arterial elasticity increases the work of the heart, and in certain circumstances tends to dilate it. *Vide* Ziegler's *Pathology*, by Macalister. London, 1884. Part ii. p. 49.

[2] A sphygmographic pulse tracing does not indicate intra-arterial blood-pressure except indirectly. It shows whether the arterial wall moves slowly or rapidly, and we reason from that.

[3] For various views as to the state of the first sound in dilatation of the heart, *vide* Hope, *op. cit.*, p. 68, etc.; Walshe, *Diseases of the Heart*, 3rd edition, London, 1862, p. 315. He says it is "full-toned and of a higher pitch." Stokes gives a "sharp flapping character," *op. cit.*, p. 271. *Vide* also Gairdner "On the Mechanism and Sounds of the Dilated Heart," *Ed. Med. Journal*, July 1856, p. 55.

[4] Naunyn, *Berliner Klinische Wochenschrift*, 1868, No. 17, S. 189; and Balfour, *op. cit.*, p. 171.

Even when the heart has reached this stage, the decadence is often very gradual. If a healthy state of the blood is maintained, and the heart is well nourished, dilatation and hypertrophy progress but slowly at advanced ages, and I know many a one over 70 years who goes about his or her ordinary occupations and amusements without a complaint, in spite of a dilated and hypertrophied heart, the natural inertia of age contributing in favourable circumstances to prevent any great discomfort or the rapid advent of untoward symptoms. But if from any cause the blood becomes impoverished, dropsy of the lower limbs speedily follows, and if the system from any cause becomes enfeebled, untoward symptoms of various kinds become rapidly developed, a serious declension takes place in the general health, and a fatal issue is only too likely to be precipitated.

In estimating the various causes which hinder the passage of the blood from the arteries to the veins, and thus increase the intra-arterial blood-pressure, we must be careful not to overlook the condition of the tissues generally. We are too apt to figure to ourselves the blood going the round of the circulation through arteries, capillaries, and veins, as it were through naked tubes, forgetting that nutrition is extra-vascular, that the tissues are always flooded with blood plasma, and that this fluid diffuses the elastic pressure of the tissues, and binds it to that of the arterial wall.[1] However notable, therefore, loss of arterial elasticity may be in itself as a source of peripheral resistance, and of increase in the intra-arterial blood-pressure, the condition of the vascular environment has even a more important modifying influence in the same direction. From a physician's point of view, indeed, the condition of the environment is by far the most momentous of the two. For the elasticity of the tissues, like that of the arteries, lessens with age and is incapable of renewal, but the fluid filling the interspaces of these tissues varies both in amount and quality according to the state of the circulation, the integrity of the secreting organs and the quality of the blood, and is capable of being profoundly modified by treatment both in quantity and quality. It is in this direction, therefore, we must look for any modification of the blood-pressure which may be possible.

---

[1] Hamilton's *Pathology*, vol. i. p. 630 and p. 694. Donders seems to have been the first to direct attention to the importance of the vascular environment in relation to blood-pressure. *Vide Physiologie des Menschen*, Leipzig, 1856. Vol. i. S. 169.

It is also important to remember, that though loss of arterial elasticity, and increase of peripheral resistance is the main disease —the great causal affection—it is through the heart that it affects the system generally. Slowly, gradually, as it were drop by drop, the heart dilates,[1] the circulation is delayed, there is a constantly increasing venous remora, venous congestion throughout the body generally, affecting not the large veins only, but very specially the venous radicles. Hence we have the secretion of every organ in the body hampered and deteriorated, the intra-areolar fluid slowly increased in quantity, and often very greatly changed in quality, and every organ of the body more or less favourably disposed to develop fibrous tissue. These are all circumstances of importance to remember, not only as indications of the character of those affections we are to regard as sequelæ of the senile heart, but also of the lines of treatment to be pursued, which though they may vary in different cases, yet hang round and are guided by certain central ideas common to all.

The effects of undue and unprepared for exertion in giving rise to disease of the heart, have been cursorily alluded to by several of the earlier writers on that subject (Hope, Forget, etc.) But the actual effects of over-exertion upon the heart and circulation have only been formulated in quite recent times, and the literature of the subject commences with the writings of Myers and Clifford Allbutt.[2] A knowledge of the subject had been gradually growing, but these were the first to give that knowledge precision, and to record with accuracy the phenomena connected with it. It is to still more recent writers that we have to look for information in regard to the effects of internal causes in producing similar results, though a knowledge of some, at least, of these results had been known from earlier times.[1]

---

[1] That the heart dilates from stress of work following an increase of the blood-pressure is abundantly proved by Roy and Adami, in their paper entitled "Remarks on Failure of the Heart from Over-Strain," *British Medical Journal*, December 1888, p. 1321. But the mode of proof employed permits of no comparison between the natural and the artificial, the one so slow and gradual, the other so hasty in its method.

[2] *Op. cit.*, 1870.

In regard to the proof that dilatation does follow overstrain of the heart, whether arising from exertion or from internal causes, Allbutt had the presence of mind and the courage to map out his own heart by percussion while suffering from the effects of over exertion, and he distinctly ascertained that the cardiac dulness was considerably increased, and that this passed completely away on recovery.[2] Roy and Adami in the course of their experiments on the effects on the heart of an artificially produced increase of the intra-arterial blood-pressure, had an opportunity of observing not only the gradual dilatation of both ventricles from residual accumulation, but also the commencement of regurgitation due to this dilatation.[3] This regurgitation in its earlier stages is the cause of the impurity of the first sound so often present, and when fully developed it gives rise to the systolic murmur audible in such cases.[4] The cause of this regurgitation has been variously explained. Walshe[5] and M'Dowell[6] supposed it to be due to dilatation of the auriculo-ventricular openings, which made them incapable of being closed by the valves. In advanced cases such a condition may exist, but it is certainly rare,[7] disproportion between the orifices and the valves is insufficient to explain the regurgitation in all but a few cases at the most. Gairdner[8] seems to consider that excessive distention of the ventricle prevents the

---

[1] Leyden, *op. cit.*, 1886, p. 47.

[2] Allbutt, *op. cit.*, p. 29.

[3] *British Med. Journal*, Dec. 1888, p. 1323,—"Increased resistance produces diminution of the systolic contraction of the ventricles; and the quantity of blood entering the heart in any given time remaining the same, this diminished contraction is, *cæteris paribus*, necessarily accompanied by increased expansion in diastole. . . . . Expansion of the heart so produced leads, if extreme, to functional incompetence of the auriculo-ventricular valves, —an effect which is all the more readily produced the more fatigued the heart is."

[4] This is now a recognised fact, but its first recognition as a necessary complement of ventricular dilatation, and not as a mere accidental complication, we owe to Dr Gairdner, *vide* "Evolution of Cardiac Diagnosis," *Ed. Med. Journal*, June 1887; and also *Ed. Med. Journal*, July 1856, p. 55.

[5] *Op. cit.*, p, 323,—"Pure dilatation may be attended with the murmur indicative of regurgital disease of the mitral valves, though these valves be sound. On the other hand, organic mitral regurgitation may, *pro tempore*, be murmurless. Now, the first difficulty will occur where the orifice is so much dilated that its valve, though perfectly sound, has ceased to be of sufficient size for purposes of complete closure. There are no possible means of distinguishing such regurgitation from that induced by actual disease of the valve; happily the phenomenon is very rare."

[6] M'Dowell takes very much the same view as Walshe, *vide Dub. Quarterly Journal*, vol. xiv. p. 352, 1852; vol. xvi. p. 76, 1853; and vol. xvii. p. 90, 1854.

[7] *Vide* Hamilton, *op. cit.*, p. 625,—"A dilatation of the mitral opening with a healthy and competent aortic comes to pass very seldom."

[8] *Loc. cit.*, p. 68.

closure of the valve segments, and Stokes[1] seems also to have considered this an efficient cause of regurgitation. Bristowe[2] has adopted and somewhat modified this view by showing that the incompetence of the valve depends not so much upon great dilatation, as upon relative shortening and displacement of the *musculi papillares* and chordæ tendineæ, precisely as Mr King[3] has shown occurs occasionally in the right ventricle as a normal process. Pettigrew[4] seems also to hold a similar view, that debility of the myocardium and dilatation of the ventricular wall are quite sufficient to prevent that rapid and complete closure of the segments of the valve necessary to prevent regurgitation. Some years ago Hesse, one of Ludwig's pupils, stated that he had succeeded in proving experimentally that during the systole of the heart its base contracts, and the auriculo-ventricular opening is thereby so narrowed as to enable the segments of the valve to close it. Regurgitation with him obviously meant a relaxed sphincter, and it has been by some accepted in this light.[5] In the right ventricle of birds there is a sphincter muscle instead of a valve, but in all the mammalia there is no arrangement of circular fibres capable of acting as a sphincter,[6] and the muscular fibres round the auriculo-ventricular openings are all inserted at right angles to the plane of the orifices.[7] Yet the base of the heart has long been known to be contracted at the end of the systole. Thus Pettigrew says, "The more important" (third stage of the closure of the mitral valve), "as being the more active and difficult, is yet to come. *This consists in adapting the segments of the valve to the gradually diminishing auriculo-ventricular opening;*"[8] and he goes on to describe how this adaptation is brought about. It is not, however, at the end of the systole that regurgitation is most apt to occur, an evanescent whiff is often to be heard at the commencement of a systole which ends with a fairly good sound. How this may be is not easily understood, if we have to wait for the action of the sphincter at the base of the heart for the perfect closure of the valve. Neither does this theory explain how there is no murmur till the heart gets dilated.[1] The heart may be feeble, and act imperfectly, but there is never any murmur until we have other signs of dilatation; we do not always have a murmur then, but we never have it till then. The old explanation sufficed for this, and in the newest we have a return to it.

---

[1] *Op. cit.*, p. 271,—"Although in theory we do not admit the valvular murmur as a sign of dilatation of the heart, yet, on the other hand, when the dilatation of the cavities is carried beyond a certain point, valvular insufficiency may result, and then a murmur is produced, which, though having its origin in the valvular orifices, does not proceed from valvular disease."

[2] *Vide British and Foreign Medico-Chirurgical Review*, vol. xxviii., July 1861, p. 215.

[3] *Guy's Hospital Reports*, 1837, vol. ii. p. 104.

[4] "On the Relation, Structure, and Function of the Valves of the Vascular System," *Transactions of the Royal Society of Edinburgh*, 1864, p. 799; and *Physiology of the Circulation*, London, 1874, p. 284.

[5] Thus Macalister says, "Let now the muscular power of the heart, the base part of it, at least, become impaired, or the contraction become less energetic. Instead of diminishing the area of the base by a half, it languidly contracts it, say by a quarter. It will not be wonderful, therefore, if from this cause alone the valves find themselves inadequate (*British Medical Journal*, October, 1882, p. 825). *Vide* also *Medical Record*, New York, April 1889, p. 421, "The Occurrence and Mechanism of Physiological Heart Murmur (Endocardial) in Healthy Individuals," by Morton Prince, M.D.

[6] W. Savory, *Royal Society's Transactions*, 1851.

[7] Pettigrew, "The Arrangement of the Muscular Fibres in the Ventricles of the Vertebrate Heart," *Philosophical Transactions*, 1864.

[8] *Royal Society's Transactions*, Edinburgh, 1864, p. 798, and *op. cit.*, p. 283. The italics are in the original.

Quite recently another of Ludwig's pupils, Dr Ludolph Krehl, has published a communication on the mechanism of the tricuspid valve,—a mechanism and an explanation which is equally applicable to the bicuspid valve. In this communication Krehl points out that the valves are closed at the end of the ventricular diastole, and before the commencement of the ventricular systole.[2] If the valves were not closed till after the commencement of the ventricular systole, the blood between the segments of the valves, on their auricular surface, would of necessity be thrown back into the auricle. But if we trace the fall of the blood-pressure in the auricle of a healthy heart by a manometer within it, no indication of any regurgitation is ever observed. When the ventricle, however, is dilated, there is regurgitation—so-called relative insufficiency is established. Not because the segments of the valves are unable to close the auriculo-ventricular opening, but because the insertions of the *chordæ tendineæ* into the papillary muscle are so placed in relation to the centre of the cavity, that the trifling pressure of the blood flowing from the auricle into the ventricle is insufficient to approximate the segments of the valve. Whenever, therefore, the systole of a ventricle is incomplete, the papillary muscles are not brought together, the valve is not closed, and blood regurgitates into the auricle.[3]

---

[1] *Conf.* Roy and Adami, *loc. cit.*, p. 1322.

[2] *Archiv fur Anatomie und Physiologie*, Leipzig, 1889, S. 291,—Da die Klappen noch in der Kammerdiastole geschlossen werden muss (am ende derselben). There cannot, therefore, be any question of any sphincter action during the ventricular systole.

[3] Krehl, *loc. cit*,—Ebenso finden sich solche Insufficienzen, ohne Klappenveränderungen, an Herzen deren Kammern stark erweitert sind. Man hat sie "relative" genannt, und sich dargestellt, dass die abnorm erweiterte Œffnung von den Segeln nicht mehr bedeckt wird. Diese annahme kann Nichts erklären, da. wie bekannt, schon ein Segel allein beinahe hinreicht, die atrio-ventricular Œffnung zu verschliessen. Der Verschluss derselben kommt an dilatirten Herzen. ohne dass die Klappen verändert sind, vielmeher nicht zu Stande, weil die ansatzpunkte des chordæ an den Papillarmuskeln so weit auseinander, so weit von der mitte der Kammer abliegen, dass sie durch den geringen Druck des Vorhofesblutes nicht aneinander geführt werden Können. Wenn der Ventrikel sich dann in der Systole nicht vollständig contrahirt, so werden auch jetzt nicht nach der mitte geführt und es strömet auch dann noch blut in die Vorhöfe Zurück.

(*To be continued.*)

II.—SOME CASES OF INTRAUTERINE HYDROCEPHALUS, WITH REMARKS.

By C. E. Underhill, M.B., F.R.C.P. Ed.

(*Read before the Edinburgh Obstetrical Society, 24th July* 1889.)

I venture to bring this short note before the Society because cases of labour complicated by hydrocephalus are not very common—about 1 in 3000, according to Spiegelberg—and they are too often not diagnosed until serious injury has been done; and, moreover, the subject has not been brought up here for a considerable time. In the year 1878 the late Dr Angus Macdonald related a case with a valuable and elaborate commentary, and in the following year Dr Croom made a shorter contribution on the subject.

Case I.—Mrs P., aged 35, fell in labour with her ninth child on 11th February 1886. She had had seven fairly easy labours at the full time, and one anencephalic fœtus at the seventh month. She attributed the deformity to being frightened by a large dog. She was seen several times by her medical attendant, and at 2 a.m. on the 12th, as no progress was being made, and the head was still above the brim, he applied the forceps. There was a difficulty in locking them, and after making slight traction, he suspected the nature of the case, removed the forceps, and endeavoured to turn. As he found difficulty in doing this, he sent for me at 4 a.m. I found by combined external and internal examination a very large head partly pushed away from the brim, which it had not entered, and one foot in the vagina. With some difficulty I seized and brought down the second foot, and completed the turning. The neck and face came easily into the pelvis, but the head much more than filled the brim. Traction was made for a short time, but, the head not advancing, I proceeded to perforate. This was not at all easy, as I could not without risk reach the parts above the ear, and the only instrument at hand was a Smellie's perforator with one prong broken. I succeeded in making an opening in the back of the neck, which lay to the left and forwards, and then pushed in the instrument, intending to perforate through the foramen magnum into the skull. A large quantity of fluid escaped, and the head then came through without any difficulty. I found the perforator had entered just below the occipital protuberance. The fluid was, as usual, contained within the ventricles, as it did not flow until I had pushed the perforator well in. There was a spina bifida as large as an egg about the middle of the dorsal region. It was not tense. I punctured it as a first step, in hopes that the fluid contained in it communicated with that inside the cranium, but very little came. The placenta came away easily, but the membranes were retained, and I had to introduce my hand into the uterus to remove them. There was a good deal of hæmorrhage, but the patient made an excellent recovery. The fœtus was a female, and the head very large—the largest I had then seen in the fresh state. This case occurred in Staffordshire, where I was staying for a few days.

CASE II.—Within a year after the occurrence of Case I., viz., 26th July 1887, I saw, in consultation with a friend near Edinburgh, a very similar case. The patient, Mrs P., aged 38, was a v.-para, all of whose previous labours had been easy. The breech presented and came slowly down, but the head was delayed above the brim. Steady traction failing to advance matters, an effort was made to apply forceps, but when with difficulty they had been locked, the handles were seen to be wide apart. I was then sent for. With the previous case fresh in my memory, I naturally guessed, before seeing the patient, that the difficulty might be due to a similar cause. On making a careful combined examination, there was no difficulty in deciding that the head was of great size, as the external hand found it completely filling and overlapping the inlet of the pelvis, while the internal hand made out the widely separated cranial bones and the tense and elastic condition of the sutures. The cord had for some time ceased to pulsate. Drawing the body well up in front, I easily perforated with Simpson's perforator just above and behind the left ear, and let out a quantity of fluid which was estimated at three pints. The head was then easily pulled through. It was larger than the previous one, and the body was, as is not common in these cases, fat and well nourished. The mother recovered well, and has had a healthy living child since.

These are the only cases I have met with where there was a very large quantity of fluid within the cranium, but I have met with four more where the condition was sufficiently well marked, and in one of which the mother's life was lost. I have only preserved very brief notes of them, and shall give them in the order in which they occurred.

CASE III.—On 24th November 1871 I attended Mrs W., aged 24, in her second confinement. She had borne healthy twins at the full time on 21st December 1870. The labour was an easy one, but I was puzzled by the condition of the presenting part, the head, owing to the extreme looseness and mobility of the bones. When the fœtus, a female, was born, it was found to have been dead some days, and the skin was peeling off. The head was very large and collapsed; it had evidently been hydrocephalic, and the fluid had escaped in some manner which I could not discover. I opened the head, and was surprised to find the brain to be reduced to a small mass not much bigger than a walnut, and reminding me of the appearance of the stump of the brain sometimes seen in cases of anencephalus. Had the fluid been retained in the cranium, and the child been alive at the time of labour, there would certainly have been much difficulty. The mother had an attack of perimetritis, brought on by getting up and walking about on a cold floor in her night-dress on the second day after delivery, but eventually recovered.

CASE IV.—On 13th June 1880 I was called to Mrs R., aged 31, in labour for the fourth time. Her previous labours, she told me, had been normal. I found the breech had been born for some time, and the cord had ceased to pulsate. The arms were easily pulled down, but there was great difficulty in bringing through the head. The pelvis seemed of full size, but the head remained at the brim; the question of hydrocephalus occurred to us, and another practitioner who was associated with me in the case went for a perforator. In the meantime, by steady dragging on the body and pushing down the head from above, the fœtus, a male, and dead, was delivered before his return. The head was large and tense, and contained a moderate quantity of fluid. The birth was followed by a good deal of hæmorrhage, and I feared the cervix was torn, but, with the exception of some incontinence of urine, which lasted for twenty-four hours, the recovery was satisfactory, and entirely without rise of temperature. The patient, however, suffered for some months from a feeling of discomfort and looseness in the pelvis. I regret that I did not wait and perforate in this case, as the child was already dead, and dragging so large a head through the soft parts put them to a degree of strain and tension which it would have been better to avoid.

CASE V.—On 30th May 1882 I was asked to help a friend in delivering Mrs E., a short distance out of town. I have no note of her previous history. The head presented in the fourth position, and forceps had been several times applied, but had slipped off. When I first saw her she was a good deal collapsed, there was a hæmatoma of no great size on the left side of the vulva, the head was above the brim, it was soft and pulpy and freely movable, and I was suspicious that the uterus was ruptured. The case being urgent, I turned without loss of time, and delivered with some difficulty a dead hydrocephalic fœtus. The placenta was subsequently removed from the peritoneal cavity. The hæmatoma gave way during these manipulations, but to my surprise there was very little bleeding from it. The patient was, however, by this time in a very weak state, and she died from shock a few hours later. The fœtus was large, and the head contained a moderate quantity of fluid.

There was no post-mortem on this melancholy case, and the patient was far too ill for me to attempt to make any exact estimate of the amount or position of the rupture in the uterus.

CASE VI.—On 29th September 1882 I was asked by a friend to see with him Mrs M., aged 39, in her eighth labour. She had been delivered without difficulty in her previous confinements. The breech presented and came down easily, but the head had been retained in the brim for two hours before my arrival. From the history of the case I suspected a hydrocephalus, but getting my fingers into the mouth, I was enabled to pull the head through without much trouble. The fœtus was a large and fat male with a huge collapsed head, which when full of fluid must have been of great size. I examined carefully for any tear in vaginal outlet or in cervix, but could find none. A quick recovery followed.

Remarks.—The difficulty in dealing satisfactorily with cases of hydrocephalus lies confessedly in the diagnosis; or rather, it may be said that as the condition is a rare one, it is apt to be overlooked, because no thought or suspicion of it has entered into our minds. For when once we suspect that we have to do with a hydrocephalic head, and direct careful attention to the point, the difficulty is in most cases not very great. Now, I think it might almost be laid down as an axiom, that when in a woman who has previously had easy labours, and who is otherwise in good health, we find the head delayed above the brim in spite of severe labour pains, we ought at once to suspect and to look for hydrocephalus. The history of midwifery contains numerous records of cases such as this, where the condition was not discerned until too late, because it was never suspected. And when we do look for it, I believe external palpation to be perhaps the most valuable means of diagnosis. The head is felt easily above the pelvis, filling up and overlapping the edges of the inlet of the pelvis. And the diagnosis is confirmed by a combined examination, the internal hand both assisting to measure the size of the head, and also discovering the widely separated bones and the tense sutures of the distended cranium. In fact, if we have the possibility of this condition in our minds we shall not often overlook it. In my opinion the diagnosis is more easy when the feet present, or after the fœtus has been turned. It is then, it seems to me, more easy to estimate the size of the skull, as well as to feel the bulging in the temporal regions. Sometimes the diagnosis is assisted by finding the body of the fœtus small and emaciated, and yet the head unable to enter the pelvis. But this cannot be relied on, as in two of the cases just related the body of the child was large and fat.

A further point, which sometimes assists in making the diagnosis, is the relatively small size of the face in comparison with the bulky forehead, with its large frontal suture and widely separated frontal bones. The earlier the diagnosis is made the better, because it must not be forgotten that the danger of rupture of the cervix—the great danger in these cases—is greatest before the os is fully dilated. The round incompressible head does not dilate the os as quickly as the head of ordinary shape and moulding, and all this time the upper part of the cervix is stretched and thinned to a dangerous degree.

There are two conditions in which the history of the case gives us no help, and here we are more likely than ever to overlook it. The first is in primiparæ who have no puerperal history, but fortunately hydrocephalus is a very rare complication of primiparity. The second is in pelves known or found to be contracted, where the examination is in itself more difficult, and we are also the less likely to look for hydrocephalus, because there is another and simpler reason for the delay in the labour. Moreover, it is much more difficult in such a case to estimate justly the relative parts which narrowness of pelvis and size of head play in obstructing the labour. In these circumstances the diagnosis will rest mainly on an accurate bimanual examination under anæsthesia.

With regard to treatment, the question arises, Where the diagnosis of hydrocephalus has been made, are we justified in applying forceps in the first instance; or if the child presents by the breech, are we justified in making prolonged traction before we have recourse to instruments for extracting the fluid? My own feeling is that we are not. The danger to the mother is so imminent, and the life of the child is of so little worth even if it be born alive, that I think there should be no hesitation in taking at once the decisive step. For a similar reason I cannot think that it is good practice to perform version in these cases. By such a course there is very little chance of saving the life of the child, and the risk to the mother is certainly increased. It is much easier to pierce the presenting than the after-coming head; indeed, if the head be very large, and the pelvis at all narrowed or the vagina tight, perforation of the after-coming head presents considerable difficulties. In one recorded case this difficulty was so great that it was found necessary to puncture the skull through the hard palate; and in my first case I found it by no means easy to make my way into the cranium through the back of the neck.

It is generally recommended to use a trocar for puncturing the skull, so as to give the child the very slender chance of surviving, as the operation done in this way is not necessarily fatal. In the two cases in which I opened the head the child was already dead, and this question did not arise. I used the perforator because it was at hand. In two of the cases it was noted that when the child was born the fluid had disappeared, and the head was baggy and collapsed. Sometimes the head gives way during the course of the labour, and the fluid escapes then; but in one of these cases, in which the child was dead and decomposing, it had certainly been absorbed, or made its escape some time before.

III.—PLEURISY AND PLEURITIC EFFUSION FROM THE ANATOMICO-PHYSIOLOGICAL STANDPOINT.

By ALEXANDER JAMES, M.D., Assistant Physician, Royal Infirmary, Edinburgh; Lecturer on the Practice of Physic, School of Medicine, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 1*st May* 1889.)

IN whatever way induced, the essential changes from the normal in pleurisy are hyperæmia with infiltration of the serous and subserous tissue, proliferation of the epithelium and of the protoplasmic connective tissue corpuscles, and effusion of inflammatory lymph, all resulting in the formation of an inflammatory false membrane on the pleural surface, and the collection of an inflammatory fluid in the pleural cavity. As this fluid increases in amount space for it is made mainly by the collapse of the corresponding lung, the inflamed pleural surfaces being thus separated. Sooner or later the inflammatory process terminates, and the effusion of inflammatory lymph ceases. Reabsorption of the fluid then occurs, in part through the bloodvessels, and in part through the lymphatics, the numerous entrances to which in the form of stomata exist upon the costal, and possibly also upon the pulmonary and diaphragmatic pleuræ. With the reabsorption of the fluid the lung expands. In favourable cases the pulmonary and parietal pleuræ are soon again in close contact, the thin, or perhaps rather the thinned, layer of false membrane between them becoming organized, and serving to form more or less firm adhesions. In less favourable cases the processes of fluid absorption and lung expansion do not work together so smoothly and uninterruptedly. As the result of delay in the fluid absorption process, allowing the lung to be bound down by firm, organized bands or layers of false membrane, the absorption of fluid is not accompanied by a corresponding expansion of the lung. There results, therefore, (1), a drawing together of the surrounding parts, namely, chest-wall, mediastinum, and diaphragm; (2), a thickening and fibrosis of the pleuræ and false membrane; and (3), with a fibrosis of corresponding portions of the lung substance itself, a traction upon it outwards so as to produce dilatations—fusiform, saccular, or irregular—of the bronchial tubes.

The degree to which all these morbid processes are manifested varies within wide limits, but this does not concern us at present. What does concern us is as to how they are explained by anatomy and physiology, and as the above epitome of the pathology of pleurisy and effusion resolves itself into three parts, namely, pleural inflammation, fluid effusion, and fluid reabsorption, it will be most convenient to consider the data which the anatomy and physiology of the lungs afford in connexion with each of these.

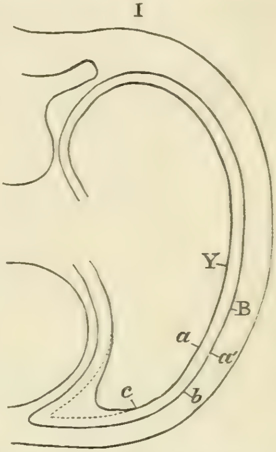
The first point, therefore, which seems to require notice is the *locality* or *site* at which the pleuritic process is most prone to manifest itself. In the vast majority of cases of so-called idiopathic pleurisy this is in the lower and lateral region. As the patient says the pain is in his side, and it is here that the characteristic friction sound is to be heard most distinctly and loudly. Of course, in secondary pleurisies the locality may be very different, but this can be easily explained. Thus, tubercular deposit at the lung apex readily involves the pleura and causes an apical pleurisy, and the great frequency of adhesions at the apices and posterior borders of the upper part of the lungs corresponds with the tendency of phthisical lung disease to manifest itself at the apices and extend along the posterior borders. Farther, the virus from a tubercular peritonitis may pass through the diaphragm and cause a diaphragmatic pleurisy, and malignant disease of the glands of the neck, of the glands of the axilla, or of the mammary gland, readily involves those parts of the pleura which are in closest proximity. At present, however, we have to do with so-called idiopathic pleurisy, and the question again is, Why should it be most common at the lower and lateral regions of the chest?

One explanation is, that it is here that the chest-wall is thinnest, and consequently the great irritant cold will best be able to exercise its injurious effect on the underlying pleura. Certainly all of us who have daily opportunities of examining chests can understand the importance of this explanation, for we know that this is, even in not very well nourished individuals, the only place where ribs and interspaces are not rendered invisible by overlying muscle. Against this theory, however, it may not unjustifiably be argued, (1), that the effects of cold are not necessarily manifested at the locality exposed; (2), that inflammation of another serous surface, the peritoneum, can seldom be ascribed to cold; and (3), that in women, in whom the acknowledged evils of low-bodied dresses have to be considered, pleurisies show no special tendency to affect the upper part of the chest. Still the theory is one which deserves notice.

But a consideration of the movement of the lung in the pleural cavity gives us further information on this question of locality. Thus, if in a living animal pleural windows be made, *i.e.*, if small portions of skin, fascia, and intercostal muscle be removed at different parts of the chest, so that through the transparent costal pleura the movement of the pulmonary pleura can be demonstrated, it will be seen that the amount of this movement is greatest at the lower and lateral regions, and becomes gradually less and less as we pass upwards or backwards, so that the apex and posterior border of a lung are practically fixed points. Hence the part of the pleura where symptoms and physical signs show pleurisy to be most common is the part of it where this movement of the pulmonary against the costal pleura is most marked; and it may be suggested that the correspondingly great stretching and relaxing of the pleural membrane which this implies will render it at this locality specially liable to be affected by morbific influences.

What is probably, however, of more importance in connexion with our efforts to explain why pleurisy seems to be specially prevalent at this lower and lateral region of the chest is the fact that, as the result of the greatest movement being at this locality, a pleuritic process once originated will here have a special tendency to be rendered more severe.

This has been well shown by Rindfleisch, and can easily be understood from Diagram I. This, which represents a section through the chest about the level of the sixth rib, shows the lung in the position of expiration, the area included by the dotted line representing its increase in inspiration. Suppose that at the point *a* on the pulmonary pleura we have a focus of inflammation. During expiration it will be opposite a^1 on the costal pleura, and will infect it at that point, but during inspiration it will be moved forward, and will infect the costal pleura at *b*, and with the next expiration a portion of pulmonary pleura further forward will be opposite to *b*. This in turn will be infected, and with the next inspiration the costal pleura opposite *c* will be involved. In time the pulmonary pleura at *c* will become affected, and so with the respiratory movements the inflammatory process will be transmitted forwards. Similarly, it will be transmitted backwards, B, Y being with the respiratory gliding of pulmonary and costal pleuræ successively affected.

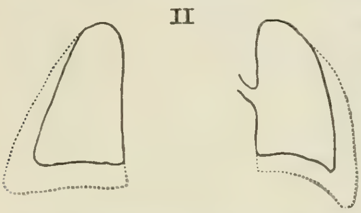


Seeing, then, that this movement spreads the pleuritic process, it follows that where it takes place to the greatest extent the pleuritic process must be most marked and severe. A small focus of pleural irritation, which at any part of the apex or posterior border of the lung would be able to induce but slight irritation and inflammation, has much greater potency for harm at any part of the lower and lateral regions.

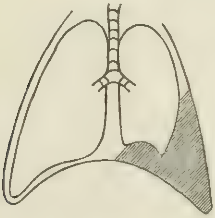
The great proneness, therefore, of pleurisy to affect the lower and lateral portions of the lung may be thus ascribed to the great amount of respiratory expansion and recoil rendering the parts there more vulnerable, and tending to a correspondingly great extent to spread and aggravate any morbid change in the pleura which may have occurred. Conversely, the fact that in a phthisical lung the pleuritic adhesions almost always present are so limited to the parts most affected by tubercular disease, *i.e.*, to the apices and posterior borders of the upper part, and show little tendency to spread, may be ascribed to the very slight amount of movement of lung in those regions. Farther, it is not unjustifiable to consider that the relative absence of symptoms and physical signs (pain and friction sounds) in such phthisical pleurisies may be similarly explained.

Effusion of Fluid.—As the fluid begins to collect it first fills up the so-called complemental space of the thorax, *i.e.*, the space between the diaphragm and costal wall into which the lung passes at each inspiratory increase. As it accumulates, more and more space is allowed by the progressive collapse of the lung; but now it is to be remembered that this collapse does not take place to an equal extent in all directions, but that it is most marked in the lower and antero-lateral region, where, as we have just seen, lung movement is greatest. Thus, in Diagram II., which represents a lung looked at laterally, and from the front, the greatest amount of collapse is seen to be from the lateral and lower portion inwards and upwards towards the lung root. Hence, as a lung collapses in the pleural cavity, the space formed by the separation of the pulmonary from the costal and diaphragmatic pleuræ is peculiarly shaped. Looked at on section from in front (Diagram III.), its base is formed by the diaphragm, its outer border by the costal pleura, its apex by the junction of costal pleura and lung, and its inner least regularly shaped border by the lung and mediastinum. Looked at next from the lateral aspect (Diagram IV.), the upper limit of this space, formed by the line of junction of pulmonary and costal pleuræ, is arched, the arch being highest laterally and lowest posteriorly. Hence, then, as fluid collects in the pleural cavity, its surface is not a level one like that of fluid in a jar, but a curved one.

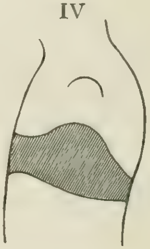
II



III



IV

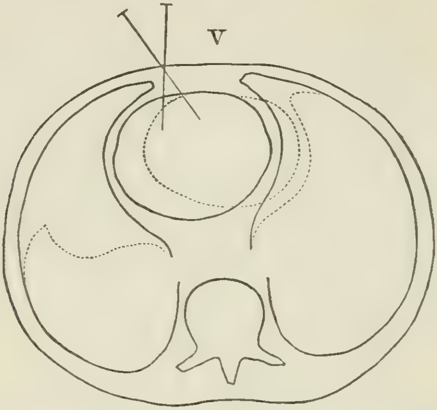


This curved line was well demonstrated by Garland of New York, by injecting warm fluid cacao butter into the pleural cavities of animals and of man after death. On opening the pleura after the cacao butter had cooled and solidified, he found its upper surface curved as described.

Garland expressly states that in his experience, "even with injections which filled one-half of the pleural cavity there was little or no intrusion of the fluid between the lung and chest wall, but the model all lay between the lung and the diaphragm." Post-mortem examination in pleurisy, however, will often reveal a condition more like that depicted in Diagram III. where this intrusion has occurred. This curved line can also be demonstrated by percussion in disease. Thus, in pleurisy, whilst effusion is going on, it may be recognised, more easily, however, in hydrothorax with the patient in a sitting posture, and probably most easily in pleuritic effusion when reabsorption of the fluid has begun to take place. Of course, for its manifestation, certain conditions must be fulfilled. Thus a certain amount of fluid is required. If the pleural cavity is quite full, the lung is not only collapsed, but is airless and even compressed, and percussion dulness is met with all over. With reabsorption, however, resonance begins to show itself anteriorly and posteriorly, and the curved line is manifested. Gradually, as reabsorption goes on, the arch of the curve gets flatter and flatter, till finally it disappears altogether. Again, in order that this curved line may be demonstrated, there must be no adhesions from previous pleurisies to bind together at this or that point the pulmonary and costal pleuræ, and, of course, the lung tissue itself must be healthy.

Next, in this connexion, the influence of the position of the patient on fluid collections in the pleural cavity requires notice. In hydrothorax, or in the accompanying or ensuing fluid effusions of pneumothorax, this influence is well marked, the percussion dulness caused by the fluid altering its position with that of the patient quite readily. With the fluid of a pleurisy, however, it is different. Here the pulmonary and costal pleuræ tend to adhere wherever they are in contact, and although in the early stages, when the fluid is increasing in amount, its position, as ascertained by percussion, may alter to some extent on altering the position of the patient, in the later stages, when reabsorption is occurring, the adhesion of costal to parietal pleura is so firm that this does not take place. Further, the precise position of a curved line (see Diagram IV.) may vary to some extent according as the patient has been lying in bed or moving about during his illness. In the former case it will, as might be expected, show the highest portion of its arch more posteriorly.

But with the accumulation of fluid and collapse of the lung other changes are associated. These are displacement of the mediastinum, with the heart to the opposite side, and displacement of the diaphragm, with the liver, stomach, etc., downwards. In most of the text-books these displacements are said to be due to pressure of the effused fluid, but this is in many cases an error. As regards the mediastinum and heart, this has been shown very clearly by Douglas Powell. Suppose that on the body after death we push a long needle through the fourth left interspace, so that its point enters the heart, as is shown in Diagram V.,



which represents a section through the body about the level of the fifth rib. If next we make a free opening into the left pleural cavity, air enters and collapse of the lung occurs; but what is of most importance to notice is, that the heart is displaced to the right side, as indicated by a tilting of the head of the needle to the left. Here nothing has pressed the heart over, it has simply been drawn over by the elastic retraction of the right lung. Under ordinary circumstances the heart and mediastinum are, as it were, held in position by the two lungs drawing them each to its own side: if this traction is abolished on one side by letting air in, as in Powell's experiment, or by fluid collecting, as in pleurisy or hydrothorax, the heart is displaced towards the other. It will thus be seen that for this displacement pressure need not be existent at all. As regards the diaphragm the case is different. Supported from below by the pressure of the abdominal viscera, its normal arched form is practically independent of lung traction on its upper surface. Hence an accumulation of fluid in a pleural cavity does not tend to lower it as the result of abolishing lung traction, but does so only when the fluid is sufficiently great in amount by its weight, or by causing positive pressure, to press it down. Clinically, the point of importance in connexion with this is, that whereas displacement of the heart and mediastinum occurs with even small accumulations of fluid, and is, therefore, a common and an early sign of its presence, displacement of the diaphragm, which can only occur with considerable effusion, is a less common and a later sign of the disease. On the other hand, it is to be remembered that in prolonged cases, where a lung has been bound down and reabsorption of the fluid is occurring slowly, a traction upwards of the diaphragm is specially apt to take place.

But there are other two clinical points of special importance in connexion with what has just been stated. The first is, that if by means of a manometer the pressure of pleural effusions be tested, it will be found that even with large effusions this pressure is never very great, and that with effusions considerable in amount the pressure may be negative. Thus Homolle states, that the greatest positive tension which he obtained in his cases was 15 mm. Hg., and the greatest negative pressure was 38 mm. Hg. in inspiration, and 33 mm. Hg. in expiration. In empyemas the positive tension was always greater than in serofibrinous effusions.

The second is, as regards the amount of fluid which can be contained in the pleural cavity. It is evident that this amount must vary with the size, age, etc., of the individual, but in an ordinary adult a pleural cavity may be considered to be full when it contains 100 to 120 ounces. With a quantity of about 60 ounces, percussion dulness will be met with up to the third rib or interspace, and with 35 or 40 ounces up to the sixth rib or interspace. With a quantity of 8 or 10 ounces percussion dulness will be just recognisable about the base or lower axillary region.

Reabsorption of Fluid.—In connexion with the reabsorption of the fluid in pleurisy, Dybkowsky's experiments are, from the anatomico-physiological standpoint, probably of the greatest interest and importance. That observer demonstrated, that if watery solutions of grape sugar or gum arabic, or if water containing in suspension minutely divided particles of Berlin blue, were introduced into the pleural cavity of an animal, these could in time be absorbed. But he also showed, that for the absorption of either of these the performance of respiratory movements was necessary. Thus, if they were injected into the pleura after death, or into the pleura of a living, quietly breathing animal, none or little was absorbed. Injected, on the other hand, into the pleura of a dead animal, whilst powerful artificial respiratory movements were being induced, or into the pleura of a rapidly breathing, living animal, absorption took place readily. Dybkowsky further showed that whatever increased the elasticity of the lungs favoured absorption, for when the trachea of the animal experimented upon was connected with a space in which the air pressure was lowered, absorption was increased, while if it were connected with an air space with higher pressure, it was diminished or arrested.

In the case of the fluid solutions, this absorption seemed to take place as the result of filtration and osmosis through all the surrounding parts,—costal, diaphragmatic, mediastinal, and pulmonary pleuræ,—into the underlying bloodvessels and lymphatics. In the case of the solid particles of Berlin blue, however, it was different. In all his experiments, he found that the absorption of these was limited to the costal pleura, and that there it took place through stomata into the lymphatic vessels. These stomata and lymphatics, he further demonstrated, were limited to the intercostal spaces,—none were found on the portions of pleura corresponding to the ribs.

From these experiments certain important practical deductions can be drawn.

I. That although in the early stages of pleurisy the respiratory movements tend to spread and intensify the pleuritic process, in a later stage they tend to promote reabsorption of the fluid effusion.

II. The fact that the absorption of minute solid particles from the pleural cavity is brought about by the stomata and lymphatics of the costal pleuræ, is of clinical importance in that when in a case of pleuritic effusion, approximation of the ribs has occurred, these stomata and lymphatics will be rendered correspondingly incapable of performing their function. Further, the mobility of the side being correspondingly impaired, the benefit of the respiratory movements will be to an equal extent awanting.

III. The fact that by breathing an attenuated atmosphere the process of reabsorption is favoured, is of importance in connexion with the ærotherapeutics of pleuritic effusion.

IV.—SCANDINAVIAN STUDIES.

By John Boyd, M.D., Slamannan.

There is abundant evidence that the countrymen of Linnæus and Berzelius are still advancing strenuously in the paths of scientific progress. From the number and importance of the medical publications we have received from Stockholm, I have for some time past felt it would be becoming to make mention of them in the pages of the *Edinburgh Medical Journal*, which might be of interest to the readers thereof.

The first booklet I shall discuss is by Dr Otto G. Wetterstrand, entitled, *On Hypnotism applied to Practical Medicine* (*Om Hypnotismens Anvãndande i den Praktiska Medicinen*), pp. 90, stated in his förord or preface to be written entirely for medical men, calling their attention to a method of treatment which he has found successful when other therapeutic agencies have proved powerless. This designation combines a number of different conditions of the nervous system, variously called forth, including all degrees, from a slight sensation of torpor of the extremities to the deepest slumber, whence the subject cannot be wakened by the loudest sound; where all sensation is abolished, so that serious surgical operations might be performed utterly unfelt by him, and, in Ovid's words, he is "gelidæ mortis imago." The susceptibility to be so influenced is very general. Out of 1011 subjects Liébault found only 27 inoperable; the author only 19 of 718 hypnotised by him since the beginning of 1887 did he find intractable. As to temperament, he noticed that nervous persons are most difficult, and in this point he disagrees with Charcot and his pupil Bottey, who would connect it with hysteria. The most marked case of somnambulism he ever encountered was in a healthy 17 year old girl (*frisk* 17-*arig flicka*), who had never been ill a day in her life. As to character, he found egotists less easily operated on than nobler natures, but the highest and deepest intellects were equally open to suggestions with those barely above idiocy. Age is an important factor. Children from 3 or 4 to 15 are susceptible, up to 30 years—after which there is less aptitude. However, some old people are easily enough affected, in evidence of which he details the case of a female of 69, who was effectually relieved by it from incontinentia urinæ diurna et nocturna of some years' duration. The method of Bernheim in his important work on suggestion is that used by Wetterstrand, who also quotes respectfully the work of Braid, 1843, but considers a Portuguese priest, Abbé Faria, who wrote in 1815, as having somewhat forestalled him. He proceeds to describe the various degrees of the hypnotic state, which Liébault divides into five.

1*st grade*, in which the patient lies still, feels a certain sleepiness and heaviness, understands all going on around him; no catalepsy.

2*nd grade*, a deeper state; patient cannot open his eyelids, cannot move his body or limbs; the catalepsy is most characteristic.

3*rd grade* is marked by automatic movements, which can be called forth by the operator—rotation of arms, etc., continued till made to desist.

4*th grade* is distinguished by the patient being in direct relation with the operator, hearing his voice alone, and remembering afterwards what *he* said, but no one else.

The 5*th grade* is the deepest and most exquisite form, in which sensibility is deadened, and the relations between operator and subject the most complete. The suggestions of the former tell on the latter with lightning rapidity.

Dr Wetterstrand next describes the *modus operandi* as applied by Liébault by vocal utterances, in firm cries fixing the patient's attention, and ordering the morbid symptoms to disappear—which implies some special power or nerve force on the part of the physician.

The method of Braid is applicable sometimes, particularly if the other plan fails. Then follow a series of cases of this treatment on nervous cephalalgia, of which I shall translate two as specimens of the author's claims:—

1. Hanna B., single, 19. For three years past she has suffered every morning from severe headaches. She is unfit for work till forenoon, but after midday feels better, though never free from pain. She looks pale, but cannot be called anæmic. Has slight gastric symptoms, but infrequent, and cannot be connected with

her head suffering. On the 1/5 1886 I hypnotised her the first visit to the third grade, and after the second to somnambulism. In all she received six operations, and on the 19/5 was perfectly well.

2. Elise R., married, 33; headache for eight or eight and a half years every day; head heavy, and feels as if icy cold; sometimes giddiness comes on so severe that she cannot walk on the streets from fear of falling; feels heavy and weary, unfit for locomotion; appetite feeble; no organic symptom, but pale and thin. The first séance, 3/5, to somnambulism; 9/5, perfectly well, free from all sickness, and appetite good.

A number of cases of chorea are recounted wherein similar success was obtained, as also one of spasmodic convulsions; but of special interest are the results of hypnotism in stammering, as for example:—"On the 2/4 I hypnotised Maria M., 32, a strong, powerfully-built woman, but from infancy suffering from stammering in the highest degree, so that sometimes she could not utter a single word. To my surprise, when hypnotised, she stammered none—spoke freely and fluently. She is specially somnambulent; in that condition could be made to converse at will, but could not begin a topic, answering my questions. I could make her open her eyes, walk through the room, sit down and write, or read a book, without a trace of her infirmity. I held a consultation with Professor Wising and other medical friends. He placed himself *en rapport* with this patient, who heard and answered him, but neither me nor any other. She gave the idea that the stammering was so abolished that she could not be made to repeat it. This case induced me to study extensively the malady in question, which is not infrequent, and always deplorable. I have since treated for it at least twenty-six persons, and in most cases the results were encouraging. The pathology of it is well expressed in Colombat's work.[1] Stammering, so far as I see, is never of organic origin, hence the inutility of Dieffenbach's operation.[2] My cases were in 18 males from 27 to 5 years—6 cured, 10 improved, 1 no better, and 1 unknown; in 8 females from 32 to 5 years—4 cured, 2 improved, and 2 unimproved."

In neuræsthenia, in which Weir Mitchell obtained so many brilliant results, Dr Wetterstrand had 17 cases, of which 6 failed, 4 cured, 4 *blifvit förbättrade* remained improved, and the rest under treatment. Cases of hysteria, amblyopia, nervous deafness, chronic alcoholism, and even rheumatism, appear to have been decidedly benefitted by the same agency.

In phthisis there is claimed for it a soothing euthanasia at the later stages, as also an improvement in cardiac pulsations, where he follows Dr Balfour, of Edinburgh, in discriminating between those of organic origin and nervous-trouble causation; giving comparative sphygmographic tracings of the improvement effected by suggestions. Diarrhœa and vesical and renal maladies appear to come within the range of his experiments, as well as disorders of menstruation.

---

[1] *Traité de tons les vices de la parole et en particulier du bégaiement.* Paris, 1843.

[2] Rohlf's *Geschichte der deutschen Medicin.* Leipsic, 1885. Vol. iv. p. 150.

On reading the 24th chapter, "Hypnotismens användning vid barnförlossning" (the Application of Hypnotism to Child Delivery), amused interest was felt. The case was a tall, well-developed matron of 25 in her sixth pregnancy; otherwise healthy, she complained of severe headaches, which impaired her memory and thinking power. She developed great susceptibility to hypnotism, and after some weekly séances was freed from cephalalgia. The case appeared most suitable for application of this agency to removing the pains of delivery, which was completed in two hours instead of, as formerly, in ten or twelve. Knew nothing about it, and when restored to consciousness she had no recollection of what was said or done; absence of after-pains, and had a complete recovery on the seventh day.

This and the numerous other cases related of the astounding successes obtained by Dr Wetterstrand are apt to give rise to a feeling that it is too good news to be altogether true, especially among us seniors, who have so frequently experienced in therapeutic applications that—

> "The best laid schemes o' doctors, mice, and men
> Gang aft agley."

Yet there is every external evidence of scientific good faith in the author, who is unmistakably far advanced in professional attainment, and well read in British, French, and German standard authorities. For those who have opportunities and aptitude for applying and experimenting in Braidism or hypnotism we would consider such as a perfectly legitimate *modus operandi*, and whatever benefit might accrue therefrom to suffering humanity would be so much pure gain.

At the Balneologisch Congress, held at Berlin 1st to 3rd March 1889, I see that Von Corval of Baden-Baden stated he had paid a visit last year to Wetterstrand in Stockholm, and had been present at 800 séances. While contending that *suggestie therapie* was not more dangerous than many other agencies in daily use, which had also their disadvantages, as a pain-calmative he averred that he had witnessed surprising results in hysteria, cephalalgia, migraine, and tabes, and more especially among drunkards and stammerers. The speaker strongly recommended the members of the Congress to apply *suggestie therapie*, and prove its claims to confidence in *cases where a cure by other processes appeared unattainable.*

We now proceed to consider the volumes (quarterly) of the *Nordiskt Mediciniskt Arkiv* of Stockholm for 1888, edited by Dr

Axel Key, Professor of Pathological Anatomy—a substantial-looking publication, with papers in Swedish and Danish, and medical notices from Sweden, Denmark, Norway, and Finland. The first paper that merits notice is one by Dr Emil Rode, on the Signs still existing after Involution Period of a previous Pregnancy (Tegn, efter Involutiens-perioden, pö Forudgäet Födsel). This paper is based on the journals of the Maternity of Christiania, on the experience of the author in private practice, and on a certain number of autopsies.

Dr Rode attaches the chief importance to the uterus. Among nullipara the vaginal portion of it forms a small cone, while with those who have borne children this portion is flattened equally before and behind as up and down. Among the sterile Dr Rode found the os ext. uteri was oval in 83 of 168 cases investigated, and circular in 72 per cent. of 449 cases of primipara. The aperture was transverse in 60 per cent. of 20 females examined post-mortem. In those who have been delivered of fully developed children, the os was transversely elongated, and one-half with indentations.

To the total length of the uterus Dr Rode does not attribute an absolute importance, but this is much greater when the length above mentioned is compared with the distance of the orifices. As to this point it was constantly observed that in women who had given birth to children at full time it is always 2·50 to 2·00 cm., a dimension never noticed in nullipara. To the mammæ he attaches little significance, seeing that all the special signs they offer might be found in virgins; only the white spots found in the secondary circle surrounding the areola is only found in females who have been pregnant, and even among them these spots are rare. This laborious and elaborate paper is illustrated with five plates, in which every possible aspect of the os uteri is delineated.

The next is "Om Snöblindhet," af Med. L. Aug. Berlin,—"On Snow-blindness," by Dr Aug. Berlin.

This malady has been hitherto regarded as a lesion of the interior parts of the eye, an irritation of the retina and optic nerve, originating in the intense reflection of the light by snow. Liber regarded it as an exaggerated dazzling. Some admit a relation with hemeralopia and nyctalopia.

Physicians who have studied cases of this kind for themselves have been forced to lay much stress on the symptoms of conjunctival irritation, or, with Reich, they have explained it as a reflex dazzling, or, with Schiess Gemuseus, as a combination of the two.

The author, who himself had occasion to observe this eye trouble during the expedition of Nordenskiöld to Greenland in 1883, has devoted a special study to it, from which he has been led to form opinions directly opposite to those which previously prevailed. After having gone over nearly all the histories of the Arctic voyages, and weighing cases mentioned in ophthalmological literature, Dr

Berlin has collected sufficient material to enable him to determine pretty exactly the geographical extension of the disease. This is very characteristic. Snow-blindness presents itself as far north as the Arctic regions have been hitherto explored, while on the south it never oversteps certain degrees of latitude. On the American and Asiatic continents it extends farther than in Europe. In America it descends to 53° N. lat., whereas it is unknown in Scandinavia. A map which accompanies the original Svensk displays clearly the Arctic territory of snow-blindness, where the meridional limit follows all the sinuosities of the isothermals of this portion of the globe.

To the south of this district the malady occurs sporadically only on the highest mountains, such as the Alps, Caucasus, and also on some equatorial summits. In the Arctic regions it is severest in the spring, but is met sometimes in summer, especially where snow remains on glaciers. The disease is produced not only when the sun appears, but also when tempests and snowstorms occur.

The symptoms are—intense burning pain in the eyes, commencing with a stinging sensation as if a foreign body was present; augmented tear secretion; photophobia and blepherospasm. At first hyperæmia of the bulbar conjunctiva, then of the eyelids; chemosis, and in grave cases dulness and dimness of the cornea. The pupils are contracted, and the fundus oculi slightly hyperæmic; the visual power not diminished, but only contracted by the foregoing symptoms. If the patient be withdrawn from the originating morbid influence, the malady is cured in two or three days; but in some other cases corneal ulcers occur, and even more destructive processes, ending in loss of sight. Excepting these, no diminution of visual power remains as a consequence of snow-blindness. Sometimes only one eye is attacked—the one mostly turned to the side of the sun. Animals also are subject to this affection.

The symptoms, as well as the manner in which they arise, enfeeble the supposition that dazzling is the cause of them. In fact, when dazzling results from a person imprudently gazing at the sun, there is no pain felt nor any sign of irritation of the anterior parts of the eye; but, on the contrary, it produces a persisting diminution of the field of vision, and vertigo, which never appears in snow-blindness. The rarefaction of the air does not give much explanation.

The regions where it prevails are remarkable for their low temperatures and consequent diminution of the absolute humidity of the air. As it is chiefly the atmospheric humidity that absorbs the radiating heat, the calorific rays of the sun must exercise in these localities an intensity of action much superior to their usual intensity. Now, this is the fact. On high mountains and in Arctic regions, insolation produces in the naked skin a dermatitis excessively painful, and the radiating heat reflected by snow redoubles the effect. From direct observation it appears that in these parts insolation attains its maximum in the spring.

It would be more than extraordinary if, under the dominion of such circumstances, the eyes should remain exempted from the pernicious influence of the calorific sun-rays. In fact, the eyes are often scorched simultaneously with the skin or a little before. It is exactly the same process—hyperæmia of the skin and conjunctiva, then serous exudation, followed by the intense smarting characteristic of snow-blindness. The congestion of the retina is easily explicable by the violent irritation of the conjunctiva. The author rejects the theory that the retinal hyperæmia is a first-product of the insolation. He relies, among others, on the experiments of Janssen, showing that the retina is only acted upon by an insignificant proportion of calorific rays.

This erythema conjunctivæ, as Dr Berlin entitles the malady, is equally produced by mechanical irritants, such as the snowy particles that strike the eye during snow-storms, or by the fine, jaggy ice-needles borne in by them.

The author advances the supposition that a milder form of snow-blindness is seen beyond the regions indicated. The conjunctivitis phlyctenulares that manifest themselves at the beginning of spring he also attributes to insolation.

The aborigines of these snowy regions employ, as a preservative against this painful ophthalmia, *snow-spectacles* made out of a piece of wood provided with a transverse cleft, or by a network of wire or of hair, of which a figure is given, also a picture of the spectacles—a metallic frame with coloured glasses and network iron sides—which are used in Arctic voyages and travels to protect the eyes, but which, unluckily, are easily covered by damp or snow on the glasses, hence the author advises their being entirely made of iron network. After the ophthalmia is once established, coloured glasses are useful for the concomitant photophobia.

For the treatment of the malady vinum opii and sulphas-zinci lotions have been hitherto employed, but Dr Berlin recommends for future expeditions the use of cocaine.

Immediately following the communication just noticed is a paper in Danish ("*Om Akute Trofonevroses hos Sindssyge, sarlig den diffuse Flegmone*," af Reserveläge Helweg, Aarhus, Sindessygeanstalt) "On the Acute Trophonevroses of the Insane, and especially the Diffused Phlegmons," by Rv. Helweg, Aarhus Asylum, Jutland. The author has had occasion to notice that diffused phlegmons occur more frequently in lunatic asylums than outside these establishments, and was inclined to infer that some causal relation existed between this malady and the pathological alterations of the nervous system of the insane, or, in other terms, that there were grounds for regarding it as a trophoneurosis. In the first place he founded this view on the frequency of diffused phlegmons among that class of patients. The list of somatic maladies among the insane in the Asylum of Aarhus (Jutland) for five years shows that this malady furnished a percentage of 2·4 of the cases, a figure which must be regarded as considerable. Besides, these phlegmons occur only in the most serious cases of mental diseases, where the most profound alterations of the central nervous system present themselves, and among these they come spontaneously without external causes.

Dr Helweg demonstrates the affinity between these pathological conditions, showing œdema and diffused erythema are introductory to the phlegmon,—on the back leading to dermatic gangrene, and on the sides to anthrax, carbuncle, ecthyma, etc. In support of his view of this affinity he gives details of seven cases, in six of which, followed by a careful anatomic investigation of the spinal cord, different parts of the peripheral nerves, the muscles, etc., displaying grave alterations found in many of the organs without local symptoms to account for them—the patient presenting a sort of general degeneration, with degeneration of the functional tissues of the organs.

In these, besides the chronic myelitis characteristic of insanity of old standing, acuter focal processes in various sections of the spinal cord were found, which the author places in relation with the acute trophoneuroses. Comparative examination showed, as previously pointed out by Charcot, that it is the central portion of the gray substance that is connected with troph. of the skin. He considers that Clark's columns have most to do with it, and the more acute the degeneration there, the more grave and violent the manifestations.

In the two last cases the post-mortem changes exactly corroborated the diagnosis made during life. The last case was of special interest. The patient of 48 showed a complete clinical picture of a subacute anterior cervical polyo-myelitis. After manifesting only slight symptoms, a sudden exacerbation occurred at the end of fifteen days, there being radiating paralysis of the two superior extremities with paresis of the second group of their muscles. In two or three days he developed simultaneously a pneumonia on the right, an acute intestinal catarrh, acute arthritis of the left knee, and a diffuse phlegmon of the left femur and leg. The paresis disappeared more or less in the course of some weeks along with the intestinal catarrh, the pneumonia became indurated, and the arthritis acquired the aspect of white swelling. As regards the diffused phlegmon, he suppurated a whole year. Then gangrene developed itself in the parietes of the femoral abscess cavities, passing on to the popliteal artery, causing fatal arterial hæmorrhage. The autopsy discovered a sclerosis of the central part of the gray substance in the inferior portion of the cervical spinal marrow and the superior portion of the dorsal, extending to the anterior gray cornua—the ganglion-cells of which were deeply affected—and posteriorly to Clark's columns. It was especially the *left side* that presented the pathological alterations.

V.—DESCRIPTION OF OPERATION TO REACH THE GUSTATORY AND INFERIOR DENTAL NERVES WITHOUT OPENING INTO THE MOUTH: WITH CASE ILLUSTRATIVE OF ITS VALUE.

By DAVID WALLACE, F.R.C.S. Ed.

THE three operations—nerve stretching, nerve division, and the removal of a portion of a nerve trunk—have, during the last few years, been more and more frequently used as a means of cure or alleviation of pain in cases of persistent neuralgia which have not been amenable to other forms of treatment. Thus, in sciatica, nerve stretching is now frequently had recourse to when other means have failed to remedy the condition; while in tic douloureux division of the trunk or branches of the fifth nerve is not at all uncommon.

The third division of the fifth or trifacial nerve consists of the three branches, the auriculo-temporal, gustatory, and inferior dental. Division of the gustatory may be necessary in severe neuralgia of the tongue, as in epitheliomatous ulcer of that organ, while in persistent neuralgia of the lower jaw the inferior dental may be divided with great relief to the patient.

Hilton first suggested that the gustatory nerve should be divided to relieve neuralgia of the tongue in a case of epitheliomatous ulcer, and he performed the operation from the mouth, dividing the nerve as it lies on the floor of the mouth just under the mucous membrane. Later, Dr Moore divided the nerve where it crosses the ascending ramus of the lower jaw after leaving the internal pterygoid muscle, a point at which he thought it more accessible than in the floor of the mouth. Vanzetti performed the operation in the same way.

The inferior dental nerve has frequently been divided from the inner aspect of the mouth, the point of division being opposite the last molar tooth. The operation is not a difficult one, but it would appear that hæmorrhage is apt to occur, and also that the patient may suffer for a time from pain and stiffness of the parts. Thus in Jacobson's *Surgery* cases are quoted in which Mr Pitt and Dr Weir were the operators. The division in the cases quoted was necessitated by severe neuralgia. In Mr Pitt's two cases simple division of the nerve was alone adopted, yet in one there was severe hæmorrhage, the inferior dental artery having been divided, and " in both cases the patients suffered from swelling of the throat and neck, as might be expected from the necessary bruising of the soft parts. Several months elapsed before complete mobility of the jaw was regained." In Dr Weir's case a portion of the nerve was removed, but there being a doubt as to whether it was nerve or not, " a blunt hook was introduced into the wound, and a cord, which appeared to be the nerve, divided. Profuse hæmorrhage took place, and could only be controlled by the packing of gauze into the wound."

Division of the inferior dental and gustatory nerves from the inner aspect of the mouth seems open to various objections. *First*, Frequently patients suffering from epithelioma of the tongue are unable to open the mouth to an extent suitable for the operation to be performed. *Second*, In cases of epithelioma the mouth is often very foul and difficult to purify, thereby rendering any wound in the mouth liable to septic action. *Third*, If division of one nerve only be necessary, the proximity of the inferior dental and gustatory nerves renders it difficult to distinguish the one from the other. But, even supposing these objections to be absent when more than mere division of the nerves is proposed, *i.e.*, removal of a portion, there is not very much room for the performance of the operation, and hæmorrhage, as in the two cases quoted above, is apt to occur and difficult to control.

Division of the inferior dental nerve from the outer aspect has been proposed and carried out by various surgeons. The methods used vary in their detail, but in all an attempt is made to reach the nerve before it enters the inferior dental foramen. Velpeau reached the nerve from an opening made through the ascending ramus of the lower jaw. He made a curved incision, with the convexity downwards, about two inches long, which passed through the insertion of the masseter muscle down to the bone. The bone having been exposed, he raised the soft structures along with the periosteum, so that the central part of the ascending ramus was laid bare. The detached soft parts were drawn upwards, so that the facial nerve and Stenson's duct escaped injury. The bone over the opening of the inferior dental canal was then removed with a trephine, and the nerve exposed. Sir William MacCormac (*Operative Surgery*, part ii.) figures a method by which the nerve may be exposed by cutting out a U or V-shaped piece of the ascending ramus of the inferior maxilla, the base of the wedge being at the coronoid notch, and the apex a little below the dental opening.

Lücke has lately introduced a method in which he exposes the nerve by passing inwards and upwards under the angle of the ascending ramus of the jaw.

James O., æt. 55 years, was admitted into the Royal Infirmary under the care of Professor Chiene in October 1887. He suffered from an epitheliomatous ulcer of the tongue with glandular infection; the extent of the disease precluded any chance of complete removal, and he was therefore told that operative interference was not justifiable. The patient, however, complained of such severe neuralgic pain in the tongue and lower jaw on the right side, which could not be alleviated by the administration of drugs, that Professor Chiene, after a fair trial of various local and general sedatives, resolved to resect a portion of the right inferior dental and gustatory branches of the fifth nerve. The condition of the mouth being very foul, he determined to resect the nerves from the outer side by removing a portion of the ascending ramus of the lower jaw, and the following operation was performed with complete success. The patient having been anæsthetized, a curved incision was made over the ascending ramus of the inferior maxilla. The incision passed through all the tissues down to the bone, the lowest part corresponding to the angle of the jaw, while the upper ends did not pass so high as the point where the facial nerve crosses the bone. The attachment of the masseter was cut through, and all the soft structures mapped out by the incision were turned upwards to a quarter of an inch or thereby above the level of the last molar tooth. In this way Stenson's duct, the parotid gland, and the facial nerves were uninjured. A trephine half an inch in diameter was then placed on the bone with its central pin almost midway ($\frac{1}{8}$ of an inch nearer anterior) between the anterior and posterior margins of the ramus at the level of the crown of the last molar tooth, and a circular piece of bone was removed. The periosteum having been cut through, the two nerves were exposed and readily distinguished from each other by position, the gustatory lying more anteriorly and more internally than the inferior dental; the end of a probe was bent to a suitable curve, the nerves hooked up, and half an inch removed from each. The flap was turned down and stitched in position. The wound rapidly healed, the patient being able to leave the hospital on the eighth day after the operation. The wound remained aseptic throughout.

The method of procedure adopted in this case was practically that of Velpeau, but the piece of bone was removed having regard to very definite landmarks. I have now performed the operation on the dead subject, using the same landmarks on three different bodies, and in each case I found the nerves with the greatest ease.

I have thought it may be of interest to describe the above operation illustrated by a case, more especially because most of the recent text-books in surgery do not refer to the external operation for the division of the nerves in question.

VI.—HYPNOTISM, OR PSYCHO-THERAPEUTICS.[1]

By R. W. FELKIN, M.D., etc., Lecturer on Diseases of the Tropics and Climatology, Edinburgh.

TWO small books on the subject of hypnotism, suggestive therapeutics or animal magnetism, having been sent us for review, we will take the opportunity thus presented to us to refer at some length to a subject which, although tabooed in this country, has been exciting great interest in professional circles on the Continent. We do this all the more readily, because of our firm belief that the subject deserves more attention than has yet been paid to it by British medical men and scientists. Be this, however, as it may, whether there be anything or nothing in hypnotism, it will be certainly well to examine the question, and to indicate what is known and thought on the subject on the Continent, so that the constant notices which it receives in the literature of the day may be read intelligently and appreciated, not passed over with an incredulous shrug of the shoulders and a contemptuous smile.

---

[1] *Psycho-Therapeutics, or Treatment by Sleep and Suggestion.* By C. Lloyd Tuckey, M.D. London: Baillière, Tindall, & Cox, 1889. Pp. 80.

Hypnotism, or Animal Magnetism. Physiological Observations by Rudolf Heidenhain, M.D., Professor of Physiology in the University of Breslau. Translated from the fourth German edition by L. C. Woolridge, M.D., D.Sc. With a Preface by G. J. Romanes, M.A., F.R.S. Second edition. London: Kegan, Paul, Trench, & Co., 1888. Pp. 103.

We have taken considerable pains to make ourselves acquainted with the literature of the subject, and, whilst not professing to have read it all, we shall, at the conclusion of this article, give a bibliography of some of the principal contributions to it, indicating the books and articles with which we are personally acquainted. Probably no one will deny that great strides have been made on the Continent in investigating the various phenomena of hypnotism, in suggesting theories as to its production and action, in experimenting as to its therapeutic uses, and in trying to establish it upon a firm scientific basis, thus raising it from the low level of arrant quackery to the higher plane of scientific accuracy. When one first commences the study of the subject as developed at the present time, one is struck by the immense amount of work which has been accomplished during the past ten years, and also by the fact that, with but few exceptions, British observers, instead of being in the forefront of the investigation, are so conservative that they have held aloof from it, and left to others the burden of the research and the possible obloquy resulting therefrom. We are not without hope, however, that the time will soon arrive when they will cast aside their prejudices and examine for themselves facts which, unless we are very much mistaken, may be justly utilized in the alleviation of suffering and the cure of disease, not in the universal sense claimed by some extreme enthusiasts, but to a limited and particulate extent.

We think we shall be able to show that the uses of hypnotism are fairly well defined, that its possible abuses are definitely indicated, and that it deserves considerable investigation before we are justified in relegating it to the domain of the unpractical.

We are anxious to confine our remarks to the present condition of the subject rather than to the past. It would be interesting, indeed, to trace the rise of hypnotism from the time when it was known in India 2000 years ago, and practised by the religious fanatics, the so-called Gogins, or when it was employed by the Hebrews and Assyrians in the cure of disease. We may note in passing that in the British Museum there is a bas-relief taken from an ancient tomb in Thebes, showing a hypnotist and his patient. We might relate how hypnotism was practised by Mahommedan sheiks in producing those states of exalted brain function which have given rise to so much of their literature, if not to their religion. One could also very properly speak of the old Greek monks, who, by gazing at their navels, induced the ecstatic trances for which they were once so celebrated. And it would be still more interesting to refer to hypnotism and suggestion as known and practised by uncivilized races, but Dr Otto Stoll is about to publish a work on the rôle which suggestion and hypnotism play in the psychology of nations, and we will not anticipate him. Hypnotism could also be traced through the ages up to the time when it was investigated by Braid in 1843, Elliotson in 1838, Esdaile in 1846, and Mitchell, U.S.A., in the same year. To refer in detail to all this, however, would lead us from our present purpose, and would involve an analysis of the true and the false, which, however interesting, would serve no practical purpose, nor would it be suited for a purely medical journal.

We must, however, allude for a moment to the apparent dread of investigating hypnotic phenomena shown by British scientists. It is to be attributed to the unfortunate impostures which were practised between the years of 1840 and 1850. Men naturally shrink from laying themselves open to the charge of dabbling in a once thoroughly discredited procedure. We are, however, inclined to think that the discovery of chloroform and ether as anæsthetics did more than anything else to prevent the adoption of hypnotism as an anæsthetic agent. It is impossible to believe that the results of, say, Esdaile's work, would have been allowed to fall into obscurity had not agents been discovered, the anæsthetic action of which was produced by physiological action, which could be more easily understood, and the operation of which gave no possible handle to the charge of possible fraud or necromancy. Now, however right and proper it may have been to avoid the chance of such charges in the past, it must be admitted that now-a-days all such fear is, to say the least, selfish and cowardly. Men whose scientific reputation is beyond all question have investigated the subject on the Continent. They have shown us over and over again that beneficial results do accrue from the use of hypnotism, and we are therefore not justified in refusing to study the matter for ourselves, and to see whether its therapeutic uses are based upon truth or error. We are quite willing to admit that any one engaging in this work must be ready to meet with opposition and criticism, but such is the case in all epoch-making discoveries or researches. We need only recall the opposition to Jenner and the vaccination he introduced, or, more recently still, the outcry against Pasteur in his endeavours to stamp out diseases, such as anthrax and hydrophobia, by inoculation, or the researches of Koch with regard to cholera. When Lister introduced antiseptics he was decried, and yet what benefits they have conferred upon suffering humanity! Ovariotomy was once thought to be an operation perfectly unjustifiable, yet study, experiment, and practice have raised it to such perfection that no one will be found to condemn it now; but Sir Spencer Wells and Dr Keith had many a hard battle to fight with prejudice and ignorance before they attained their present position. Electricity was once, and is still to a large extent, in the hands of self-styled professors, and yet it has a great future before it in connexion with medicine, and few will now deny the benefits to be derived from its scientific use in suitable cases?

And, to come nearer home, the profession in this city, at least, should not forget how Simpson and his chloroform were decried. The religious world was in arms at his audacity in trying to relieve the pain which was believed to be in accordance with the Divine intention. So it will ever be, and those who take up the investigation of this subject may make up their minds at the outset for many a *mauvais quatre d'heure.* This, however, notwithstanding, we sincerely believe it to be the duty of professional and scientific men to face facts, and to lend their learning, experience, and scientific training to the investigation of this subject, by which means they will wrest it out of the hands of charlatans and quacks, and place it upon no uncertain footing.

Statistics are always dry. We will, therefore, compress as far as possible the information we possess in regard to the literature which has been written during recent years upon the therapeutic action of hypnotism. We find that from 1860 to 1888 more than 178 papers and books were published in France upon this subject. In Belgium and Holland, between 1885 and 1888, 18 publications appeared; in Italy, between 1860 and 1888, 49; in Spain, between 1860 and 1888, 16. In England and America, between 1843 to 1887, 39 were published, most of them making their appearance between 1843 and 1853. During the last three years we believe that 4 papers have been read on hypnotism in Great Britain, 1 lecture delivered on the subject in Aberdeen, and about 15 references made to it in the current medical literature of the day. We shall, however, subsequently refer to this. In Greece, Poland, and Russia, between 1884 and 1888, 11 papers appeared; in Denmark and Scandinavia, in 1888, 23 papers were published; and in Switzerland, 13 between 1880 and 1888. In Austria and Germany, between 1886 and 1888, no less than 118 books and papers were published. We thus find that a total number of 476 books and papers (many of the papers extending to three or four numbers of various magazines) have been published, and the vast majority of this literature has appeared on the Continent, for the information published in the English language can hardly be considered as either original or more than a drop in the bucket, if we except the writings of Braid of thirty years ago.

We think that we are entitled to take it for granted that this accumulation in literature could not be brought about owing to the simple phantasy of misguided men, and we have therefore a *prima facie* case for justifying us in expecting for this subject earnest attention and investigation. But more than this, so convinced are various Governments of the reality of hypnotism, that they have forbidden all public exhibitions of its phenomena, and have limited its use to the medical faculty. Holland, Portugal, Italy, and Switzerland have all taken this action.

Although the profession at large has, we think, unjustifiably held aloof from the study of hypnotism, it must not be thought that no intimations of its possible value are to be found in our text-books, and to drive this fact home a few quotations may be allowed. Sir Thomas Watson said,—"Do not suppose . . . that I ignore the glaring *facts* of mesmerism. I have seen and scrutinized too many of them to doubt their genuineness, or to ascribe them to mere imposture or collusion. You may desire to know, and you have a right to know, my creed upon this vexed and much abused subject. It is this. Of the brain and nerves there are many and various strange conditions which we reckon as manifestations of disease or disorder. . . . I might have described to you the curious condition or alternating conditions of double consciousness and the familiar phenomenon of sleep-walking. Now, whatever condition of this kind may arise thus spontaneously may also, I believe, be produced in some persons under the mesmeric proceedings. The phenomena are subjective phenomena; the determining influence is from within as much as, or even more than, from without, . . . not, however, through any material or occult influence emanating from the mesmeriser, but subjectively from the mental attitude in which the person mesmerised is led to place himself. . . . I believe, too, that sleep may sometimes be conciliated by the monotonous biddings of mesmerism where drugs might fail to procure it, and that such sleep may become sometimes and in certain diseases a mode and instrument of cure.[1]

In Tanner's *Practice of Medicine* it is stated that "a cateleptic state may be induced by strongly fixing the attention on one object for a short time. The mental faculties get tired, there is diminished nervous influence or force, and persons so affected then believe that they are unable to move, cannot see, etc., until the so-called mesmeriser grants them permission." And again, "there can be no doubt that the manœuvres employed by mesmerists will induce sleep when practised on certain susceptible persons. In properly selected cases there is no objection to the physician resorting, therefore, to this remedy."[2]

In Quain's *Dictionary of Medicine* H. Charlton Bastian writes,—"The scientific study of the phenomena presented by hypnotised persons is unquestionably of great interest and importance from the point of view of the higher cerebral psychology." Again, "the whole subject is one of great interest for the practitioner of medicine, now that the absurd theories have been got rid of. We must be careful, however, to pursue the study of the condition itself in a strictly scientific manner, and watch lest the too ready adoption of hypnosis as a curative agent may do harm rather than good, and that not to the patient only, but also to the practitioner. The statements of the results obtained by Braid are little less than marvellous; and there can be no doubt that the therapeutic uses of hypnotism ought to receive a new and thorough investigation by some instructed and well-trained observers. The pitfalls besetting such an investigator are by no means few; but, on the other hand, the gains to the science of medicine and to therapeutics might be great."[1] And again he writes,—"Braidism certainly deserves more attention than it has received. . . . . If only half the results which have been attributed to it would follow the systematic adoption of this method for the alleviation of many diseases, it is one which should commend itself to the earnest attention of future inquirers, who may be able to place the practice upon a broader and firmer foundation than that on which it now rests."[2]

---

[1] *Lectures*, 4th edition, 1875, vol. i. p. 717.

[2] Vol. i., 7th edition, pp. 505 and 523 (1875).

With the opinions expressed in the above quotations we agree. At the same time, we can quite appreciate the reluctance to investigate the phenomena of hypnotism, which undoubtedly obtains in this country. It is certainly due to the remembrance of the unfortunate experiences of Braid, of Gregory, and of other observers in the forties; and also to the failure of travelling charlatans who practise hypnotism to demonstrate its phenomena to the satisfaction of the scientific mind. This surely is not to be wondered at, and we can hardly understand scientific men expressing an opinion on exhibitions where trickery is perfectly allowable, although the exhibitions are sometimes distressing and undesirable. An example of this kind of inquiry may be given. At the Medical Congress held in London in 1881, a Dr Beard from New York invited a number of scientists and medical men to witness some experiments in hypnotism and clairvoyance. It appears that he failed in his experiments, and his failure gave rise to a correspondence in the *Times* and *British Medical Journal*. Naturally the scientists were disappointed, and we mention this case because, in the first place, it shows that when hypnotic phenomena are witnessed in a scientific spirit by competent observers, fraud, or even unconscious deception, are not likely to pass muster; and, secondly, because there are a few sentences in the letters written to the *British Medical Journal* which we quote, in order to show the view which the writers take of hypnotism and its manifestations. For instance, J. Mortimer Granville writes,—"I have no more or other faith in the phenomena of hypnotism than all physiological psychologists possess, and that is, as nearly as possible, *nil* . . . . . In common, probably, with all who have studied mental disease and disorders, I believe there are certain neuroses in which minds, not so constituted that their faculties converge and may be focussed, will, under certain conditions or influences, fall into a state of disorganization akin to the polarization of light, and when this state occurs and while it lasts, there is likely to be a disruption of the normal relations between the centres, or congeries of centres, which compose the nervous system. Taking advantage of this disorder, an expert operator may succeed in establishing special communication between his mind and the revolving faculties of his subject. Of course the communication thus established must be physical and actual. There is nothing mysterious or out of the ordinary course of things in this. The whole phenomena of mind, without exception, are, I believe, purely physical in their mode of causation."[1]

---

[1] Quain's *Dictionary of Medicine*, vol. ii. p. 976.

[2] *Ibid.*, vol. i. p. 132.

Dr H. Donkin, referring to the same séance, after giving an account of its failure, says,—" I think that to some degree, in the interests of science, and more especially in that of practical medicine, the alleged cures of the kind described above should be investigated. That a psychical condition, which may be called by the name of hypnotism or artificial somnambulism, exists will probably not be denied. Its counterfeit is so easily produced, so many of its phenomena being entirely subjective, and offering such a fertile field for imposture, that physiologists and physicians cannot be too jealous in sifting every alleged case that comes before them. The majority of alleged cases, which are probably wholly impostures, requires the supervision of the police more than that of the physician; the minority, which are partly or wholly genuine, are more or less pitiable cases of disease, the 'treatment' (or prolongation) of which will probably lead to the ultimate damage or incurability of the patient." We shall see subsequently how far this last statement is justifiable.[2]

Crichton Browne also published a long letter concerning the séance, giving in great detail a description of it, but we will only quote a paragraph which apparently gives his opinion of hypnotism. He says, " All experts are agreed that certain curious phenomena, which have been designated hypnotism, are witnessed under exceptional circumstances in certain animals and human beings; but most experts are equally well agreed that much that has passed as genuine hypnotism has been rank imposture. No practical physician with a knowledge of nervous disease can read the literature of hypnotism, or critically watch a series of hypnotic exhibitions, without arriving at this conclusion. The difficulty is to eliminate the imposture, and determine what remains. The process of elimination must, I presume, be carried on by the ordinary methods by which error and falsehood are disengaged from truth. Testimony must be sifted, evidence weighed; and hence the paramount importance of determining the credibility of persons who manifest hypnotic phenomena, a large majority of which may be successfully simulated."

---

[1] *British Medical Journal*, 1881, vol. ii. p. 305.

[2] *Ibid.*, 1881, vol. ii. p. 306.

As a contrast to the above-mentioned exhibition of failure, we may refer for a moment to a demonstration of the phenomena of hypnotism given before the Medical Society of Manchester in 1885. Dr Walter Whitehead, referring to it, makes the following remarks:—"I can affirm that of every one I have since met who witnessed the demonstration—and I have discussed the matter with at least a score of our leading professors, teachers, and medical men—not one expressed a hint of disapproval at what took place, or stated that they disbelieved the genuineness of what they had seen . . . . Six weeks ago I was practically ignorant and wholly sceptical of the merits of mesmerism; now I am convinced that it is a subject of deep psychological interest, but devoid of any practical application in the domain of surgery, and I strongly deprecate its being made the means of pecuniary gain and public amusement. These convictions I believe I share in common with the major part of those who have had frequent opportunities of investigating the subject during the last few weeks in Manchester."[1]

Let us now very briefly refer to the attention paid to the subject of hypnotism in this country, and it is probable that the little attention which it has received is due to, or perhaps we should say took its start from, a paper published by Dr Arthur Gamgee in 1878, in which he gave an account of a demonstration on the phenomena of hystero-epilepsy given by Professor Charcot at the Salpetrière. At this demonstration were present, amongst others, the following physicians and scientific men:—Professors Virchow, Grainger Stewart, Turner, Oscar Liebreich, Ray Lancaster, Dr Broadbent, Mr Ernest Hart, etc. Professor Charcot demonstrated the remarkable influence of solenoids and magnets upon the diseased organisms, as well as the ready induction of hypnotic and cataleptic conditions in various patients. It is unnecessary to describe just now the remarkable effects which were produced, but they appear to have been greatly appreciated by the gentlemen present, and we cannot imagine that they were likely to have been misled in regard to the facts they saw, either by the distinguished demonstrator, or by the patients he submitted to their observation. Professor Gamgee, in a few remarks at the end of his paper, says:—"The reader who is acquainted with physics and physiology will at once admit that many of the facts narrated are absolutely inexplicable in the present condition of physiology," and he goes on to mention that Claud Bernard, shortly before his death, told

---

[1] *British Medical Journal*, 1885, vol. ii. p. 721.

Charcot, after having seen the same phenomena, that "the facts show how little we *do* know."

Since this time there have been a number of short notices with regard to the subject in our current medical literature, consisting chiefly of a few lines giving quotations from Continental sources, but we may search in vain for any real general scientific interest exhibited with regard to the subject. In November 1880, Professor Stirling gave a lecture on hypnotism in Aberdeen, in which he expressed the opinion that Braid's facts had been rediscovered in Germany by Weinhold and Haidenhain, and that the subject was one which must shortly occupy the attention of the physician and physiologist, as well as the psychologist.[1]

At a meeting of the Brighton and Sussex Medico-Chirurgical Society on 3rd February 1887, Dr Gasquit read a paper on hypnotism. He thought it was due to various degrees of removal of inhibition, that its curative value was very doubtful, and that the social and moral dangers connected with it were enormous.[2]

The Paris correspondent of the Lancet, referring to a communication made by M. Luys to the Academy of Medicine of Paris, in 1887, in regard to his researches concerning "Solicitation expérimental des phénomènes des motifs chez les sujets en état d'hypnotisme," says—"It is regretable that the 'facts' stated at the last meeting should have been brought forward by one who has hitherto enjoyed the reputation of a savant on cerebral physiology, and whose work on the brain is a standard text-book, but M. Luys will have done a service in forcing the Academy to an expression of opinion concerning the practices carried on in many of the Paris hospitals, where hypnotism, daily misapplied under a therapeutic pretext, has nearly constantly only one effect—that of destroying the mental and moral equilibrium of those previously sound in this respect, and of inducing many patients into the gravest degradation of hysteria." As will be subsequently seen, this correspondent's view is hardly borne out by facts.[3]

We next find a leading article on hypnotism in the *Provincial Medical Journal* (1887, p. 559), contrasting the methods of M. Luys with those formerly made by Dr Esdaile in India. The writer concludes by saying, "We fail to see the medical advantages of hypnotism. If a patient wants a piece of steel extracted without pain, ether and chloroform are at hand, and the thing is done. Public exhibitions of this kind have an injurious effect, and in some countries they have been forbidden by law."

At the Medical Institute in Liverpool on 6th December 1888, Dr Karl Grossman read a paper on hypnotism, and illustrated his remarks by means of hypnotised individuals.[4]

---

[1] *British Medical Journal*, 1880, vol. ii. p. 797.

[2] *Ibid.*, 1887, vol. i. p. 512.

[3] *Lancet*, 1887, vol. ii. p. 547.

[4] *British Medical Journal*, vol. ii. p. 1413.

At a meeting of the British Medical Association in August 1880, Preyer of Jena read a paper on sleep and hypnotism, and Dr G. M. Foy read another on hypnotism at the Royal Academy of Medicine in Ireland, on April 6th, 1888. We shall have to refer to those two papers later on, and here we have reached the end of our list. It must be admitted that it is not extensive, nor does it show that the importance of the subject has been recognised.

(To be continued.)

Part Second.

REVIEWS.

The Natural History of Cow-pox and Vaccinal Syphilis. By CHARLES CREIGHTON, M.D. London: Cassell & Company: 1887.

"Vaccination." Article in the *Encyclopædia Britannica*, Ninth Edition, Vol. XXIV. By the same Author.

Jenner and Vaccination: A Strange Chapter of Medical History. By the same Author. London: Swan Sonnenschein & Co.: 1889.

(*Continued from page* 149.)

IN connexion with the origin of the cow-pox tradition as it existed in Ireland, I should have pointed out in last month's *Journal* that not only was there no resemblance between the words *shinagh* and small-pox, but that the native term for small-pox, *Galra breac,*[1] was also incapable of suggesting any verbal relationship between the two maladies.

Before passing from the evidence already adduced that a belief in the anti-variolous power of cow-pox was entertained by a number of medical men in the south-western counties of England, I wish to point out that Dr Creighton's lack of knowledge of the facts has led him astray, both in his reasonings on the subject and in his charges against Jenner. Having reached the curiously mistaken conclusion that Jenner's professional brethren were entirely opposed to his opinion, the doctor goes on to urge that this could not be otherwise. "We can hardly have a stronger or better founded conviction than they had that, whatever the similarity of names depended on, the diseases themselves were totally unlike" (p. 26, *Jenner and Vaccination*), and "a fancy of that kind could not exist along with real or even empirical knowledge of the two diseases" (p. 34). How, then, did Jenner think differently? Because "his knowledge of the affection does not appear to have been more than an acquaintance with the name and the common talk" (p. 25). "It was just because Jenner had no profound sense of these empirical realities that he went blundering into visionary nonsense in the first instance, and at length into systematic mystification and chicane" (p. 26). Further, "it was reserved for Jenner to take up that surprising legend and make it scientifically passable, despite the impatience and ridicule which his prosaic medical neighbours in the cow-pox districts had met it with. It is difficult to acquit Jenner of recklessness or of culpable laxity, even in the very inception of his idea" that cow-pox "was an amulet or charm against small-pox" (pp. 33, 34). Indeed, it would be wrong to accuse Dr Creighton of making any serious attempt at the acquittal. On the contrary, the reader soon finds, with some amazement, that Jenner was guilty, not of ignorance, but of knowledge. At the time when "he rushed off to London to publish his *Inquiry*" he "was well aware that there were quite as many instances telling against protection as there were in favour of that popular fancy" (p. 76). Not only so, but "Jenner knew these differences between cow-pox and small-pox well enough,—indeed, he knew of far more striking differences, only he took care not to dwell on them" (p. 115). How and when Jenner, "lazy and unmethodical," acquired all this exact knowledge does not distinctly appear, but the assertion that he did know was clearly necessary to the proper setting forth of the charge regarding his comparative want of "honesty and candour."

---

[1] Reynold's *Medicine*, 2nd ed. vol. i. p. 223.

In the light of the facts that many of Jenner's "prosaic medical neighbours" *did* believe that cow-pox prevents small-pox, and that they were "well aware" that there were few instances, real or apparent, to the contrary, and that the man who, according to our author, knew most about cow-pox, had implicit faith in its prophylactic power,—all this assumption that it was only Jenner, "loose thinking and imaginative," who held or could hold such an opinion, seems very ridiculous. Fortunately for succeeding generations, he differed from his neighbours in not stopping at the mere opinion.

(3.) We come now to the Latin name *Variolæ vaccinæ*, or small-pox of the cow, given by Jenner as an alternative to the English *cow-pox.* Dr Creighton waxes very wroth over the introduction of the new term. It is not merely that he believes it to be scientifically inapplicable to a disease whose relationships were not with *variola* but with syphilis. It was a "subterfuge" (p. 45), "the unblushing invention of a misleading name" (p. 76), an "artifice" (p. 81), a "foisted name" (p. 97), a "secret artifice" (p. 118), "disingenuous" (p. 239), and so on. The influence of the invention was remarkable. "The name of variolæ vaccinæ was accepted as proof enough that cow-pox *was* a sort of small-pox of the cow" (p. 73). "The invention of the new name was artfully concealed, and was never found out; and under the influence of the plausible idea which the new name covered, the evidence of protection was accepted" on unusually easy terms (p. 124). Indeed, in this connexion Jenner was an embodiment of cunning. For, "having found that the name on his title-page was adopted without suspicion, Jenner used it ostentatiously in the text of his second essay, although it is not used at all in the text of the first" (p. 79).

I am not about to enter into a pathological discussion on the correctness or otherwise of the name variolæ vaccinæ; and that the medical profession, or any other body of sane men, was so very easily led away, hardly needs disproof. But, as a matter of fact, it was not found necessary to the acceptance of vaccination as a preventive of small-pox that cow-pox should be believed to be small-pox of the cow, for some of the most enthusiastic advocates of the practice held no such view. Dr Creighton, indeed, mentions one such—Dr Pearson, already referred to. But, in doing so, he minimizes the nature of the objection, which, he says, "was of the mild, grammatical kind . . . . a catachresis of speech, as if one were to speak of the plumage of a bear" (p. 80). Pearson went much further. In one place he says, "We must distinguish the poisons as of distinctly different species;"[1] while the passage referred to by Dr Creighton is as follows:—"Now, as the cow-pox is a specifically different distemper from the small-pox in essential particulars, namely, in the nature of its morbific poison and in its symptoms,—although the cow-pox may render the constitution not susceptible of the small-pox,—it is a palpable *catachresis* to designate what is called the cow-pox by the denomination *variolæ vaccinæ*, for that is to say in English, *cow-small-pox*, and yet the cow is insusceptible of infection by the variolous poison." Dr Huggan, another supporter of vaccination, says, "It has been asserted, and that without the least shadow of proof, that the cow-pox is only the small-pox having undergone a certain modification by passing through the quadruped."[2] There was no more enthusiastic vaccinator than John Ring. Dr Creighton calls him "the most active of the cow-pox propagandists" (p. 191); but his activity had a different mainspring, for he insists that "it is certain that the cow can neither receive nor communicate the small-pox."[3] And finally, Odier of Geneva, described by Dr Creighton as "the great promoter of vaccination in Switzerland" (p. 262), invented the name "*la vaccine* or *vaccina*, rejecting as absurd the name of the English 'variolæ vaccinæ.'"[4]

Regarding Dr Creighton's charge against Jenner, that having quietly "foisted" the name on the profession, he ostentatiously paraded it in his second essay, I find the facts hardly as stated. As Dr Creighton mentions, the term "variolæ vaccinæ" occurs in the short title of Jenner's *Inquiry*. In his second paper he refers eight times to the *Inquiry*, and on seven of them he speaks of it as his "essay" or "treatise" on the variolæ vaccinæ. In addition, he mentions the words only once, while he uses the term cow-pox sixty times. But besides, we get a little insight here as to how deeply the much abused term had engraven itself on and influenced the medical mind. The paper contains a number of letters from doctors, and they make eighteen references to the bovine disease. On seventeen occasions they speak of it as cow-pox, and once only as variolæ vaccinæ. And eight months after the publication of his *Further Observations* Jenner issued his third paper, *A Continuation of Facts and Observations*, in which the term in question occurs only once, and that in a letter from a correspondent. What becomes here of the amazing craft and anxiety which Jenner displayed in forcing the title into use? .

---

[1] *Medical and Physical Journal*, vol. iii. p. 100.

[2] *Ibid.*, p. 244.

[3] *Ibid.*, vol. vi. p. 487.

[4] *Ibid.*, vol. iii. p. 100.

(4.) *The Variolous Test.*—One of the most difficult tasks which Dr Creighton sets himself, but one which it was absolutely necessary for him to face, is the explaining away of the numerous small-pox inoculations by which the early vaccinators tested the protection afforded by the cow-pox. That he fails in his attempt is not to his discredit, for it is an attempt in which no man could have succeeded. At the same time, the theory which he sets up is interesting, both in its originality and its ingenuity.

When inoculation was first introduced from the East by Lady Mary Wortley Montagu, the disease produced was in a certain proportion of cases so severe, and even fatal, that the practice fell into desuetude for twenty or thirty years. After its gradual resumption, a change in the method of performance came into vogue, and was accompanied by lessened risk and mortality. A change was badly needed. Here is the method as practised by James Burges, who wrote in 1754:—

> "Let a slight incision, of about an inch long, be made on each arm through the cuticle into the skin, but not through it so as to wound the cellular membrane; let a thread saturated with variolous matter be laid along the whole length of the wound, and covered with a pledget of digestive ointment, fastening it on with an adhesive plaster, and binding it on with a thin linen roller. Let this dressing continue on two days."[1]

But Sutton and Dimsdale (afterwards Baron Dimsdale) introduced a milder method.

The English practice, as Woodville tells us in 1796,[2] was Dimsdale's, whose instructions, published in 1766, had "almost, without exception, deservedly continued ever since to regulate the practice of inoculation." Dimsdale's method was as follows:—

> "The patient to be infected being in the same house, and, if no objection is made to it, in the same room, with one who has the disease, a little of the variolous matter is taken from the place of insertion, if the subject is under inoculation; or a pustule, if in the natural way, on the point of a lancet, so that both sides of the point are moistened. With this lancet an incision is made in that part of the arm where issues are usually placed, deep enough to pass through the scarf skin, and just to touch the skin itself, and in length as

---

[1] Woodville's *History of the Inoculation for the Small-pox in Great Britain*. London, 1796.

[2] *Ibid.*, p. 375.

> short as possible, not more than one-eighth of an inch. The little wound being then stretched open between the finger and thumb of the operator, the incision is moistened with the matter, by gently touching it with the flat side of the infected lancet. This operation is generally performed in both arms, and sometimes in two places in one arm, a little distance from each other. For, as I have not observed any inconvenience from two or three incisions, I seldom trust to one. . . . . Neither plaster, bandage, nor covering is applied, or in any respect necessary."[1]

This brings us to Dr Creighton's contention. He holds the new method to have been a sham. But it was this method that was advised by Jenner in testing his cow-poxed cases. Therefore the test was a sham. Not only so, but the whole medical profession, all over the world, agreed to deceive itself and the public in a quite unheard of fashion. Up till Jenner's introduction of vaccination, a single pustule, or even a little redness of the skin, had been held sufficient evidence of success. When vaccination cases were tested, the pustule was produced as often as before, or nearly as often. But now, acting with one mind, medical men turned round, and said that the single pustule and the slight inflammation were *no* evidence of variolation: that, indeed, they were the very opposite, and that unless an eruption of small-pox resulted, with the accompanying fever, it could not be said in any case that vaccination had failed.

It is, of course, to be recollected that vaccination did not at once supplant small-pox inoculation. Many doctors practised both. What, then, are we asked by Dr Creighton to believe? This: That medical men went on inoculating in the same manner as before, that they inoculated the vaccinated and the unvaccinated alike, and that in both classes alike they obtained the same average result as before—this being in a great many cases a local pustule, with or without a few additional pimples. Supposing, now, that a practitioner had with this result practised variolous inoculation on 100 persons, 50 of whom had never been vaccinated, and the other 50 had been vaccinated, then to each member of the unvaccinated group he would say,—"This pustule on your arm is the true small-pox—you have been successfully infected, and are now safe against the disease;" while to each of the vaccinated 50 he would say,—"This pustule on your arm is no evidence whatever of small-pox; I have been unable to infect you, because you have already been vaccinated." Not only would each medical man say this to his patients—he would also say so to his medical *confrères*, and his medical *confrères* would with equal solemnity say so to him—they had also experimented, and with a similar happy result. Indeed, besides seriously telling his patient and his professional brother this contradictory tale, he would with equal sincerity repeat the story to himself, and he himself would swallow it as easily as his patient and his colleague had done! And not only would this happen in London, where Jenner's treatise was published; it would happen wherever the inoculation test was tried,—all over England, and all over Europe, and in India and America, and throughout the world.

---

[1] *The Present Method of Inoculating for the Small-pox*, 7th ed., 1779, pp. 23–25.

This in effect is the theory that Dr Creighton sets up to explain away the variolous test. Do my readers think it incredible? To me it is incredible that they should think it anything else. Nevertheless it is so. Here is Creighton's summing up:—

> "We come, then, to this extraordinary result, that the very same degree of small-pox infection, namely, the local pustule alone, or the local pustule followed by an abortive fever and a few abortive pimples, which had come to be reckoned a sufficient manifestation of the disease when inoculation was an end in itself, was now reckoned an insufficient manifestation, and, in fact, an evidence that the infection had not taken at all, when inoculation was done after cow-poxing and with a view to test the alleged antagonistic power of the latter against small-pox."—(Pp. 146, 147.)

It is clear that the Doctor himself sees the halting nature of the thesis. Very oddly does he strive to make it pass muster, however, trying to turn even its weakness into strength. He says,—"I am aware of the *gravity* [the italics are mine] of that accusation against the common intelligence and moral prudence of the medical profession." The assertion that the charge is a "grave" one is a stroke of real genius. Its characteristic is not gravity, but absurdity.

In developing his argument, Dr Creighton, as usual, falls foul of Jenner. He desired not only to mislead the profession by the name Variolæ vaccinæ, but to guide it craftily into a method of testing which would be itself a fraud. He quotes Jenner's statement in the *Inquiry*, that "In some of the preceding cases I have noticed the attention that was paid to the state of the variolous matter previous to the experiment of inserting it into the arms of those who had gone through the cow-pox," and he italicises Jenner's words, "*This I conceived to be of the greatest importance in conducting these experiments.*"

Then he goes on to say—

> "It is only in one of the 'preceding cases,' not in 'some,' that any notice is taken of the point; but that notice is quite significant enough of what this super-subtle genius wanted to hint to his readers. Case III.—John Philips, a cow-poxed milker, aged 62, was tested with small-pox, the matter having been 'taken from the arm of a boy just before the commencement of the eruptive fever.' Just so, the variolous test was applied in the most mitigated form of Gatti's and Sutton's 'new method;' the matter for inoculation was taken from the local pustule of a previous case of inoculation, not from a general eruption of natural small-pox; it was taken at a very early stage, before it had undergone the supposed 'putrefactive' change which made it spurious; and it was inserted, not by a deep incision, but by a superficial puncture, as well as in small quantity."

Part of this, it need hardly be said, is mere assumption. Jenner does not say that he made a puncture (like Gatti) instead of an incision (like Dimsdale), nor that he introduced only a small quantity of matter. But, in the first place, I must amplify a little the quotation from Jenner. Here it is,—"*I inoculated him, and* *was very careful in selecting matter in its most active state.* It was taken from the arm of a boy just before the commencement of the eruptive fever, *and instantly inserted.*" The words which I have italicised are those which Jenner's critic did not think it necessary to include. Dr Creighton's accusation, therefore, comes to this—that Jenner had the audacity, or rather the folly, to describe as "matter in its most active state" that which the medical men to whom he was writing (and who were thoroughly acquainted with everything relating to inoculation) were well aware would produce variolation only "in the most mitigated form." But surely even Dr Creighton will not deny that limpid matter should be used alike for variolation and vaccination. Indeed, Jenner's advice as to the latter is almost exactly similar. "The limpid fluid should be taken for the purpose of [cow-pox] inoculation, as soon as the vesicle appears sufficiently prominent for that purpose.[1] Then, in regard to the matter being taken "from the local pustule of a previous inoculation," Geo. Lipscomb, to whom Dr Creighton refers as a well-known inoculator, deliberately aiming at getting a "mere formality of small-pox," says that matter should be taken "either from the inoculated part, or, what is preferable in the opinion of the most experienced, from the natural small-pox pustule." And Dimsdale[2] says, "It seems of no consequence whether the infecting matter be taken from the natural or inoculated small-pox as soon as any fluid can be obtained from it." Thus there was no advantage, in the way of mildness, to be got by using the local pustule, nor any depth of craft on Jenner's part in selecting it for Case III. Nor is Dr Creighton correct in asserting that it was only in this case, and not in "some" that Jenner had referred to the state of the matter used. In Case V. he says, "I inoculated her with active variolous matter." Case XX. was inoculated "with variolous matter immediately taken from a pustule." Case XXIII. "was inoculated with variolous matter from a fresh pustule," and "to convince myself that the variolous matter made use of was in a perfect state, I at the same time inoculated a patient with some of it who never had gone through the cow-pox, and it produced the small-pox in the usual regular manner." In the course of his references to the subject, Jenner argues as follows:—(1.) That variolous matter for inoculation should be taken early (and therefore in a limpid state), in order to get it not only in a condition of variolous activity, but also free from the risks incident to the insertion of pus; (2.) That it should not be preserved on cloth, corked up in a vial and carried in the warm pocket, so as to induce putrefaction, but, as he says in the words omitted by Dr Creighton, should be "instantly inserted;" (3.) That in performing the operation the adipose tissue should not be wounded; and (4.) That matter should not be inserted by lodging a thread, dipped in the virus, in the part operated on.

---

[1] *Medical and Physical Journal*, vol. vi. p. 64.

[2] *The Present Method of Inoculation*, 7th ed., p. 26.

Will any medical man excepting Dr Creighton see in these precautions any attempt to falsify the variolous test, or anything which does not meet with his heartiest approval? Indeed, Jenner's correctness of view as to the practice of vaccination itself is nowhere better displayed than in connexion with the parallel subject of the choice of lymph. Dr Creighton has great admiration for Woodville, and some even for Pearson, as compared with Jenner. But at a time when Jenner was insisting on "a rule never to inoculate [vaccinate] with matter after the eighth or ninth day," as stated in G. C. Jenner's evidence before the House of Commons' Committee, Woodville declared that "he had never been able to discover any difference" between the effects of "vaccine matter taken on the eighth, ninth, tenth, or eleventh day,[1] and Pearson held that "no difference is perceived between the effects of matter taken before the red areola appears and that taken when it is distinctly formed, notwithstanding the 'golden rule' [Jenner's] that has been laid down, never to use matter when such areola is distinctly formed."

Even, however, if Dr Creighton's opinion of Jenner's intention were correct, the question would remain, Were the means adopted at all likely to carry out the intention? Is it conceivable that medical men, having hunted through Jenner's book, as Dr Creighton has done, in search of the "preceding cases" which contained his references to the state of the variolous matter, and having found them, or rather having found the particular case which Dr Creighton says is the only case, should thereby be guided into the application of a fallacious test to the new inoculation? To a country practitioner advocating the prophylactic properties of a cow disease, of which they knew nothing, they might indeed listen. But were they likely to pay heed to the same man when he asked them not merely to reverse their views as to what constituted successful variolous inoculation, of which they conceived they knew everything, but to do so for the very purpose of using the reversed doctrine as a standard by which to judge of the success or failure of the Jennerian method of preventing small-pox?

Surely the "super-subtlety" is not with Jenner, but with his latter-day critic.

John C. M'Vail.

(*To be continued.*)

The Treatment of Lateral Curvature of the Spine, with Appendix on the Treatment of Flat-foot. By Bernard Roth, F.R.C.S., Fellow of Medical Society of London; Member of the Clinical and Pathological Societies, and of the Medical Officers of Schools Association. London: H. K. Lewis: 1889.

It is impossible to read this short book without agreeing with what is evidently the author's view, that he has some specially valuable knowledge which he wishes to share with others. The book is in fact what it professes to be, a chapter, and a valuable one, on the treatment of lateral curvature of the spine. Mr Roth refers to the causes of lateral curvature only to the extent necessary to advocate their removal as a preliminary to special treatment. He holds to properly directed muscular exercises as the only means of treating lateral curvature, and thinks that all forms of spinal support are useless or worse than useless. His argument is plain. In most cases muscular weakness and faulty position will account for the lateral curve; therefore, he says, strengthen the back muscles by systematic exercise, and avoid by watchfulness the faulty position. Mr Roth believes that supports only relieve the muscles of their proper work, and hence allow them to grow weak; although why supports might not be used in the interval between exercises, it is difficult to see, unless from the danger of trusting too much to the supports.

---

[1] *Pearson's Examination of the Committee's Report*, pp. 91–93.

The exercises which Mr Roth has found of service are minutely described. He also explains his method of measuring and recording the degrees of lateral curvature, so that surgeons may follow accurately the progress of their cases.

In classifying cases of lateral curvature, the author divides all cases into those without, and those with osseous deformity. The latter cases, even when slight, he thinks never admit of complete cure. He does not show us, however, how we can tell the point at which osseous deformity begins, and if we do not know this we cannot assert that no case of lateral curvature which has been apparently cured by treatment has not had some osseous deformity to begin with.

The cases cited show that great improvement in appearance and in comfort to the patients resulted from Mr Roth's treatment, when years of treatment by spinal supports had been of no use whatever.

An interesting and valuable chapter on the Treatment of Flatfoot is given as an appendix. The author is precise in all his directions for treatment, and freely illustrates his views by photographs and woodcuts.

As great harm has frequently resulted from the prevalent ignorance on the treatment of lateral curvature, we most cordially recommend this work to all our readers.

On Curvatures and Disease of the Spine. By BERNARD E. BRODHURST, F.R.C.S., Corresponding Member of the Imperial Society of Surgery of Paris, etc., etc. Fourth Edition. London: J. & A. Churchill, 11 New Burlington Street: 1888.

THIS work has now reached a fourth edition, the second having been published in 1864. In the first part considerable space is occupied with the consideration of lateral curvature. The causes of this condition are fully discussed, and we may note, as especially worthy of attention, the author's review of the theories of the rotation in lateral curvature, with his own concise and clear explanation of this phenomenon. For treatment he takes us back to complicated spinal supports again, and is neither clearer in their description nor more convincing as to their benefits than other writers who follow similar lines.

In the second part of the work the author conveys to his readers the results of his extensive experience of disease of the spinal column. But surely he goes too far when he says that "Pain is an invariable symptom in the commencement of spinal disease" (page 113), even if pain is made to include, as the author seems to do, tenderness on pressure? Patients are undoubtedly presented for advice where displacement, accidentally observed by a child's mother, has been the first, and for some time is the only symptom of disease.

In the treatment of spinal disease the author is a careful advocate of rest, but strongly opposes suspension for the application of a plaster jacket as being dangerous. He advocates the recumbent posture for this purpose.

The author's account of the treatment of cases in which suppuration has occurred is unfortunately marred by the want of a working knowledge of antiseptic principles.

Curvatures of the Spine. By NOBLE SMITH, F.R.C.S. Ed., L.R.C.P. Lond., Surgeon to All Saints' Children's Hospital, etc., etc. Third Edition. London: Smith, Elder, & Co., 15 Waterloo Place: 1889.

THIS work on *Curvatures of the Spine* is chiefly concerned with lateral curvature, a short chapter only being devoted to anterior and to posterior curvatures. The author accepts Harrison's and Judson's explanations of the rotation which occurs in lateral curvature, and discusses fully the diagnosis and treatment of this common deformity. He is an eclectic in his treatment, and advocates a special form of back-support—the description of which, however, is not very clear—and also certain muscular exercises which he believes are specially suited to overcome the chest deformity. Whether they are of value more than as general exercises is doubtful. The author's view of their rationale is difficult to follow. He gives, however, this useful precaution: "Here, as in the former instance, the surgeon must *see* that the effect is beneficial before he allows the exercise to be practised."

Our chief objection to this book is a want of precision in directions. Thus the principles only are given of the spinal support: a practitioner would like to know the details of construction also. Again, the author says this instrument is to be used when a spinal apparatus is necessary, but he does not tell us when it is necessary. Again, he says, "If a case is to be treated without one (*i.e.*, spinal instrument), the following plan may be followed," but we are not told how to decide whether to do without an instrument or not.

Mr Noble Smith gives a great deal of useful information in his book, but often leaves his reader just at the critical point to seek for information where he can find it.

As an Appendix the author quotes Blaikie's directions for simple exercises for children as a preventive treatment.

Lavage de la Vessie sans sonde. Par J. M. LAVAUX. Paris: G. Steinheil: 1888.

THE question of washing out the bladder without a catheter depends on the amount of force necessary to overcome the resistance of the compressor urethræ and the muscular coat of the bladder. To accomplish this the pressure of the atmosphere may be employed, and Dr Lavaux applies this force by means of a very simple apparatus. He has the usual reservoir for containing the antiseptic fluid, a long rubber tube (with a stop-cock or clip), and a nozzle that fits into the orifice of the urethra. This nozzle is the special part of the apparatus. It consists of two parts—the outer, shaped somewhat conically so as to fit the urethra tightly; and the inner, which is a hollow cylindrical stilette. There are various sizes of the stilette, and the force of the bladder injection depends on the diameter of this tube and the height to which the reservoir is raised above the bladder.

Dr Lavaux says that the bladder is easily safely and painlessly filled by this method. When the bladder is full then the patient empties it himself, the nozzle having been removed. The operation may be frequently repeated at a single sitting. There is "gentle, continuous, constant pressure easily regulated and measured."

Dr Lavaux records a number of cases in which he employed his method with benefit to his patients. He recommends its employment in cases of cystitis, calculus, stricture, and operations. It is only applicable to cases in which the patients can void urine naturally. The method and apparatus seem simple and harmless, and may be very useful in certain cases, especially where antiseptic and anodyne irrigations are indicated.

An Atlas of Illustrations of Pathology. Fasciculus VI. London: The New Sydenham Society. Agent: H. K. Lewis.

THIS fasciculus contains one plate illustrating hydatid disease of the liver, and four illustrations of urinary calculi. The descriptive letterpress on the latter is by that most versatile surgeon Mr Jonathan Hutchinson, and possesses the character and quality which we look for from him. The illustrations are taken from the catalogue of the Royal College of Surgeons' Museum, and the author acknowledges his indebtedness to the same source for much of the descriptive part. After dealing with the composition and structure of calculi, he briefly and pointedly considers the possible explanations of local prevalence of calculi; the distribution of calculus in reference to climate, race, etc.; and he sums up with an enumeration of the various conditions which may favour the formation of urinary calculi. All this latter part is full of suggestiveness and calm judgment, and is well worth perusal.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXVIII.—MEETING VIII.

Wednesday, 20th March 1889.—Dr JOHN SMITH, *President, in the Chair.*

(*Continued from page* 173.)

Dr Cathcart.—I merely intended to speak on one subject, the local causes of tubercular glands. When I was thinking over the subject I did not expect that the question of the lymph origin of the tubercular bacillus in the glands would be discussed, but would be taken for granted, and therefore, perhaps, I may say a few words as to why I would incline to support the lymph entrance theory rather than that by the blood stream. In the first place, it seems so easy to trace a distinctly local cause in the majority of cases of enlarged glands that it is almost going out of the way to account for it by a condition of the blood. Take, for instance, the enlargement of the glands in the neck from other causes than tuberculosis, as we find them, for instance, in malignant disease. We find the glands distinctly enlarged, and we always attribute that to an involvement of the glands from the diseased locality. In this way, when the disease is in the tongue or jaw, we find the corresponding glands enlarged, and never suppose for a moment that the origin has been an infection going from the original seat into the general system, and afterwards localizing itself in these particular glands. One of the difficulties suggested was to trace the course of the lymph infection. Dr Duncan said that in a large number of cases no visible breach of surface could be ascertained, and also, as the result of his experience, that the glandular affection in the neck was very often symmetrical. In regard to the first of these statements, Dr Duncan, besides stating that he was unable to trace any cause, said, "We find distinctly tubercular manifestations in joints where we cannot find any localized cause which would be the starting-point in them." But Dr Woodhead's investigations throw light upon this, for he showed that in a large number of children dying from other diseases, where there was no suspicion of enlargement of the mesenteric glands, this did exist, and it might account for the condition in joints in such cases as Dr Duncan referred to.

As regards the want of being able to trace a distinct breach of surface, it is not necessary that one should trace the breach of surface at the moment that the glands are enlarged. It is quite easy to suppose that there has been a breach of surface, that the tubercle bacillus has got into those glands, and that the breach of surface has healed, so that at the moment it is inspected there is nothing visible. Dr Woodhead shows that in a certain proportion of cases of enlarged mesenteric glands we are not able to trace injury in the intestine, although that was believed to be the source of them. In my comparatively limited experience, I have seen a large number of cases where the glands were distinctly unsymmetrical, and have always been able to associate these with want of symmetry in a local origin. The last case was in Dr Duncan's Wards, where he was treating it. The history of the boy was that he had one of his tonsils excised for enlargement some time ago; the other tonsil is now in the same condition, and the glands of the neck are enlarged on the same side as the tonsil involved. Then another point is—If the blood channel and not the lymph channel is the way by which the tubercle bacillus affects the glands, why should it be that these particular lymphatic glands were the first affected? It is quite easy to say, and it would be difficult to gainsay it, that we cannot account for the predisposition to attack certain parts, and that the tubercle bacillus is specially liable to attack glands. But why should it specially be the glands in connexion with the alimentary canal rather than the lymphatic glands of other parts? Treves has shown that in 155 cases of tubercular glands in the Hospital for Scrofula at Margate, something like 133 were in the neighbourhood of the neck, and a small portion connected with the skin, with the joints, in the groin, and the glands in the axilla.

Having thus tried to establish what seemed to be a starting-point, that we must, in most cases, look for a local origin as the causation of enlarged glands, I had intended to limit my remarks to one local cause which was very frequently omitted—that was painless enlargements of the fauces as causes of enlarged glands. The conditions are, first of all, chronic enlargement of the tonsils and adenoid tissue of pharynx, with thickening and ulcerations of the mucous membrane—this condition following a chill or coming on without any other apparent cause, except that the young person is slightly out of health. In two or three cases, seen lately, I found enlarged glands in the neck; and on examining the condition of the pharynx I have discovered, and unexpectedly to the patients, that they were suffering from this condition of the throat. They were not conscious of having any sore throat, and, without examining, one would have supposed that there was no local origin to account for the mischief. The other condition is what one finds in attacks of measles. It is part of the symptoms of measles that the mucous membrane of the nose and naso-pharynx should be attacked, but I think there are probably a large number of cases where the symptoms are so slight that the physician never takes any heed of them, and these are the cases where very often the child recovers from measles but remains with a scrofulous tendency. In a certain number of cases the tubercular attack comes through the alimentary canal,—where it is said that a child was quite well until it had measles. From that we can often trace a distinct tubercular condition of the joints and bones.

As regards the explanation of the pathology of these conditions, it is generally recognised that when lymphoid tissue is attacked the affection of the glands is much more rapid and certain, and that is the reason why it has been found that the glands of the alimentary canal are so frequently involved. The tonsils are themselves masses of lymphoid tissue, and there are localized masses in the pharynx, and all down the alimentary canal there are deposits, especially in the small intestines. What may be the cause of the throat enlargement I am not prepared to say. Whether it is an indication of the strumous taint or hereditary tendency I am not prepared to say, but in most cases under my observation showing glands enlarged and tending to caseate, there was no history which would have accounted for this mischief, and I suppose that it was purely a local condition. Then it is also recognised that any localized injury is liable to promote the entrance of the tubercle bacillus. It is not only a predisposing cause, as Dr Duncan has allowed, but I believe that it is at the same time an actual cause. The tubercle bacillus enters at this weakened spot. Then, as regards the source of the bacillus, Dr Woodhead has given good evidence that the milk is, in many cases, an actual carrier of the tubercle bacillus; and we also know that many tubercular conditions seem to be attributable to the spores found in the air contaminating many parts besides those affected by milk. Thus bronchial glands may be distinctly tubercular. Then, as regards the painlessness of the tubercular glands. One of the distinctive clinical characters of this tubercle bacillus growth seems to me its painlessness. The effect is so slight in itself that unless there be something else working, or at least a very great aggravation of the tubercle bacillus, the irritation it sets up is so slight as not to cause pain. For instance, in strumous joints the patient does not complain of pain until the cartilage is affected. And about tubercular bones there is often no evidence of periostitis. That will help us to understand why the condition in question is so chronic, and why, if there be an actual tubercular condition starting this mischief, it may not be necessarily painful. Then the treatment is partly prophylactic. In the first place, by maintaining the patient's general health we prevent the liability to this condition; but the point I wish to lay special stress upon is that there should be a very careful routine examination of the tonsils and the throat in all cases of enlarged glands, especially those near the angle of the jaw. I have had under my care, several times, patients who have been treated for glands by external applications, in whom I have found a distinct local condition which had been setting up and keeping up the mischief, and without the cure of which any other treatment would be, of course, useless. So much is this the case, that I would also say that it ought to be the routine part of the treatment of measles to examine the throat and then to treat it. I think too much has been spoken in a general way of treating these conditions. Wherever the throat is in this chronic, painless, enlarged, and ulcerated condition it should be painted with an antiseptic, and I have found that equal parts of tincture of iodine and glycerine locally applied with a brush, combined with absolute rest, a very efficient treatment. I have very often tried the fixation treatment which Dr Duncan has advocated, and found it most admirable. I heard him in one of his clinical lectures laying down this principle, and I have found it most efficient.

Dr James Carmichael.—I am obliged to you for giving me an opportunity of offering a few remarks, and would take leave to say that I think there is no subject which requires more careful attention, both from a medical and surgical point of view, and in which the practitioner requires to avail himself of varied resources. The surgeon who treats the case solely from his own point of view, without due reference to the general condition of the patient, can hardly expect good results. I will confine my remarks to the etiological basis of the treatment, and they will be as short as possible. I believe there is no such thing as idiopathic lympathic glandular disease even in strumous children. The glandular enlargement in all cases is the result of peripheral irritation of skin or mucous surface. It seems to me that the keynote which Mr Annandale struck in the earlier part of his remarks was the right one. We should rather, instead of classing these enlargements of the glands as "strumous," use the term adenitis, because I believe clinically, at all events, we cannot at first distinguish between simple and strumous enlargement of glands. There is no definite, no hard and fast line between the ordinary inflammatory conditions of a chronic nature and struma. We know that in struma the ordinary manifestations are of a chronic nature; that there is no healthy suppuration, but a tendency to formation of cheesy products. The leading features of a strumous gland are these,—that the gland is in a chronic condition; that there is an increase of corpuscular and lymphoid cell formation, with tendency to degeneration, softening, and caseation. In other cases we have a different course, attended with acute inflammation and a formation of pus. I do not think we can always distinguish between the simple adenitis and the tubercular form. In regard to treatment, it must necessarily be etiologically determined, and that is our difficulty at starting. Take a very common example of adenitis—that of the femoral glands from a wound in the foot. We have all seen cases of simple and scrofulous adenitis in this way. A boy running about barefooted gets adenitis of the femoral glands from a wound in the foot. We meet in one case with an inflamed bubo and the formation of a healthy pus which heals under ordinary treatment; in another you have a chronic condition in which you have not the formation of a healthy pus, but of cheesy products, with chronic inflammation. Mr Cathcart alluded to cases of cervical adenitis in connexion with the faucial and nasal conditions after measles. In his experience in this disease adenitis was much more common in the bronchial than in the cervical glands. The cervical condition is much more common in scarlatina. Bronchial adenopathy is a dangerous complication after measles and also whooping-cough. We cannot doubt that in all these cases we have an irritated condition—a hyperplasia—of the bronchial glands. If the child convalesces favourably, and is naturally healthy, everything goes well. If it is a strumous child, and has a disposition which fulfils those conditions known as struma, then the glands become tubercular or strumous, which I take it, in the present state of our knowledge, are synonymous terms, and the probable ultimate result will be general tubercular infection. There is no doubt that you have these cases of simple adenitis in measles and whooping-cough, because the enlargement of the glands gets perfectly well, and it depends very much on the condition of the child whether this is the case, or the glands go on to tubercular degeneration. Then in regard to the treatment, it must be etiological. You must seek out the cause or you are groping in the dark, and you have no chance of treating the case successfully. All sources of peripheral irritation must be removed. In regard to whooping-cough and measles, I am in the habit of always teaching and advising that children suffering from these diseases should be guarded very carefully during convalescence, because it is often months before the signs of tubercular or strumous disease manifest themselves. Therefore I think the constitutional element is of great importance. The child should be carefully nourished, and should have change of air if possible, and the administration of tonics. But when I say carefully nourished, I would enter here a caveat. We are all too much in the habit of recommending a sort of routine treatment. We say, "The child must have the best nourishment," etc. That is all very well, but if you over-feed a delicate child, it is perhaps worse than giving it too little, because the digestive power is overtaxed. Overdosing with cod-liver oil is a common mistake. Only the other day I saw a little boy suffering from adenitis in the neck, several abscesses had been opened, and I recommended that scraping should be adopted, and the little fellow improved very much; but in this case there was another, and a much more important part of the treatment, requiring attention. He was being stuffed with everything he could take, and the stomach and bowels were in a state of irritative catarrh. I carefully ordered a regulation of the diet, reduced it very much indeed, and stopped all the tonics, substituted simple stomachic remedies, the child improved, and is now doing well. In regard to the mouth, I quite agree with the remarks of the previous speakers as to the importance of attention to it, which is too often overlooked. The slightest cause of irritation will give rise to enlarged glands—not only give rise to enlargement, but keep it up. It is no use looking at the mouth and seeing that there is nothing about the tonsils and the fauces. You must ascertain the state of the tongue and digestive system generally, and examine all the signs and symptoms, and you will seldom fail to relieve. As to the special surgical treatment, I have perhaps no special title to offer any remarks. Scooping is the best method, I believe, of treating those glands; they seldom require excision except in isolated cases. As regards internal remedies, they are entirely secondary to dietetic and hygienic conditions, but such drugs as arsenic, barium salts, and lime salts are often useful. The chloride of calcium has a wonderful effect on the nutrition of the lymphatic glands. Iron is also beneficial, but must be carefully given. Cod-liver oil is extremely useful, but should be given in carefully regulated doses. External applications of an irritant nature are worse than useless in strumous adenitis. One form of local application, however, the mercurial, is decidedly useful.

Dr Woodhead.—In the few minutes at my disposal, I should like to take up one or two points that have been traversed already by several of the speakers. I should like it to be understood that any supposed pathological fact that does not agree with the clinical history of a case must be thrown on one side. In the same way I should reject all clinical observations that do not agree with pathological facts. First of all, I should like to say a few words about the tubercular inflammation of glands and about the histological changes which may occur in these glands, and to point out that in these changes a giant cell is not an essential part of the tubercular nodule. It is rather of accidental occurrence, not anything specially belonging to it. One finds in the case of tubercular inflammation that this form of histological structure is met with, but it is the result of several conditions into which I need not here enter. There are other forms of evidence of tubercular inflammation quite apart from the giant cell. I should join issue with those gentlemen who look upon the terms used in the classification of glands as all coming under one heading. Undoubtedly they are all forms of adenitis, certain forms of inflammation of the gland; but we must remember that a gland may be enlarged as the result of various forms of inflammation, and that these have a definite course in each case. We know that in certain forms of simple adenitis we have the inflammatory products running through a certain series of changes, and ultimately there may be a return to something very near the normal condition. We have again a form of inflammation in which, owing to the presence of a certain irritant material, certain forms of micro-organisms probably, we have a suppurative inflammation that results in the tissues breaking down (under the action of those micro-organisms). Then, again, as in tubercular inflammation, we have a special tendency to caseation in certain forms of inflammation. It is this special tendency to caseation in certain forms of tubercular disease with which we ought to deal when we are speaking of struma. It was remarked that in the history of struma we had the history of tuberculosis. That is true so far, but only so far, because struma is only the evidence of one form of tubercular inflammation, in tissues in a certain condition, and with a certain form of irritant. In considering the condition of tuberculosis of glands, we have two factors to take into account. We have, first, the amount and intensity of the irritant material present; and, secondly, the condition of the tissues themselves.

Now, in struma we have a condition in which the gland is exceedingly ready to undergo caseous degeneration in the presence of a small number of bacilli. That is, we have in struma a weakened condition of the adenoid tissue, which causes it to be much more readily affected by the tubercular irritant, and it breaks down under this irritant inflammation, and undergoes caseation much more readily than does a healthy gland. I believe that that is really what we should understand by struma. This condition, of course, may be inherited, or it may be acquired, and I pointed out in a paper I recently published that an acquired condition is very frequently met with in children as a result, say, of catarrhal inflammation of the intestine. The gland itself in non-specific inflammatory conditions has more work to do; its vitality is thereby lowered, and when attacked by the tubercular bacillus, it undergoes inflammatory and caseous changes much more readily than it would if the tubercular bacillus had got in under ordinary conditions. We may have the same condition inherited. Take any tissue in any part of the body in one individual coming into the world under certain conditions. Those tissues will not react to stimuli in the same way as do those in a healthy individual.

There is one point, however, that I should like to mention, and that is that we are always apt to go to extremes, and I believe that a very large number of the conditions which clinically are attributed to the presence of tubercle bacilli are actually not due to tuberculous action at all. Recently we have got into the way of assuming that every induration, especially in glands, is the result of a tuberculous process. I for one do not believe this. I am as firm a believer in the activity of the tubercle bacillus as any one, but I don't believe that all the cases put down as tubercular come under that heading by any means. That by the way. There is one point I should just mention here, and that is that Prof. Annandale would keep out the syphilitic inflammation from those cases of adenitis. I believe the syphilitic form is just as true a form of adenitis as is the tubercular. It is one that plays a very important part in a very large number of cases of disease of the glands. Then there has been mentioned a point in connexion with the spread or affection of tubercular glands, whether it is by the lymphatic or the vascular channels. Dr Duncan said he believed that in a large number of cases the affection of the glands was by means of the bloodvessels. Now, if Dr Duncan means by that the lumen of the bloodvessels, I join issue immediately. If he mean, however, the lymph sheaths around the vessel, and at the terminal ramifications of the vessel, which are more readily affected, then I agree with him. But beyond that I cannot go. In the majority of cases, tubercular affection of the gland, especially the strumous form, is a comparatively chronic process. You find it associated, in almost every case, with tubercular ulceration, because tubercle is essentially a disease of free surfaces. As pointed out by Coats, you always find tubercular ulceration, or the absorption of tubercle from a free surface, somewhere in the neighbourhood of the enlarged glands. In tubercle of the mesenteric glands you may find no ulceration of the intestine, but, as a rule, you can find some history of catarrhal process, or of some irritation of the intestine, in the region of the glands. In the mediastinum you find them associated with tubercle in the abdominal cavity, or connected with disease of the lung—in fact, you will always find evidence of there having been at some period or other an altered surface from which these tubercle bacilli could find access. Then, too, one remembers that the blood supply from these parts to the glands is not a direct one, but that the lymph supply is essentially direct. We may look upon the lymphatic glands as regular filters placed at intervals along the crown of the lymph stream throughout the whole of the body; in evidence of this we find that wherever a tuberculous gland is present the glands beyond it are not affected for some time; once excise the gland, and you find that the second is very rapidly affected. There is a filtering process going on along the course of the lymph stream and along the line of the lymphatic glands. You do not find caseation along a line of glands unless it takes place in a regular order, from one extremity to the other. Then as regards the resistance of the tissues: I have said before this is one of the factors; but another factor is the number of bacilli, because one finds that, in certain cases, there may be a very considerable number of tubercular bacilli actually passing along a healthy gland, and there being destroyed as they pass from the ulcerating surface. But if the gland tissues be weak, then the cells of the gland are not in a fit condition to enter into that contest of which Dr Duncan spoke, and the tissues undergo a rapid degeneration. I think, in connexion with this, one should mention that certain organs appear to be specially affected by bacilli getting into the blood stream. Bacilli may get into the blood stream in a variety of ways, but it has only lately been pointed out that they pass from the lymphatic glands by the blood stream. One finds that if bacilli make their way into the blood stream, there ensues a condition of acute general miliary tuberculosis, and it is only when the bacilli do get into the blood stream that you get a miliary tuberculosis. It has been suggested that the meningitis of the brain one finds in young children is due to the passage of the tubercular bacilli by the blood channels to the base of the brain. In the same way it has been pointed out that bacilli may pass from tubercular glands into the general circulation, and set up a somewhat chronic form of miliary tuberculosis; where there is a slow passage, not as in the case of hæmorrhage, but a somewhat slower process, as in certain cases of miliary tubercle of a more chronic character, there is still evidence of an undoubted miliary tubercular condition.

Dr Duncan spoke especially of tubercular condition of bones and joints. I believe that in a very large number of cases, and experiments support this fact, cases of tubercular disease specially affecting the extremities of the bones and the joint itself are the result of the passage of the tubercular bacilli along the bloodvessels. But, on the other hand, if one examine a number of the vertebræ in which you have caries, one is able to determine the passage from the glands in the mediastinum to the bone, and thence to the membranes of the cord. It did not appear that there was the very slightest evidence of the passage by the bloodvessels, but there was evidence of the passage of bacilli by the lymphatic channels; but this is in case of the vertebræ, and not in the long bones. In them I believe, with Dr Duncan, that the affection by the bloodvessels is of far more frequent occurrence than the spreading by the lymphatic circulation.

Another point that makes me think that these glands are infected specially by the lymphatic channels is, as we have said, that it is a comparatively chronic condition. Wherever it has been traced to infection by bloodvessels it has been an acute, or comparatively acute condition. In certain glandular cases it is undoubtedly so.

It may affect the glands just as other organs may be so affected, but in the majority of cases it is a chronic process. Even where caseation has become advanced, we find there is a dense fibrous layer at the periphery of the caseous mass. This is another point in favour of supposing that the lymphatic glands are affected by the lymph channels. In regard to surgical interference, I think there are certain points which some of the points I have mentioned may help to clear up. From my own observations—and, of course, I do not know how far that is borne out by clinical experience—I should be very chary of scraping without first applying the cautery. I believe in a number of cases the history of an acute miliary tuberculosis dates from the scraping of some of these glands. This we can understand if we remember the condition under which we are working. We have a large, or it may be a small number of tubercular bacilli, a caseous mass in which there are spores, and we have this mass in a closed cavity which we constantly bring into connexion with the circulation by scraping out part of the caseous material and by breaking down the fibrous walls themselves. This strikes me as being exactly what takes place in hæmorrhage from the lung in phthisis, where you have the presence of small blood-clots in which tubercular bacilli can develop in enormous numbers; unless you can so stimulate the tissues around these clots that you set up a rapid inflammatory reaction, you have the bacilli taken direct into the blood circulation and giving rise to the many tubercular sequelæ. The cautery, I believe, would be of more value than any antiseptic. Iodine would be valuable with the cautery. In iodine you practically set up a granulating surface, formed in the most rapid way you can form it, and to a certain extent protect the bloodvessels from the invasion of the bacilli directly. There are a number of ways, of course, in which this may be done. I believe if you could cause suppuration in a gland by injecting a strong escharotic, you could obtain the same results if you leave the opening free; but in any condition where we have a closed cavity I believe it is dangerous to the health of the patient. I should object very strongly to the injection of bichloride of mercury, which may so weaken the tissues not actually destroyed, that by the time they have recuperated the bacilli have attained the upper hand. Not only that, but the bichloride of mercury in presence of albumen of almost any kind rapidly loses its action as a germicide, and would therefore affect the cells directly in the first instance, and would not affect the bacilli, as one is led to expect, in the second instance. Of course, I am now speaking theoretically, and must ask indulgence from the surgeons, but I give what I believe to be the result of observation. I have noted several other points which I think would be better taken up by other speakers. The only point I should like to insist on is, that in all cases, in the first instance, tubercular glands are localized to that area presided over by them, in which we have the tuberculous irritation or inflammation, and that the process is, in the first instance, the same as in the case of all other irritations; that the irritant is specific in its character, as are also the results.

Dr Woodhead, in reply to a question by Dr Allan Jamieson, said —The chloride of zinc would be valuable to a certain extent if we scrape out the glands and get everything away as freely as possible, but only under those conditions should I say that any antiseptic is of any good at all.

Professor Annandale.—Dr Woodhead has expressed the opinion that there are many cases which resemble tuberculosis but are not tubercular, and mentioned that the giant cell is not necessarily diagnostic of tubercle. Will he kindly tell me what is diagnostic of the tubercular glands? Is it the presence of the bacillus, or the presence of the giant cells, or all of them together?

Dr Woodhead.—I should certainly say the whole of them together. The ultimate result of tubercular inflammation is caseation. In chronic cases, even where caseation is not distinctly marked, there are almost invariably minute foci of caseous degeneration. In the earlier stages you must look for bacilli, though you may not find them in every stage, yet you do get them in some stage of the disease.

Professor Chiene.—I had intended to make some remarks, but the point that I really wanted to bring out has been brought out by Dr Woodhead. He does not need to crave my indulgence as a surgeon at all. He has put forward exactly the position I intended to take up to-night. I brought two microscopic specimens to bring out the point that scraping tubercular glands is a very dangerous process. The tubercular bacilli are, as these specimens will show, close to the capsule, and I don't believe that it is possible to remove the organisms by the scraping process. I have, I acknowledge, frequently adopted it as a surgeon. Sometimes you are driven to it when you have to deal with an open gland, and the great point, therefore, is to have a free opening. I wish to make one other remark—that bichloride of mercury is not a good thing. It acts upon the cells and destroys them. Recently a series of experiments have been made in regard to a new antiseptic —new in this country, but used in America—a material termed hydronaphthol, which is more powerful than the bichloride. It doesn't act upon the cell elements nearly to the same extent as bichloride of mercury, and I want to bring it forward as an antiseptic. We may be able to introduce it into a cavity after we have found it necessary to scrape the glands. I am pleased to hear that Dr Woodhead desires us to throw aside bichloride of mercury, because I believe it has not been efficacious. The scraping has this tremendous disadvantage, that you open a large number of vessels and expose individuals to miliary tuberculosis. In two cases lately in which I have done this in the prostate (I didn't know then of hydronaphthol), the one patient is dying, and I don't think there is very much chance for the other, as the result of my operations. It is such cases as these that have brought before me the evil of scraping what I believe a primary tubercular gland, by which I mean any chronically inflamed gland. You should excise it, and the sooner the better. You say, "Then you would excise chronic adenitis." I say, "That is a very small matter in the case of a thing which at any time may give rise to disease." I do not believe in the injection of iodine into a gland. You enclose dead tissue, and give the tubercle bacillus a chance. You are to scrape with a large opening that the mischief may escape. All antiseptics have failed me in the past; but hydronaphthol should receive a trial, because, with all respect to Dr Woodhead's views as to the cautery, deep glands in the neck are in a situation which renders it impossible to apply the cautery. I am glad that Dr Woodhead has given his sanction to the fact that scraping should be avoided.

Dr James Ritchie, from a practical point of view, thought it of great importance that the relation between struma and tubercle should be recognised. He believes that in struma we have simply a peculiar vulnerability of constitution which may be inherited or acquired, syphilis playing a very important part in some cases. But that doubtless there are many cases in which, through unfavourable hygienic conditions, or overfeeding, or something which has injured the constitution of the individual, he is made much more liable to the invasion of the tubercle bacillus.

SESSION LXVIII.—MEETING IX.

Wednesday, 1*st May* 1889.—Professor CHIENE, *Vice-President, in the Chair.*

I. DEATH OF AN HONORARY MEMBER.

The President read a letter from Madame Donders intimating the death of her husband, Professor Donders of Utrecht. It was unanimously resolved to enter it in the minutes and send a message of sympathy to Madame Donders.

II. EXHIBITION OF PATIENT.

Mr F. M. Caird showed a PATIENT on whom the operation of skin-grafting had been performed for an extensive destruction of the skin of the left upper arm by burning. The upper portion of the sore had been treated first by Thiersch's method, large pieces of epidermis being transplanted from the patient's thigh with a very successful result. The lower portion was afterwards treated by grafting of small pieces taken from a recently amputated foot. Two coloured drawings showed the sore in different stages after the skin-grafting.

III. Exhibition of Drawings.

Mr Caird showed COLOURED DRAWINGS OF SYMMETRICAL GANGRENE OF THE EXTREMITIES. The patient was a woman. She had had no premonitory symptoms such as occurred in Raynaud's disease.

IV. Original Communication.

Dr A. James read his paper on SOME POINTS IN THE PATHOLOGICAL PHYSIOLOGY OF PLEURISY AND PLEURITIC EFFUSION, which appears at page 223 of this Journal.

Dr Affleck thought Dr James's paper required more consideration than they could give it immediately after hearing it read. He would refer to only one or two points. The position of the pleurisy was probably in the majority of cases as Dr James had said, but it was by no means always so. He accounted for this position by its greater exposure to cold and the freer movement of lung taking place there. The first of these, exposure to cold, was a cause which could not be ignored. Some constitutional condition, however, very often preceded the action of cold. It was curious to note how seldom a contiguous serous membrane, the peritoneum, was affected by cold. With regard to movement of the chest, Dr James was probably aware that relatively in women the upper part of the chest moved much more than the lower. Had he made any observations on pleurisy in women, bearing this fact in mind? As to the level of the effusion, his experience was that it was frequently highest posteriorly and lowest anteriorly, just the reverse of what Dr James had found. This was a matter governed probably by the exudation and adhesions which had taken place.

Dr James Ritchie agreed with Dr Affleck that the constitutional condition had much to do with the development of pleurisy, and probably after it the causes laid down by Dr James came into play, but he doubted whether the thinner covering of the lower costal parietes had any causal relation. He did not accept Dr James's explanation of slight displacement of the apex beat of the heart in cases of moderate effusion. In the experiment on the dead body described by Dr James in support of his theory, Dr Ritchie believed that the displacement of the heart is chiefly due to atmospheric pressure at the side opened, and not to the elastic traction of the opposite lung, and he believes that all effusions exert pressure in proportion to their amount. As a further proof that fluid caused no intrathoracic pressure, Dr James stated that much fluid might be present and yet fail to flow when a trocar was inserted. Dr Ritchie had never experienced this result, and he believed it could only happen if the lung were bound together by strong adhesions. He wished to know why vocal fremitus was increased when the lung began to expand.

Dr James Carmichael thought the apical physical signs of pleuritic effusion were of great clinical interest, more especially in the case of children, in whom they were specially distinctive. He thought Dr James's explanation of the cause of these apical signs was probably the correct one, and he commended his clear explanation and elucidation of them. He recollected many years ago hearing Traube of Berlin in one of his cliniques give a very similar explanation of the phenomena. He did not agree with the theory that in pneumo-thorax they had always collapse of the lung. There was a certain class of cases in which Nature effected a cure of purulent effusion. He referred particularly to those cases where there was a limited or partial pneumo-thorax established in the efforts of Nature to effect a cure—those cases in which there was a discharge of the pus by the bronchi, and limited pneumonic areas on the surface of the pleura, ulceration of the membrane, and escape of pus into the bronchial tubes. In these cases they had pneumo-thorax present and coincident with this expansion of lung. He had met with many of such cases, the last not many months ago. A little boy expectorated in this way a pint of purulent fluid in a very short time, and a perfect cure resulted. This was a matter of very great interest, showing that clinical facts did not always accord with preconceived ideas on the subject. In ordinary cases of pneumothorax in which a partially or generally consolidated lung existed the tendency was to retraction rather than expansion.

Dr W. Russell said Dr James drew attention to a matter well known to physiologists, but which clinicians were apt to ignore—the greater functional activity of the lower part of the chest. Dr Affleck's reservation on this point he thought correct, for in the lungs of females in the post-mortem room the upper parts were more emphysematous than in males. Whether the greater functional activity of the lung at the lower part was to be regarded as a common cause of pleurisy depended on the question whether this was the commonest situation for pleurisy. His own experience would hardly bear this out. The greater majority of pleuritic adhesions he found post-mortem at the apex and along the posterior aspect of the lung. Upon the determination of the situation hung a considerable part of Dr James's contentions. The process was supposed to spread from this lower surface by infection. He could not say he was inclined to accept this infective theory. The question arose, Why should the action cease before it involved the whole lung? Further, they knew that in inflammatory affections of the pleura in this situation there was great immobility. The diaphragm was restrained from its action, and the chest-wall was also restrained. This might, of course, account for the arrest of the inflammatory process. The statement that the apex did not give symptoms of pleurisy when present was one he was hardly prepared to accept. There was, no doubt, difficulty in distinguishing between a pleurisy at the apex and a pneumonic condition, but because of the difficulty he was hardly willing to grant that the condition did not give rise to palpable symptoms which the physician might recognise. He was not inclined to accept Dr Powell's and Dr James's views as to the displacement of the heart and diaphragm. It was all very well to talk of the elastic traction of the lung, but it was difficult to understand how this acted, seeing the lung was in a condition of hyper-distention; and, in addition, the view was based on the statement that in moderate degrees of effusion there was displacement of the heart and diaphragm. This was a statement open to question. He doubted if there was such displacement of heart or diaphragm in moderate degrees of effusion. He agreed with Dr Ritchie, for he had found no difficulty in removing fluid from the pleural cavity with an ordinary trocar and canula. The position of the fluid was determined very much by the position of old adhesions. They frequently found effusions limited by old adhesions. The presence of these, and owing to them the compression or collapse of the lung in certain directions, gave rise to physical signs which sometimes were confusing. Not long ago he saw a case in which pneumonia was diagnosed at the posterior part of the lung. The physical signs were dulness on percussion, bronchial breathing, and some fine crepitations. There were certain points connected with the case which led to the belief that there was effusion, and the aspirator drew off a large quantity of fluid, and forthwith the bronchial breathing, increased fremitus, and other signs of pneumonic consolidation disappeared. This was a mistake which he fancied was oftener made than they perhaps were inclined to recognise. The view with regard to the cause of increased fremitus was a feasible one, but there was evidence to show that the carnification of the lung produced necessarily a condition analogous to pneumonic consolidation. The lung was practically a solid body. It acted like a solid body in conducting sound. They had bronchial breathing as in pneumonic consolidation, and the breathing louder than it was over the trachea. The lung was really, then, an intensifier of sound.

Dr James agreed with what had been said of cold as a cause of pleurisy. It was not his own statement. In the books the covering of the lung was said to be thinner here than elsewhere, and this was supposed to influence the situation of the pleurisy. He did not believe that in the natural state the upper part of the lung in women was the most used. He did not believe that pleurisy in them was more frequent at the upper than at the lower part of the lung.

As to the level of the fluid, he was well aware that that was very much governed by adhesions and the state of the lung. What he said was that, if they took a healthy lung in a healthy pleura and allowed fluid to collect in that pleural cavity, they got a curved line which was highest laterally and lowest posteriorly. In referring to Douglas Powell's experiment, he wished to explain that when an opening was made in the pleura the corresponding lung collapsed completely, the opposite one slightly, and the heart movement showed that in the normal state each lung dragged upon the heart to a certain extent. By opening into one pleura one lung collapsed, and its elastic traction on the heart was taken away. The adhesions at the apex of the lung and along its posterior border referred to by Dr Russell were, he held, due to secondary pleurisies. Pain and friction were certainly not marked in these positions as they were at the lower part of the lung. As regards the vocal fremitus, he was glad to know from Dr Carmichael that a similar explanation had been given by Traube. He was not aware of it, and had not found it referred to in any of the books, and had regarded it as original. As to consolidation, Skoda had shown that consolidated lung did not conduct sound better than the healthy lung tissue (*Markham's Translation*, p. 39), hence the increased vocal fremitus and resonance so often met with in the reabsorption stage of pleurisy could not be explained in that way.

SESSION LXVIII.—MEETING X.

Wednesday, 15*th May* 1889.—Dr JOHN SMITH, *President, in the Chair.*

I. EXHIBITION OF PATIENTS.

1. *Mr A. G. Miller* showed a young man whose KNEE-JOINT he had excised three years before. The result had been a very successful one, the operation having been done for synovial disease.

2. *Mr F. M. Caird* showed a young man suffering from PARALYSIS OF THE RIGHT SERRATUS MAGNUS. There did not seem to be any special cause for it, and the patient himself said it came on with pain in the shoulder after exposure while driving. The scapula projected in the back whenever he pushed forward against an obstacle. The digitations of the muscle were hardly visible.

II. EXHIBITION OF PATHOLOGICAL SPECIMEN.

Dr Argyll Robertson showed a DARK CATARACT removed from a woman about 65 years of age. Before operation the perception of light was too small to be accounted for by the condition of the lens. It was evident that the interior of the eye was not healthy, and there was found extensive atrophy of the optic nerve and pigmentary degeneration of the choroid. The other eye was operated on afterwards, and was in the same condition. On the whole, the result in both instances was satisfactory. Her vision, which was so bad that she required to be led about, was so far re-established that she was now able to guide herself with ease and to read good large print.

III. Original Communications.

1. *Professor Chiene* read his paper, entitled HOSPITAL EXPERIENCES, PART I., which appeared at p. 43 of this Journal.

Dr William Taylor said Professor Chiene had started several questions which were well worthy of consideration. There was the question how the two cases of inflammation of the lungs got the inflammation,—whether it was due to the severity of the injuries or to over-ventilation, which in hospitals might be inimical to the recovery of persons with weak chests. Patients sometimes complained very much of draughts, and it was admitted that phthisical patients did not get on in the general wards, and any one with latent chest weakness would not benefit by excessive ventilation. The excess of antiseptics was another question of importance. One or two cases of deaths in private practice must be attributed to some such cause—the chilling effect of the cold spray and absorption of carbolic in wounds with large surfaces.

Dr Allan Jamieson, in speaking of the cases in which pneumonia occurred after burns, said that where there was extensive congestion of the skin from exudation or psoriasis or exfoliative dermatitis, the patients were peculiarly sensitive to cold. The regulative function of the skin seemed to be disturbed, and there was a shrinking from a cold atmosphere which was quite remarkable. The blood was retained in the skin to an abnormal extent, and the evaporation from the surface could not be lessened or increased as under normal conditions. The action of slight cold in such sensitive conditions acting on a small surface of body might cause congestion of internal organs. In the City Hospital they had noticed that erysipelas and inflammation of the lungs seemed sometimes to run hand in hand. Cases of the latter were sometimes sent in by mistake as cases of continued fever, and frequently at the same time an increase in the numbers of erysipelas cases was noticed.

Dr Ireland, in remarking on Mr Chiene's classification of the causes of death, said he did not think there was ever one cause for any single death. Probably some of the patients died from a combination of all the five causes.

Mr F. M. Caird thought a record of Mr Chiene's own experience would have been more instructive than one in which he dealt with the cases of the assistant-surgeons as well as his own. As to the cases of death during anæsthesia, he did not think that Mr Chiene had been present at any one of them. In the case of carbolic acid poisoning it would have been instructive to have had the signs and symptoms given on which the diagnosis was founded. He recollected Dr Angus Macdonald saying that the deaths which in his earlier practice he ascribed to the antiseptic he would in his later years put down to septicæmia.

Mr Miller remarked on Professor Chiene having given up carbolized cutgut because of its being antiseptically unreliable. He thought that for this the tradesman was more to blame than the surgeon. Cutgut required a long time to be properly prepared, and when there was a great demand for it they were sometimes supplied with what was literally shoddy catgut. The first case that drew his attention to this was one of excision of the mamma, in which suppuration took place at the points of sutures only. The same thing occurred in cases which followed, and did not cease till the catgut was given up.

Mr Cotterill said, with regard to septic pneumonia in connexion with burns and putrid wounds, he was much struck with what he saw a few weeks ago in a small ward in the Infirmary. There were in it three cases of removal of the jaw and tongue. One of these took erysipelas, and two days after the other two had septic pneumonia. The ward was thoroughly disinfected. He (Mr C.) then came on duty. One case took septic pneumonia after excision of the tongue. Another did the same after removal of an axillary tumour, and he learned subsequently that two friends of the man who had undergone excision of the tongue took pneumonia after visiting him. As to the death after operation for harelip, he had a case in which death occurred from suffocation. The child died during the night when left by the mother for a few minutes. On looking up the subject there were other cases on record of the same thing. The purification of the skin, to which Mr Chiene attached so much importance, was a matter of great difficulty, and its impurities were a source of failure and danger. He did not know what to do to remedy this. Mr Edington, he observed, despaired of cleaning instruments except by boiling them for an hour or soaking them in carbolic lotion for twenty-four hours. This they could hardly do with their hands.

Mr Miller considered that there was a difference between hands and instruments, inasmuch as the skin absorbed antiseptic solutions and instruments did not. He recommended a combination of carbolic acid and glycerine as a useful disinfectant.

Mr Cotterill said the question was not whether the skin absorbed the antiseptic, but whether the antiseptic killed the germs.

Dr Allan Jamieson observed that the skin was covered by a fine horny layer, and the great point was to keep this horny layer very smooth. Both carbolic acid and corrosive sublimate lotions broke up this layer. An overfatted soap, such as that recommended by Unna, kept the horny layer smooth better than anything else. The original soap made by Douglas of Bremen was still the best, and the smoother the layer the less likely were germs to find spots where they could lie.

Dr James Ritchie, in the course of a few remarks, referred to the difficulties surgeons had to contend with in hospital, from the fact that many of the students who assisted at operations were at the same time working in the dissecting-room. He remembered the very perfunctory manner in which they washed their hands before going to the Infirmary in those days. He also asked if the case which exhibited acute bronchitis after the administration of ether had been exposed in any way after the operation.

Prof. Chiene, in replying, said he hoped to continue these experiences, and give the Society the other side. The patient who succumbed to double bronchitis was not specially exposed to cold. He could not agree with Mr Caird. The experiences he desired to give were those of an hospital ward. He did not think the harelip case died from the cause suggested by Mr Cotterill.

2. *Mr Miller* read his paper on EXCISION OF THE KNEE-JOINT, which appeared at p. 41 of this Journal.

Prof. Chiene thought Mr Miller's idea and plan of systematic removal of the synovial membrane thoroughly good and practical, and would give it a fair trial. He did not attach so much importance to the removal of the piece of skin to prevent redundancy.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION L.—MEETING VIII.

Wednesday, 24th July 1889.—Dr UNDERHILL, *President, in the Chair.*

I. *Dr Brewis* showed—(1.) UTERINE APPENDAGES, presenting the condition of HÆMATO SALPINX, which he removed from a patient who had been sterile and had suffered from severe pelvic pain for fourteen years. The uterus was retroflexed, and the ovaries, to which the disturbed tubes were firmly attached, were embedded in adhesions low down in the pelvis. The blood contained in the tubes was in a fluid state. (2.) A PAIR of greatly thickened FALLOPIAN TUBES presenting the characters of CHRONIC SALPINGITIS removed from a patient who had suffered pain for sixteen years.

II. *Dr W. Fraser Wright* read his paper entitled CASE OF HEART DISEASE COMPLICATING LABOUR TREATED SUCCESSFULLY BY NITRITE OF AMYL, which will appear in a future number of this Journal.

Dr Berry Hart thought Dr Fraser Wright's paper contained a very valuable suggestion. So far as Dr Hart knew, the treatment was original and well worthy of repeated trial.

Dr J. W. Ballantyne had not had any experience with nitrite of amyl after the expulsion of the placenta in cardiac cases, but in one case he had used it for the purpose of relieving severe anginal attacks in a case of mitral disease complicating pregnancy. In this case the nitrite of amyl seemed to make the heart pain worse, and if Dr Fraser Wright's theory be correct, one need not be surprised at this result, for, during pregnancy in mitral disease any dilatation of the arterioles would most probably increase the painful heart contractions, whereas, in cardiac cases, after the expulsion of the placenta the nitrite of amyl would, by dilating the arterioles, relieve the distended right heart. The fact that he, Dr Ballantyne, had not found relief to follow the use of nitrite of amyl in pregnancy would, therefore, go to support the procedure proposed by Dr Wright, and to show to some extent the *modus operandi* of the drug in the post-partum condition.

Dr W. Fordyce asked Dr Wright if, in a similar case, he would give the nitrite of amyl at the same time, *i.e.*, after the placenta was born, and if he thought any good might result by giving it sooner, either before the placenta was born or immediately afterwards; for it has been pointed out that the danger to patient lies in the sudden engorgement of the right side of heart after the placenta is born. By causing dilatation of the arterioles before the placenta was expelled, the pressure in the right side of the heart, which must in mitral stenosis be above normal, would be lessened, and when placenta was expelled, and the extra amount of blood entered the left ventricle, it would be more able to withstand the sudden engorgement, as the tension had been previously lessened owing to increase of blood by the dilatation of the systemic arterioles. Also suggested a difficulty in explanation of *immediate* relief to patient by the dilatation of the arterioles—in fact, that before the blood from right side of heart reached the systemic circulation, it had to pass through the already engorged lungs and a stenosed mitral orifice.

The President congratulated Dr Wright on his prompt and successful treatment of this very serious and anxious case. Those who had watched cases where labour was complicated by severe heart disease knew well how immediate was the danger and how trying was the anxiety. He thought the use of nitrite of amyl in this case was a just deduction from a scientific knowledge of the properties of the drug. In trying to find an explanation for its instantaneous good effects, we must not overlook the fact that the blood was a continuous whole, and that when an extra quantity of it was let off, as it were, into the systemic arterioles, the effect was to diminish greatly the tension in the whole system. The tension of the right ventricle was diminished equally with that of the rest of the circulation, and the almost paralyzed heart was thus enabled to act again with advantage. In the cases of this kind he had seen the danger was very great, but it also seemed to be soon over.

Dr Fraser Wright, in reply, said he would like to point out, that in Dr Hart's case the extreme pallor of the face which preceded death indicated contraction of the peripheral system, and nitrite of amyl would very likely have given great relief. It was unnecessary to anticipate the difficulty or danger, as Dr Fordyce suggests, as it was time to use the drug when danger threatened, and that was when the placenta had been expelled. He had to return thanks for the kindly way in which the communication had been received, and hoped it would lead to a trial of this remedy in suitable cases.

III. *The President* read his paper on SOME CASES OF INTRA-UTERINE HYDROCEPHALUS, with remarks, which appears at page 218 of this Journal.

Dr Freeland Barbour referred to a case recorded by Benckiser, which shows that hydrocephalus may cause rupture of the uterus even during the first stage of labour. This case was of interest in relation to the thinning of the lower segment, which takes place even in normal labour; and it was of practical importance to remember that in hydrocephalus this thinning might go into rupture before the cervix was dilated.

The President, in replying, stated that the two points he specially wished to emphasize in the paper were,—firstly, the importance of the history of the case in suggesting the diagnosis of hydrocephalus; and, secondly, the risks incurred in dragging through the pelvis a head which was even moderately distended instead of proceeding at once to puncture. These risks were the damage liable to be done to the soft parts, vaginal wall, bladder, etc., and also to the joints of the pelvis.

IV. *Dr Allan Sym* read the following remarks on INJURY TO FŒTAL NECK THROUGH PRESSURE OF UMBILICAL CORD:—Mrs R. was delivered by me on the 29th May. The labour was tedious, and forceps had to be applied with the head in transverse position, the blades being on the occiput and sinciput. When the child's head was born it was noticed that the cord was tight round the child's neck. My patient had severe post-partum hæmorrhage, which prevented me from paying any attention to the child beyond seeing that it respired satisfactorily.

Next day my attention was directed by the nurse to the child's neck, and I found an ulcerated surface on the right side about the length and breadth of my little finger, and tailing away at each end into a well-marked groove, which extended round as far as the border of the trachea in front. The ulcerated surface was sloughy in character, and before I got it healed several small sloughs came away. It is interesting to note how much pressure had been exerted by the cord in the neck so much as to cause destruction of tissue without injuring the life of the child. One would have expected under the circumstances that the child would have been still born, but on the contrary he is a very healthy boy. I think no apology is necessary to the Society for bringing before it such a peculiar and uncommon case.

V. *Dr Allan Sym* read also the following note on a case of ANNULAR HYMEN:—On the 22nd of last month Mr B. called on me, and explained that he had been married three days previously, and had come to Edinburgh the same day, and resided in one of the Princes Street hotels. He complained that he had been unable to consummate the marriage on account of some obstruction at the orifice of the vagina, and requested me to see his wife. I called next morning, and saw Mrs B., and on examination found a very thick, tough hymen, with an annular aperture that would hardly admit of the passage of a pea. As the parts were very tender I applied a solution of cocaine, and then by the aid of a pair of scissors and digital pressure I succeeded in breaking down the hymen. The hæmorrhage was slight, and was easily controlled by a plug of absorbent wool. Mr and Mrs B. left town on the following day, but in a note which came from them recently I was informed that everything was satisfactory. Mrs B. was quite young, somewhere about 25, and her husband some ten years older. The hymen was extremely tough and thick, and I was not in the least surprised that it had not been ruptured.

This case I have reported on account of its uncommon nature. A year or two ago Dr Halliday Croom reported a case in which he had to catheterize a patient for reflex irritation after rupture of the hymen, but the cases where medical assistance is required to cause the rupture are, I think, of rare occurrence.

Dr Milne Murray stated that two years ago he saw an almost identical case under similar circumstances. Dr Brewis assisted him by giving chloroform, and he caught the thick edge with catch forceps, nicked it in two places, and tore the hymen freely with the fingers. The condition gave no further trouble.

Dr Ballantyne had seen a case similar to that reported by Dr Sym, and under very similar circumstances; and the *President* made some remarks.

ROYAL MEDICAL SOCIETY.

June 27.—*H. H. Littlejohn, M.B.*, read a communication on Death from Overlaying. Among the most constant post-mortem signs are: externally, well-marked lividity of the whole body, but especially of the lips; internally, the presence of small ecchymoses over the surface of the heart, lungs, and thymus gland. Those on the heart are often very large, and are situated chiefly on the posterior surface. Unfortunately there is nothing absolutely diagnostic in the post-mortem appearances, and thus it is difficult to bring parents to justice when they put forward the plea that the child died of convulsions. Some pathological specimens were exhibited from a case of overlaying in a new-born infant.

A. L. Turner communicated a case of severe Psoriasis and the various therapeutic agents, and their effects on its course. Photographs were shown from this case, and also from another of Purpura.

T. Watts Eden, M.B., related the case of a labourer, aged 26, who was admitted to the Royal Infirmary, under the care of Professor Fraser, on 3rd April 1889. For six months previously he had suffered from cough and shortness of breath on exertion, and he attributed his illness to an ordinary cold. During almost the whole of this time he had been unable to work, had been getting steadily worse, and latterly losing flesh. Occasionally he had noticed streaks of blood in the sputa, and he sometimes had a little pain in the right mammary region. The history seemed suggestive of phthisis, but this suspicion was not confirmed. The physical signs were somewhat puzzling, and the diagnosis consequently uncertain. In the upper part of the thorax was an area of impaired percussion note, extending over the inner half of the first and second right interspaces, and passing over to the left half an inch beyond the left border of the sternum. In this area there was somewhat indistinct bronchial breathing and bronchophony. In the right base and lower axillary region percussion was impaired, and breath sounds and vocal resonance were indistinct. The heart was slightly displaced to the left, and the vertical diameter of the liver was 7 inches in the right mammary line. The superficial veins of the neck and trunk were somewhat distended, particularly upon the right side. At this time the presence of a mediastinal tumour and possibly a little pleural effusion were suspected. The evening temperature was often above 100° F., and the pulse-rate was rapid, being invariably above 100. There was a good deal of distressing cough, with purulent sputa and a little hæmoptysis. These conditions remained unaltered until on 10th May a large right hydrothorax was found to have accumulated, the fluid extending to the third rib in front and to the spine of the scapula behind. Aspiration was twice performed—on the first occasion 10 oz., and on the second 30 oz. of clear fluid being withdrawn. After the second aspiration the physical signs cleared up very much, but over the right base there remained some dulness with indistinct breath sounds. At this time the patient suffered from attacks of violent coughing, which generally needed to be checked by morphia. There also developed considerable œdema of the trunk and of the right arm, and the superficial veins became more prominent. The glands in the right axilla became painful, and were found to be enlarged and hard, but not tender. There was also some continual dyspnœa, which obliged the patient to lie chiefly on the right side. The physical signs yielded by the right side of the chest now were very puzzling. Areas of hyper-resonance developed in the upper part in front and behind, only to disappear in a short time, and be succeeded by a degree of dulness. The lower part of the chest remained more or less in the condition mentioned above. On 17th June an extensive pneumo-thorax was found extending from the third rib in front and the sixth behind to the extreme margin of the thorax. The bell sound was readily obtained, but there was no metallic tinkling or hippocratic succussion. It was attended with extreme dyspnœa; the right side of the chest showed almost no movement, while the movements of the left side were greatly exaggerated. The aspirator was employed on 19th June, with the view of inducing some expansion of the lung. The needle was introduced into the eighth intercostal space, which was tympanitic to percussion, and pus began to flow freely at once, and continued until 27 oz. had been withdrawn. This relieved the dyspnœa very greatly, but the physical signs remained almost unaltered. On 21st June the patient died suddenly during an attack of coughing, without expectoration of either blood or pus. On post-mortem examination, the right pleura was found very greatly thickened, and 95 oz. of pus were taken from its cavity. The apex of the lung was very firmly adherent to the chest-wall. The lung itself was collapsed, and compressed against the spinal column. About 1½ inch from its lower margin was a perforation admitting a No. 12 bougie, and communicating with a large bronchus. On section of the lung, a mass of scirrhus was found lying to the right of the lower part of the trachea, and extending along the right bronchus through the whole of its extent and the left bronchus in its commencement. The mucous membrane was extensively ulcerated. There was a good deal of pus in the right lung; the left lung was congested and emphysematous. The tumour compressed slightly the superior vena cava. There were a few enlarged and indurated glands in the anterior mediastinum.

July 4.—*A. L. Gillespie, M.B.*, communicated a case of Tubercular Meningitis. A child of 13 months was admitted to the Sick Children's Hospital with complaint of bronchitis on 4th June 1889. There was nothing particularly noticeable at this time save great enlargement of the liver. Patient, however, lost ground rapidly, going down 3 lbs. in weight in a fortnight, and having a high temperature every evening. *June* 18.—In the evening had fits repeatedly, the left side being especially affected with clonic spasms, and died at 2 A.M. on the following morning in a convulsion. *Post-mortem Examination.*—General ricketty condition. Liver and lungs showed the presence of a few tubercles. In the brain were found marked congestion of vessels all over the right hemisphere, and one patch of capillary congestion over the left superior parietal convolution. Over right superior temporo-sphenoidal convolution was a small collection of pus running along the veins issuing from the Sylvian fissure. The sides of the fissure were more adherent than normal, and on separation a few tubercular granulations were to be seen, but nowhere else. There was excess of fluid both inside and outside the brain.

R. Muir, M.B., communicated a case of Malignant Jaundice.

July 11.—*H. H. Littlejohn, M.B.*, showed a specimen of Interstitial Emphysema in the lung of a child that had died of diarrhœa and vomiting.

S. W. Carruthers communicated a case of Eczema-Lupus in a child, and showed photographs.

A. L. Gillespie, M.B., communicated the case of a boy, aged 13½, admitted to the Sick Children's Hospital, with a staggering gait. For a month previously his ear had been running; but two days before admission, the weather being very hot, he had exerted himself considerably, the discharge had ceased, and pain in the frontal and temporal regions commenced, with giddiness and staggering, especially towards the left and backwards. On admission,.pulse was 132; respirations, 40; temperature, 102°·8 F. There were no special physical signs in the chest, although a slight catching pain was felt in the right side. There was no pain in the head, only a feeling of dizziness. Subsultus tendinum was noticed; the patellar reflex absent. The right pupil was larger than the left, but both were responsive. There was slight nystagmus on looking up. No discharge from the ear, and hearing normal. The temperature rose shortly to 104°·2 F., and patient became delirious. Three days after admission signs of dulness developed at the right apex, and spread gradually downwards. The pneumonia subsequently resolved in an ordinary way, but the staggering still continued.

Part Fourth.

PERISCOPE.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. Allan Jamieson, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

Confluent Warts affecting both Legs.—Gémy relates a case in which a native of Morocco, aged 23, had both legs, from the root of the great toe to the middle of the leg, literally covered with warts of a variable size. The smallest were of the size of a lentil, the largest as large as a shilling. The toes, sole, and ankles, those parts covered by the Arab shoe, were free, and the warts ceased where the trousers began. Close inspection revealed a mother wart of large dimensions, having a number of satellites—ranged behind like the tail of a comet—of warts, becoming gradually smaller and smaller. The direction of the tail was different in each case, thus imparting the appearance of disorder. According to the patient, the warts had been present for a year, but this was probably much within the mark. The application of a plaster of salicylic acid—kept in place by a bandage, and renewed weekly—caused their complete disappearance in a couple of months. The skin was naturally brown, but the situations which had been occupied by the warts had assumed a white tint without any cicatrix.—*Annales de dermatologie et de syphiligraphie*, No. 2, 1889.

Purpura and Malignant Growths.—Dr Thomas Harris has collected a series of cases in which hæmorrhage into the skin and other organs was found in association with sarcomatous growths, in the majority of which the course of the disease was a very rapid one. He endeavours to prove that the purpura met with was due to emboli lodging in the small vessels, and asks the question, Is it not quite possible that emboli, composed of sarcoma cells, may lodge in vessels, and yet no secondary growths ensue?—*The Medical Chronicle*, February 1889.

Bromide of Potassium in Ovarian Acne.—Dr Arthur Jamison directs attention to the value of potassium bromide in acne coincident with, if not caused by, ovarian irritation and its attendant menorrhagia. In a case related at length, the patient was a young woman of 22, who had had acne for four years. She complained of debility and profuse menstruation, the flow recurring every fortnight, one period having scarcely ceased when another came on. There was pain and tenderness over the right ovary, the pain beginning about two days before the period; and there was much backache. Twenty grains of bromide in infusion of gentian was prescribed, to be taken three times a day; and when this had been continued a month her face was already much better. The medicine has been continued steadily during and for a few days before and after each period; and the acne has been kept in check, while the other symptoms were relieved. In one similar case the bromide rash was produced, but this could easily be differentiated from the acne. In all the cases local treatment had been unsuccessful.—*Practitioner*, May 1889.

The Anatomy of Scabies.—Török combats the idea which is held, that the acarus scabiei forms its burrow in the prickle cell layer of the epidermis. Were it so, from the succulence of the part, the acarus would be drowned out, and fluid could be squeezed on pressure from the openings of the burrows. Again, this portion of the rete is so provided with nerves that itching would be constant, not chiefly nocturnal and intermittent. He has made sections of the burrows in some cases, and concludes:—1. That the burrows are not in the prickle cell layer, but in the lowest part of the middle horny layer. 2. That the eczema in scabies is not one evoked by scratching, but from histological considerations is clearly due to the acarus, and is caused by an irritating substance secreted by the insect itself. The results he obtained were uniform in all the seven cases, but he thinks that further observations must be made to show if his conclusions hold good in general. —*Monatshefte für praktische Dermatologie*, No. 8, 1889.

Psoriasis and Eczema Seborrhœicum.—Since Unna introduced the term eczema seborrhœicum into cutaneous medicine, much discussion has arisen as to the relationship between this condition and other well-marked diseases. At the New York Dermatological Society Dr Fox presented a case of psoriasis, located on the scalp, extremities, scrotum, and scrotal fold, penis, in both axillæ, and between the toes. The disease had lasted for twenty years, off and on, and had always been dry. It did not itch, except on the scrotum. Some of the lesions were typical of psoriasis, and some were more yellowish and crusted than is usual in that disease. In the discussion Dr Bronson believed the case was one of a group which had puzzled him, leading him to adopt the theory that psoriasis was not always an independent form of disease, but was a condition which might complicate or modify other cutaneous affections. Usually a person with psoriasis had the disease for life, though the tendency might be first manifested at an advanced period. In the case under discussion, psoriasis was secondarily associated with eczema. At some points, as in the axillæ, the trouble appeared to be solely eczema; in others the two diseases coexisted. In a case he cited the psoriasis seemed to have been superinduced by syphilis. Dr Fox, in closing the discussion, said he thought the term eczema seborrhœicum had led to more confusion than it had helped to a better understanding of the disease. We used to believe that there were three diseases:—1, eczema, which was moist; 2, psoriasis, which was dry and scaly, and tended to recur; and, 3, a disease that occurred on the sternum and also on the scalp, in the form of circles, which were scaly. This latter was called seborrhœa, not perhaps a good name, but he doubted if that of eczema seborrhœicum was any better. He believed that the present case was one of psoriasis, with an eczematous tendency. The eczema accounted for the crusting. It is important to recognise the fact that psoriasis does not always have white silvery scales. When it occurs in the axillæ or in the pubic region, it naturally undergoes changes in the same way that eczema has different appearances in particular localities; and the recognition of such modifications being liable to occur should aid us in our diagnosis. Dr Bronson suggested a mycotic element as causing this modification.—*Journal of Cutaneous and Genito-Urinary Diseases*, March 1889.

The Relations of the Seborrhœic Processes to some other Affections of the Skin.—Dr Brooke, of Manchester, observes that when on a visit to Dr Unna's clinique he was struck by the number of cases which presented the distinctive features of seborrhœa; and since this visit, three years ago, he has been more than ever confirmed in the opinion of its wide prevalence and its extensive influence on other affections of the skin. Though most common on the scalp, it may occur on other parts of the body, and the main features in all situations is the greasy exudation. This may be copious or scanty, either somewhat fluid or more solid in nature. At times it predominates over the desquamated epithelium, and then forms either a fatty cake or a varnish-like layer; or the epithelium may predominate, and the presence of greasy scurf, loose or agglomerated, is the prevailing feature. As Unna insists, it is from the scalp that seborrhœic affections of the body almost invariably originate. Another and different form of the affection is, in Brooke's opinion, when there is on the sternum and between the shoulders, often only there, an outbreak of papular, nummular, or circinate patches, or gyrate lines made up of a connected series of fragments of circles more or less covered with dull-coloured, greasy scales. Should the scales be removed, the surface of freshly-growing patches, which is always raised, is of a bright, active red colour. The circles are often incomplete, and itching is present. The condition is apparently a dermatitis caused by the presence of one, or possibly several, micro-organisms, and leading to a specific irritation of the fat-forming functions of the skin. It is not surprising that the individual micro-organism causing it has not been isolated, when Taenzer has already found about eighty varieties of bacteria and fungi from the scales and secretions of eczema seborrhœicum! Unna regards all the symptoms of the complaint just described as forming one variety of his eczema seborrhœicum. This is not yet conceded, though the existence of typical eczema in connexion with seborrhœa is recognised, and this combined form constitutes, perhaps, the commonest type of eczema. Sometimes the extension of the seborrhœa follows on that of the eczema. Seborrhœa may again determine the localization of an attack of acute eczema. When a patch of eczema has once become seborrhœic, it may remain for years in the same condition, even in spite of very varied treatment. In no instance, however, is the influence of seborrhœa more marked than in connexion with rosacea. Whether it is, as Unna remarks, the most frequent cause of this affection Brooke is not able definitely to state, since the active manifestations are so frequently introduced by gastric and uterine irritation; but observation of a large number of rosacea patients has shown him that seborrhœa of the scalp and face is present in nearly every case, and it is certain that the simple removal of the seborrhœa by purely local means will go far not only towards removing the desquamation and pustulation, but also to allaying the active flushing and chronic hyperæmia.—*British Journal of Dermatology,* June 1889.

Leprosy.—The subject of leprosy is at present attracting much interest, and some extracts from the *Report of the Trinidad Leper Asylum* for 1888, by Dr Beavan Rake, which has just reached us, may be instructive. Referring to the case of the convict Keanu, inoculated with leprous material by Arning in 1885, he writes that, though the man after three years was found to be leprous, an experiment of this kind, to be scientifically perfect, must be performed in a country free from leprosy, and on an individual who has never left that country, and whose immediate ancestors have always stayed at home. Even if we admit the worst, viz., that the disease is directly inoculable beneath the skin in some cases, it is no argument that it is contagious in the ordinary sense of the term, like typhus fever or small-pox. He has lately tried creolin a good deal. In five per cent. solution he finds it rapidly heals the eczema so often complicating leprosy. It is also an excellent stimulant for indolent leprous ulcers. In cases of gangrene it completely reduces the smell. After excision of leprous tubercles he finds pure creolin useful as a caustic to hinder recurrence. After various experiments on living animals with inoculations of the bacillus lepræ, he can so far record no growth when planted in animal tissue. Arning has stated that when leprous tissue was laid aside, and the growth of the larger fungi excluded, the bacillus lepræ held its own against other micro-organisms; and further, that it was met with so abundantly and so laden with spores as to suggest actual increase. The bacillus was also found in large numbers on examining the corpse of a tuberculated leper, which had been buried for nearly three months. Arning acknowledged that he could not definitely determine whether these bacilli were still alive and capable of reproducing the disease, but from their microscopical appearance he felt confident that such was the case. The question is one of immense importance with reference to the public health. If it is possible for the bacillus to multiply in the bodies of dead lepers, it would no doubt be safer that all such bodies should be burned, even though it is not yet actually proved that the bacillus is the cause of leprosy. Rake examined this question, but found no evidence that the bacillus lepræ can resist putrefaction. The putrefactive bacteria seemed to have it all their own way; and he failed to find any bacilli which stained well with magenta. He concludes that he cannot agree with Dr Arning as to the power of leprosy bacilli in resisting putrefaction, much less have his observations convinced him that they have any power of germinating under such circumstances.

GONORRHŒAL ERYTHEMA.—Most medical men still ascribe to the absorption of the classic drugs for the treatment of gonorrhœa, copaiba and cubebs, those eruptions which appear in the course of this disease. According to Molenes this opinion is obsolete. True erythemas due to these medicines may occur, but are rare; and it is the gonococcus of Neisser which is the active agent. How otherwise can we explain the fact, that in affections other than gonorrhœa, in which copaiba or cubebs are prescribed, erythema is so extremely rare? Again, Besnier has shown that, contrary to expectation, in instances of gonorrhœa in which an erythema shows itself during the exhibition of these drugs, if their administration be continued, the rash, instead of extending or persisting, declines more or less rapidly. Molenes relates a case in which an eruption presenting all the features of erythema, such as is called copaiba or cubeb rash, in form, extent, course, and duration appeared, and yet the patient never took any internal medicine capable of inducing such an eruption. When it was fading he administered a sedative mixture containing cubebs and copaiba, and though the dose was large, the eruption continued to grow paler and to disappear, and did not return, although the medicine was persisted in for fifteen days.—*Annales de dermatologie et de syphiligraphie,* No. 2, 1889.

A Minimum Cauterizer.—The finest cautery, either Paquelin's or an electric one, causes an eschar three or four times its diameter, and though this is not of much consequence when diseases which of themselves leave a scar are being dealt with, it is otherwise when a rosacea or a capillary nævus is so treated. Taenzer has set himself to discover a plan by which a cautery causing a much smaller eschar might be made, and has designed one on the following principles:—A finely-pointed copper needle, set in an isolated holder, is surrounded by a platinum spiral, which does not quite come in contact with the needle, and does not extend to its point. When the spiral is connected with both poles of the battery it becomes red hot, but it does not raise the temperature of the included needle so high. The copper needle does not become red hot, merely very warm. In addition, the diameter of the included needle is much less than in any other form of electric cautery. These two combined factors, the small diameter of the cautery and the low temperature, render it possible to produce such a small eschar, that it is less than the head of the smallest pin, and the scar which remains is invisible without a lens. With this instrument one can fearlessly cauterize wide areas, as, for example, port-wine marks on the face; but care must be taken not to crowd the eschars too closely together. The needle must, however, be brought to the proper heat before cauterization is begun, and this can best be estimated if the needle can be easily withdrawn from the skin. If the needle is not hot enough, it can only be extracted with pain and difficulty. The instrument is made by Bauer & Häselbarth, instrument makers, Eimsbüttel, Hamburg.—*Monatshefte für praktische Dermatologie,* No. 9, 1889.

Acne Varioliformis.—Cæsar Boeck has made a study of this rare and peculiar disease in a well-marked case, and has pointed out some hitherto unnoticed features. The complaint has its special seat on the forehead, hence has been called acne frontalis; but it has been seen on the back, hence this term is too restrictive. Boeck describes the evolution of the disease thus:—The minute papules which form the first stage are produced by the enlargement of a hair follicle and its immediate neighbourhood. This small papule gradually increases in size, and when it has reached that of a grain of hemp there can be seen in its centre round the follicle a number of very small hæmorrhagic puncta, which impart the aspect of a small, ill-defined, violet macule. This in the smaller papules disappears when the tension again subsides; but when the size reaches that of a pea, and includes several follicles, it persists, and produces a swollen, elevated, tolerably firm area of a punctated appearance and bright violet hue. Next, the affected part sinks in the centre, and assumes a brownish violet tint, which spreads over the entire surface. Then the entire diseased part sinks down to or beneath the level of the neighbouring sound skin, and lies embedded in it as a sharply-defined, circular, hard disc of a purplish-brown, or finally of a brownish hue. If one endeavours to raise the mummified mass into which the epidermis and the greater part of the corium have been transformed by means of pincers, it is found to be extremely tough and coherent. The disc dries up from the margins; and when at length it is detached, a cupped cicatrix persists. The process is always accompanied by necrosis of a part of the cutis. Boeck points out that it cannot be called acne atrophica nor lupoid acne, as it bears no relation either to atrophy or lupus. The necrosis is that form called mummification. The crust thrown off in such circumstances retains many of its anatomical features unchanged, and in the absence of any portions of affected tissue, Boeck has made careful sections of these crusts, after removing the fat with which they are abundantly impregnated by means of alcohol or ether. He found hyperplasia of the epidermis. The vessels of the papillæ were dilated, convoluted, and distended with blood, while there were many small hæmorrhages, and the deeper vessels of the cutis were in like manner enlarged and surrounded with a peculiar hyaline substance. Various micro-organisms were found, but it was impossible to say whether these bore any part in causing the disease, or had come accidentally from without. As there are no previous descriptions of any microscopical examination in this disease, these facts, though meagre, are of value.—*Archiv für Dermatologie und Syphilis*, 1 Heft, 1889.

OCCASIONAL PERISCOPE OF THE DISEASES OF CHILDREN.

By CHARLES E. UNDERHILL, M.B., F.R.C.P.E., Physician to the Royal Hospital for Sick Children.

ACUTE LOBAR PNEUMONIA IN CHILDREN.—This disease is much commoner in infancy than is generally believed. It is usually an acute primary disorder, and differs entirely from broncho-pneumonia, or catarrhal pneumonia, which is always secondary to bronchitis, and is due to an extension of the inflammatory process. Acute lobar pneumonia is frequently overlooked in infancy and childhood, because it seldom presents all the cardinal symptoms which mark it in adult life. The onset is acute, but instead of a rigor, it generally sets in with vomiting or a convulsion. The pain in the affected side, so common in adults, is frequently wanting in children, or the pain, if present, is referred to some other region, often to the abdomen. Cough is generally present, but often very slight; as a rule, it becomes more frequent as the termination of the disease approaches. The viscid, rusty expectoration, so characteristic of the disease in adults, is very rarely seen—hardly ever in children under eight years of age. The temperature always runs high, and the case generally terminates by crisis about the seventh or eighth day. This is by no means an invariable rule. The crisis, in a small minority of cases, varies from the third to the thirteenth day. In about one-sixth of the cases the temperature falls gradually. In a few cases the physical signs, which usually disappear within a week after the crisis, continue for several weeks, and the lung clears up very gradually. They can rarely be made out at the outset of the disease, and are frequently not found until the third day; in some cases they cannot be made out at all, the patch of disease being central. Respirations are always rapid, never under 40 per minute. The pulse is also much quickened, but not in proportion to the rate of respiration. The alæ nasi dilate. Cerebral symptoms are very frequent, and are not confined to cases where the apex is affected. The prognosis is decidedly favourable; Ashby says the percentage of deaths is probably smaller than in any other acute disease which attacks children. This does not hold good in very young infants, especially the newly-born, in whom the disease is very fatal. A table of 1138 cases, collected from various sources, is given, of whom only 28, or about 2 per cent., died. Broncho-pneumonia, on the other hand, is a far more dangerous and fatal disorder. Little treatment is required: all active and debilitating treatment should be avoided, the natural tendency of the disease being towards recovery.—C. W. Townsend, M.D., Boston., *Arch. of Pædiatrics*, March and April 1889.

Stomach Washing in Infants.—Since this mechanical treatment of gastric troubles in infancy and childhood was introduced by Epstein in 1880, it has been carried out systematically by but few physicians. These few, however, speak most highly of it. Over 5000 children have now been treated in this manner in Epstein's clinique with very gratifying results. Siebert has practised it largely in New York, and recommends it in three different conditions—(1.) The dyspepsia of infants, with regurgitation of food after each meal. These are generally cured by a single washing. (2.) In acute gastric intestinal catarrh or cholera infantum. If the cases are got hold of early enough, the results are most satisfactory. Instead of depressing the children, the treatment seemed to have a stimulating influence. (3.) In cases of chronic catarrh of the alimentary tract. These require more frequent washings. All the cases of this series were treated with complete success, though some were very severe and of long standing. The principle of this treatment is that thus the poisons formed in the stomach by retained masses of curd or other food are mechanically diluted and washed out of the body. Only plain warm water without any medicaments is used. The washing is continued, using small quantities of water at a time, until it returns quite clear. *Modus operandi.*—A soft, "velvet-eyed" catheter, with tubing attached, is introduced into the stomach. The tube is then attached to the pipe of a glass douche case; about a teacupful of warm water is then passed into the stomach, the tube detached, and the water allowed to run out again, the process being repeated as often as necessary, In chronic cases a few small doses of calomel are given, subsequently followed by an enema. Plain barley water is the only food given for twenty-four hours, and then a gradual return to milk is allowed.—*Arch. of Pœdiat.*, April 1889.

THE TREATMENT OF WHOOPING-COUGH WITH QUININE, ESPECIALLY BY SUBCUTANEOUS INJECTION.—Whooping-cough is so fatal a disease in young children that a whole pharmacopœia of drugs has been tried and discarded in its treatment. Quinine was first suggested in 1868 by Binz, on the strength of its strong antiseptic powers, and on the assumption that the disease has a parasitic origin. Fervers has collated all that has been written, a goodly number of papers, on this subject, and briefly recapitulated them. Most authors have found some benefit, a few others, of whom Henoch is the best known, reject it as useless. Binz believes it of much value, but it must be given in large doses and continued for a considerable time. It has been given by the mouth, by the rectum, by the spray, and by subcutaneous injection. In this paper some experiences of the last mentioned mode of using quinine are related, and the author claims that he has met with much success; but while some of the cases related seem to have benefited promptly and permanently, in others there was little definite improvement, and in several there was a relapse. Nor were the injections without their drawbacks. In many cases the skin at the points of puncture was indurated and inflamed, and in several of them there was some sloughing. The preparation used, on account of its solubility, was the carbamide (a combination of hydrochlorate of quinine with chemically pure urea); about three grains were injected once or twice a day. This treatment was carried out under the direction of Prof. Ungar of Bonn in his Polyklinik. The final advice is that this method of administering the drug should be reserved for those cases in which it cannot be taken in any other way. A complete bibliography of the use of quinine in whooping-cough closes the paper.—*Jahr. f. Kinderheilk.*, B. 28, H. 2.

ON NEPHRITIS AND ALBUMINURIA IN THE TYPHOID FEVER OF CHILDREN.—The paper is based upon twenty-five cases observed in the children's wards of the hospital at Heidelberg, which are related, some briefly, others in more detail; only one of them was fatal. The author summarizes his results thus:—1. Albuminuria is of very frequent occurrence in typhoid in childhood. It appears generally in the first, or the beginning of the second week, sometimes as early as the second day; it varies in duration, the average being from one to two weeks. 2. Nephritis occurs in typhoid in childhood as it does in adults; but the higher degrees of it (dropsy, etc.) are not so frequent; there is no special renal form of typhoid in childhood. 3. In childhood, infectious diseases, such as scarlatina, in which the kidneys are frequently affected, appear to render the children specially liable to nephritis, should they take typhoid shortly afterwards. 4. Fever, albuminuria, and nervous symptoms in typhoid, are the result of one and the same cause, namely, the intoxication of the organism by poison depending on the typhoid bacillus.—A. Geier, *Jahrb. f. Kinderheilk*, B. 29, H. 1.

CEREBRAL PALSIES OF CHILDREN.—A most interesting and instructive series of clinical lectures on this subject was published last summer by Professor Osler of Philadelphia in the *Medical News*, the first five of which we propose briefly to notice. At the onset the cerebral palsies are differentiated from the more common spinal palsies (poliomyelitis anterior). The cases used for the purposes of these lectures numbered 150; many of them are related in more or less detail. Cerebral palsies occurred much less frequently than spinal ones, the proportion being about 1 to 4. Of the 150 cases 120 (57 boys and 63 girls) come under the heading of—

1. *Infantile Hæmiplegia.*—The great majority occurred within the first three years of life, 15 being congenital. In 9 of these latter cases forceps had been used during delivery; in 5 of them there was no difficulty during the birth; 1 was probably syphilitic. The causes to which the remaining cases may be ascribed are infectious diseases (measles, whooping cough, scarlatina), trauma, convulsions, embolic processes. The symptoms fall into three divisions—those of the onset, of the paralysis, and the results. The onset is generally attended by convulsions and coma, but occasionally it comes on quite suddenly, without spasms or unconsciousness. A latent gradual onset was noticed in 10 cases; delirium and vomiting occurred but rarely, and violent screaming fits only once. In the paralytic stage it was generally observed that as soon as the child became conscious the hemiplegia was complete. In 68 cases the right side was involved, in 52 the left. The face is not always affected, and when it is the paralysis almost invariably soon passes off. As in adults, the leg recovers more completely than the arm, and the palsy may entirely disappear, which rarely happens with the arm; the wasting is not so marked, nor does the

arrest of development, generally very extensive in the arm, often occur. Rigidity is present in a great majority of the cases when first seen, but not in all, and permanent contracture may result. The reflexes are almost invariably increased in the affected limbs; they may also be heightened on the sound side. Sensation was normal in all the cases examined; vaso-motor disturbances, coldness, blueness being generally present; electrical reactions usually normal; quantitative, but not qualitative changes noted. Post-hæmiplegic movements, such as tremor, chorea, athetosis, etc., were observed in 31 cases; aphasia in 13. Speech usually returns in time, often imperfectly. Epilepsy and mental defects are among the common results of these hemiplegias.

Morbid Anatomy.—In 16 cases there was embolism, thrombosis, and hæmorrhage; plugging in 7, hæmorrhages in 9. Atrophy and sclerosis were found in 50 cases, affecting either groups of convolutions, an entire lobe, or the whole of one hemisphere. Porencephalus, *i.e.*, a loss of substance in the form of cavities or cysts, situated at the surface of the brain, and passing deeply into its substance, was present in 24 cases out of 90 autopsies in hemiplegia. Diagnosis depends in the early stages on the frequent onset with convulsions, absence of rapid wasting, retention of electrical reactions; in the later stages on the rigidity, increase of reflexes, gait, impaired intellect, convulsions. Prognosis, *quoad life,* not bad; the leg often recovers perfectly, the arm seldom. The mental prognosis is bad; the earlier the onset of the disease the worse the prospect.

2. *Bilateral Spastic Hemiplegia.*—Mostly congenital, and due to intrauterine disease, or to injuries received during birth. Hence Gowers calls them "birth palsies." Nineteen cases, characterized by spastic condition of the extremities, the legs being involved more than the arms; no wasting; sensation not affected; reflexes increased; imbecility in all cases but one; athetoid and ataxic movements very common; electrical conditions unchanged. The essential lesion is destruction of the motor centres of the cortex, with, frequently, diffuse atrophic sclerosis.

3. *Spastic Paraplegia.*—We owe much of our knowledge of both this and the preceding condition to the orthopædic surgeons. The symptoms are—spastic paralysis of the lower extremities, coming on at birth or within the first few years of life; no wasting; a condition of talipes equinus or equino-varus; adductor spasms; gait stiff, patient walking on the toes, or progression cross-legged; intellect usually, not always, impaired; arms rarely involved; no pain; sensation unaffected; no trophic lesions; bladder and rectum act normally; the reflexes are increased. The pathological anatomy has not been exactly made out.

The last lecture deals with pathology and treatment. In a few cases, hæmorrhage from causes similar to those in adults. In many of the "birth palsies," probably meningeal hæmorrhage and capillary extravasations are the primary lesions. Infantile hemiplegia is probably the result of various processes, of which the most important are—1. Hæmorrhage occurring during violent convulsions, or during a paroxysm of whooping-cough. 2. Post-febrile processes: (*a*), embolic; (*b*), endo- and peri-arterial changes; and (*c*), encephalitis. 3. Thrombosis of the cerebral veins. Of the treatment there is not much to be said. In the later stages, careful and regular massage and orthopædic apparatus are most beneficial. Patient training and teaching in many cases improves the mental condition. Surgical interference with the brain is not to be recommended.—W. Osler, *Medical News*, July and August 1888.

SURGICAL PERISCOPE.

By A. G. Miller, F.R.C.S.E.

Treatment of the Hypertrophied Prostate (Francis S. Watson, M.D., of Boston, *Annals of Surgery*, Jany. 1889).—The author wishes "only to establish the claim for surgical interference in certain cases of the disease, and to lay down the lines in which such action should be taken." The "claim for surgical interference" is established on the grounds that in many cases of hypertrophied prostate a condition arises in which "something must be done." The "something" is divided by Dr Watson into "palliative" and "radical." The former he considers the more dangerous of the two, and this opinion he supports by both argument and statistics. There is no doubt that supra-pubic puncture with a retained canula is a most dangerous operation. Catheterization is also far from being free from danger, even in the hands of experienced and careful surgeons. Dr Watson says that "many more cases die from unskilful catheterization than from any or all the radical operations performed by competent surgeons." Again, he says, "a radical operation well done may be less dangerous than ordinary catheterization badly done." In direct favour of a radical operation, Dr Watson argues that while not more dangerous (to say the least) than a palliative operation, it accomplishes more. As to the special operation to be selected. He considers there are practically only two, viz., perineal and supra-pubic incision. From a careful examination by post-mortem dissection of thirty cases, he has arrived at the following conclusion,—"Anatomically, two-thirds of all cases are operable from the perineum." By this he means that the prostate is not so far removed from the perineal surface but that an ordinary finger may reach it through a free incision. From examination of statistics, Dr Watson finds that "clinically the perineal operations are the safest." Consequently he recommends this operation, with a prolonged period of drainage (eight or ten weeks), as the best to perform, at first at any rate. The aim of the operation is not only to reach and drain the bladder, but to remove a sufficient amount of the prostate to secure perfect and permanent drainage. If this cannot be done by a perineal incision, then the supra-pubic operation can be performed either at once or after an interval.

BALLOONING OF THE RECTUM (Thos. Bryant, F.R.C.S., *Lancet*, 5th Jan. 1889).—"The rectum in its normal condition is a collapsible tube, and when a surgeon introduces his finger through the sphincter into this tube, in its healthy state, he finds the walls of the bowel in contact, and these he has to separate to complete his examination. When a stricture of the rectum exists, this condition does not always hold, and the surgeon will often find, when his finger has passed the sphincters, that he has entered a cavity, the walls of which are expanded or 'ballooned.' In this cavity the surgeon will be able to move his finger freely, and the walls of the cavity will only be felt when searched for. The extent of ballooning will be found to vary in every case."

EXCISION OF THE SUPERIOR MAXILLARY NERVE FOR TIC DOULOUREUX: SEQUEL TO AN EXTRAORDINARY CEREBRAL INJURY (*New York Med. Journal*, 16th Feb. 1889).—Dr Abbe presented a male patient upon whom he had successfully operated six weeks before. "The man's unique injury had been reported in detail by Professor Zenner, of Cincinnati, in the *Medical Record*, October 10, 1885. It was very briefly as follows: In July 1885, while lighting the gas in a millinery show-window, he fell and was pierced in the neck by a sharp upright iron rod used for holding hats. The rod entered under the right jaw, and went upward, inward, and backward. It stuck so tightly, presumably in the bone of the base of the skull, that it took "a forcible effort by a powerful man" to remove it. It was at once observed that he could neither speak nor use his right arm or leg. Professor Zenner saw him half an hour afterward, and found complete paralysis of the right upper and lower extremities, of the tongue, of the muscles supplied by the lower branches of the facial nerve on both sides, and that he could neither swallow nor utter a sound. He was conscious, but restless and in much pain on the left side of the face. Later, anæsthesia was observed of the upper two branches of the trifacial nerve, which was probably present from the first. During one week he was nourished *per rectum*, but began to swallow a very little and to speak a few sounds a few days after the accident. In two weeks he could speak single words fairly and could walk by the aid of crutches, though there was still paresis of the right extremities. There was constant driveling of saliva. At six weeks his hemiplegia was no better. Hemianæsthesia of the portions supplied by the fifth nerve—namely, the entire half-face and neck and scalp, except the lower half, the ear—was now present. The subjective sensations grew worse. Constant pain was in the left half-face. Polyuria set in. Excessive flow, with sp. gr. 1·010, and no sugar. This was controlled after a while. The right foot dragged badly, and the hand had feeble flexion and almost complete paralysis of extension. Slight paresis remained on the right side of the face, but no aphasia. The diagnosis was made of a lesion of the pons and crus, with injury to the emerging fifth nerve from a penetrating wound of the basis cranii, giving crossed paralysis—viz., right hemiplegia, with left facial hemianæsthesia. The patient since 1885 had been examined and lectured upon by the most eminent neurologists, both in New York and Germany, where he had been during the past year in search of relief from the tic douloureux. Finding no encouragement, he had attempted to take his life by a pistol wound of the chest, the mark of which he bears. He lately came under the care of Dr Græme M. Hammond, who referred him to the reporter. Before operation he was shown by Dr Abbe to the Surgical Society, and showed the following condition: Absolute facial anæsthesia of the left side, the ocular conjunctiva bearing the finger to be rubbed about within the lids without noticing it. The nostril, lips, and mouth utterly insensitive to the prick of pins. The sense of taste and smell were gone on the affected side. The right foot showed a slight trace of the muscular weakness. The right arm and hand responded slowly and poorly to volitional attempts at flexion and extension, and, on attempted rapid or powerful actions of this limb, the patient experienced a sharp respiratory spasm, ending in a species of snort, which seemed to be explained by an interruption of the message at the site of the cicatrix between the surface of the convolution and the hand, giving a reflex irritation to the respiratory centre. It was the opinion of the late Dr Sands, who among others saw him in consultation, that excision of the nerve in front of the supposed cicatrix could hardly be expected to relieve the neuralgia. However, as only one branch was neuralgic, while all were anæsthetic, Dr Abbe decided to operate. On 20th October he removed the superior maxillary nerve through a vertical incision in the cheek by Carnochan's method, finding little difficulty in breaking through the front and back wall of the antrum and cutting off the nerve at the foramen rotundum. Meckel's ganglion was broken up by lacerating the tissues in which it was imbedded. The wound healed promptly without suppuration. The neuralgic pain was at once completely relieved and has not returned. Furthermore, an unexpected and almost inexplicable sequel has ensued, in that sensation has returned completely over the entire side of the face, except in the immediate vicinity of the scar, the ocular conjunctiva, lower lid, and part of the upper lip. Careful tests of smell and taste made by Dr Hammond and Dr Abbe show that he has partially recovered both faculties on the affected side. The patient, as shown, was in excellent health, free from pain, and experienced only slight disability of the right arm, with anæsthesia about the scar on the cheek."

SURGERY OF THE SPINE (*New York Med. Journal*, 16th February

1889).—Dr R. Abbe of New York read a report of two cases in which he had opened down upon the spinal cord, had excised diseased parts or growths attached to it, and had obtained rapid recovery and relief of symptoms. He added an account of other operations and of the literature of the subject. "In the first case the symptoms of spinal tumour had developed, anæsthesia of the lower half of the body, twitchings in the legs, a girdle sensation, pains, incontinence of urine and fæces, and a marked hectic and progressive wasting. He had exposed the cord by cutting away the spines and laminæ with the rongeur, and had found and removed a large degenerated tumour. The patient had at once recovered. In the second case an ascending neuritis had spread up the ulnar nerve from a bone disease of undetermined nature in the right hand. The pain had been violent, and had persisted in spite of medical, electrical, and surgical treatment, including nerve-stretching, undertaken by a large number of physicians in succession. Amputation of the arm high up had finally been performed, but the pain had not been changed in quality by this proceeding; the patient had developed tremor and other general nervous symptoms, and half a grain of morphine at short intervals had scarcely mitigated his suffering. He had first exposed the spinal cord as far as the dura, and severed the sixth and seventh cervical nerves on the right without the foramina, but afterward the old pain had returned unabated. He had then opened the dura (without ether), evacuated the spinal fluid, and severed the posterior roots of the seventh and eighth spinal nerves, by which at the time great pain had been caused. The least touch on the posterior columns of the cord had caused exquisite pain through the whole body below that point. After this operation the pain had been markedly lessened, so that the morphine had been discontinued. Under the microscope the nerve roots had been found changed by inflammatory exudation. The dura of the cord had been found not sensitive."

TRANSPLANTATION OF LARGE STRIPS OF SKIN FOR COVERING EXTENSIVE GRANULATING SURFACES (George R. Fowler, M.D., of Brooklyn, *Annals of Surgery*, March 1889).—Dr Fowler's paper is well worthy of careful study. He records a case of a negro who was extensively burned over the buttocks. The raw surfaces left by the burn were 17¾ inches by 11 inches on the left side and 15 inches by 9¾ inches on the right. The left side was covered with strips of skin from three frogs, the right with strips of human skin. Six months afterwards the note is—"Discharged cured. The frog skin is soft and pliable and of a reddish hue. The white human skin is firm and of a darker colour than the above, but much lighter than the rest of the patient's body. The portion of skin transplanted from the patient's arm is nearly as dark as the natural colour of the patient. There is not the slightest tendency to contraction of the surfaces of repair." The method of transplantation of human skin was that recommended by Thiersch (*Archiv für klinisch. Chirurg.*, Bd. xvii., 1874). The human skin transplantations were performed at various times during the period of treatment, and were much more successful than those of the frog's skin. The method of operation as described was briefly as follows:—1. Granulation surface as healthy and aseptic as possible. 2. Skin of arm or thigh shaved and carefully cleansed. Dr Fowler considers the congested condition of the skin produced by scrubbing to be beneficial. 3. By means of a sharp razor laid flat on the skin, which is made tense for the purpose, a strip or shaving of skin is removed (length and breadth not mentioned). This strip adheres to the razor and sometimes curls up on it. 4. The strip is transferred to the granulating surface, and lightly pressed on to remove all blood or other fluid from between the surfaces. Several strips may be applied, taken from one or more subjects. The whole raw surface should in this way be covered with strips, touching but not overlapping one another. 5. An antiseptic dressing is applied, beginning with protective oil-silk to prevent adhesion of the strips to the dressing. Dr Fowler recommends daily dressing, with great care, to prevent movement of the strips of skin. These at first become rosy or pale. A bluish colour indicates blood underneath, and is apt to be followed by death of the strip. After a week or two the part operated on may present a peculiar mosaic appearance (especially in the negro doubtless). This ultimately disappears, and a sound reliable skin surface results, with little or no tendency to contraction.

PRE-CANCEROUS CONDITIONS OF THE TONGUE (H. T. Butlin, *Illustrated Med. News*, 30th March 1889).—In a paper read before the Harveian Society of London, Mr Butlin gives the result of his experience on the above subject. His conclusions are under three heads:—I. The proportion of cases of cancer of the tongue in which there is a preceding well-recognised diseased condition. On this point the result is that of 14 cases observed by himself, "more than 70 per cent. of them had suffered from a pre-cancerous condition, some form of leucoma, or chronic superficial glossitis." II. The relative importance of various pre-cancerous conditions. On this point Mr Butlin gives as his opinion, "that the most dangerous condition is that in which the leucomatous areas are very thick and warty." III. The question of the early and free removal of the pre-cancerous conditions. Mr Butlin's remarks on this head resolve themselves mainly into a recommendation to remove pre-cancerous affections of the tongue freely if at all,—in other words, to treat such conditions as if they were really malignant, and not merely suspected.

AN OLD METHOD OF SECURING SCALP WOUNDS REVIVED.—Dr Hasson (*Internat. Journ. of Surgery*, February 1889) recommends the following simple procedure:—After checking hæmorrhage and cleansing the wound and the surrounding parts, two or three locks of hair are taken from opposite sides and tied together firmly enough to approximate the edges of the wound. Dr Hasson has frequently obtained primary union by this means. He considers that dangerous tension is impossible. He secures drainage by a few strands of catgut.

Part Fifth.

MEDICAL NEWS.

University of Edinburgh.—*The following received the Degrees of Bachelor of Medicine and Master in Surgery:*—Robert Oswald Adamson (M.A.),* Robert Dunlop Reid Allison,† Charles Arthur Anderson, Charles Macdonell Anderson, John Henry Aytoun, Jonathan Ernest Augustus George Becker, James Richard Bibby, Arthur Sandeman Bowes, Edward William Rawle Branch (B.A.), Frederick Perthes Bremner, Lewis Campbell Bruce, James Brunton, William Arthur Bryant, Leonard Horner Bryson, Robert Dick Buchanan, Thomas Lowe Bunting, Barend Burger, Thomas John Burton, Ernest Stanley Radford Cadman, Alfred Walter Campbell, David Allan Carruthers, Samuel William Carruthers, Percy Carr-White, Arthur William Carter, Robert Cattley, Louis de Bylandt Christian, Ernest Shaw Clark, Arthur Clarkson, Samuel Grahame Connor, James Hill Conyers, Allaster Edward Cox, John William Crerar,* Harry George Critchley (M.A.), George Matheson Cullen, James Cunningham, Henri Dardenne, David George Davidson, Frederick Arthur Day, John Rhodes Dickson, Herbert Dobie, John Josiah Douglas, Andrew Gardner Dow (M.A.), Edward Hugh Duncan (M.A.), John Watkin Edwards, Matthew Elder, William Everett,† William Frederick Farquharson,* Edward Fawcett, Samuel Fergusson, Thomas Frankish, Robert Valpy Fulton, Alfred Walter George,† Charles Gordon Gibson, James Albert Gibson, Walter Scott Gibson, Colin Gordon,† Henry Laing Gordon, George Govan, Guy Grindlay, Patrick Balfour Haig, John Hardie, Patrick Wood Hislop, Frank Alfred Amphlett Holmden, Alexander Cruikshank Houston, Robert Edward Howell, Hougham Charles Hudson, William Hugh Hughes, James Huskie, Thomas Whittaker Iddon, Herbert M'Lelland Inglis (B.A.), Peter Jones, Hugo de Dreux Kunz (M.A.), James Lackie,* James Hill Williamson Laing (M.A., B.Sc.)*, Hamilton Graham Langwill,* Charles Bunbury Lawson, William Leask, James Westwood Leitch (B.A.), Henry Adolph Leschen, Oswald Richard Lewis, Thomas Herbert Littlejohn, James Luke, John Stuart Shepherd Lumsden, Stuart Gerald M'Allum, John Macdonald, William Stuart M'Dougall, James Murdoch M'Gill (M.A.), Ronald Pierson Mackenzie, John M'Laren, Alexander Maclean,* Ewen John Maclean,† Charles Gordon MacLeod (M.A.), William Butterworth MacTier, Frederic William Mann, Charles John Linskill Mansel, William Manuel, Edward Forster Maynard, William Ombler Meek, Ernest Arthur Milner, James Monteith,† Thomas Lewis Morgan, Ambrose William Mortlock, Henry Temple Mursell, Alexander Nasmyth, Francis James Nicoll, William Oliver, William Henry Basil O'Neill, William O'Rourke, Robert Griffith Owen, William Maskelyne Parham, Charles Paton Parry, William James Peddie, Eustace Graham Pilgrim,* William Polson, Samuel Poole, Alexander Jack Prentice, Petrus Gerhardus Pretorius, Frederick Arthur Gordon Purchas, Peter MacPherson Reid, Francis William Reitz, Martin James Richardson, Alexander Brown Ritchie, Thomas Morton Ritchie (B.Sc.), Charles George Robson-Scott, Frederic William Forbes Ross, Robert Dawson Rudolf, William Gordon Woodrow Sanders,* John Alexander Scott, William Allan Simpson, James Lorrain Smith (M.A.), Harold Arthur Stalkartt, John Douglas Stanley, Ernest George Best Starkie, Edward Steehan, John William Steven, Frank Stevenson, Frederick Naylor Stewart, George Neil Stewart (M.A., D.Sc.), Kenneth Trevor Stewart, Thomas Pollard Stewart (M.A.), Benjamin Sweeten,† Ernest Edmund Taylor, James Maxtone Thom, Evan Cambria Thomas, Alexander Thomson, William Ernest Francis Thomson (M.A.), James Andrew Thyne,* Arthur Logan Turner, Gilles Vandewall, John Justinian

Vernon, Arthur John Wallace, Herbert Watthews, David Westwood, Abraham Wheeler, Arthur John Whiting, Herbert Henry Wilde, George Wilkinson,* Alexander Frazer Wilson, Arthur Wilson, George Robert Wilson, John Wilson (M.A.), John Johnstone Wilson, Charles Neil Campbell Wimberley,† Richard Wyse (M.A.).

* Indicates that the Candidate has passed the examinations with first-class honours.

† Indicates that the Candidate has passed the examinations with second-class honours.

Edinburgh Medical Examinations.—The following is the official list of candidates who passed the Second Professional Examination in Medicine at Edinburgh University:—John Anderson, R. J. Ashton (with distinction), Hugh Bennett, R. J. A. Berry, J. W. Bone (with distinction), H. J. M. Buist, W. M. Cairns, K. M. Cameron, J. J. W. Campbell, Charles Cochrane, R. P. Cockburn, D. B. Crerar, G. D. Darlington, Arthur Dennison, H. R. L. Davis, C. J. W. Dixon (with distinction), D. P. M. Farquharson, F. T. Foster, E. J. Fox, R. A. Fryer, E. B. Garland, P. F. Grant, Henry Goudie, Frederick Gourlay, G. C. Greaves, F. T. Griffin, T. A. Green, R. E. Harrold, H. H. Hearsey, G. E. Helme, H. M. Hewer, A. G. Horden, J. V. Hulme, John Joakim, J. M. M. Kay, C. B. Ker, Robert Lamb, M.A., E. P. T. von Landsberg, H. E. Lee, R. E. Little, John Livingstone, R. M. Lyon, James MacBride, J. G. Macdonald, James Macfarlane, H. M. MacGill, Wakefield MacGill, T. G. M'Keller, H. J. Mackenzie, H. V. Mackenzie, John Maclaren, J. D. Maclean, W. B. Macleod, Charles Macmaster, C. B. Martin, H. F. Malabre, George Melville, J. Adie Menzies, A. S. Miller, Alexander Mitchell, T. W. Mitchell, E. C. Moore, C. A. Morgan, Robert Muir, J. A. Murison, John Newington, P. A. Nightingale, N. G. Noble, C. M. Ormsby, John Panton, Wm. Paul, E. R. Parry, R. L. Price, Harry Rainy (with distinction), J. L. Reed, E. H. Ridley, J. H. Roberts, J. C. Robertson, David Rorie, Robert Rosie, Wm. Sanders, Walter Sansom, P. W. Schmidt, John Sharp, William Simmers, F. M. T. Skae, Thomas Stodart, Archibald Stodard-Walker, Robert Strachan, Charles Stuart, David Stuart (with distinction), William Thyne, J. C. Tosswell, A. K. Thomson, L. J. Weatherbe, J. C. White, George Wilson, O. R. M. Wood, James Younan, R. J. E. Young.

Royal College of Physicians, Edinburgh.—The following gentlemen passed their final examination for the degree in Medicine at the sittings held on 9th May 1889, and were admitted L.R.C.P. Ed.:—Rich. Headley Betton, Boston; Joseph Beadle, Middleton-on-Teesdale; Edward Burdett Pooley, Bradford; Edwin Johnson Elderkin, Nova Scotia; John Wm. Chambers Herbert, Pendlebury.

Royal College of Surgeons of Edinburgh.—The following gentlemen have been admitted Fellows of the College during the half-year to 30th June, 1889:—(*After Examination*).—Messrs Alexander Oswald Cowan Watson; John Campbell Graham, Sumatra; Alex. William Flood, County Donegal; Augustus Hewett; Damer Harrisson, Liverpool; Wm. Day Stewart; Claud Herbert Clabburn; Alex. Yeates Reilly; George Trevor Harley Thomas; John Stewart Nairne, Glasgow; John Ross Murray. By election—Messrs Thomas Edward Williams, Breconshire; and Robert Somerville, Galashiels. During the July sittings of the examiners, the following gentlemen passed the first professional examination for the license in dental surgery:—John M'Laren Mason, Edinburgh; and Arthur Sherwood Anderson, Edinburgh; and the following gentlemen passed the final examination, and were admitted L.D.S., Edinburgh—Charles William Stuart Wilde, Liverpool; Thomas Gregory, Edinburgh; William Graham Campbell, Dundee; Charles Henry James Acret, Canada; John Henry Cormack, Edinburgh; James Seymour Allen, Ripley; Walter Graham Routledge, Exeter; Alfred Elliot Smith, Norwich; Lawson Storrow Shennan, Houghton-le-spring; and Frederick Leonard Floyd Masters, Huddersfield. The following having passed the required examinations were admitted L.R.C.S.E.:—Miss Julia Cock, Shrewsbury; Miss Jane Harriett Walker, Yorks.; William Alexander Mackay, Caithness; Herbert Edward Balmain Dickson, Sussex; Frank Haley, Melbourne; Jabez Davenport Windle, Oldham; and William Clow Little, Ontario.

International Medical Congress.—We, the undersigned, do hereby give notice, that according to the resolution passed at the Washington meeting, 9th September 1887, the 10th International Medical Congress will be held in Berlin. The Congress will be opened on the 4th, and closed on the 9th day of August 1890. Detailed information as to the order of proceedings will be issued after the meeting of the delegates of the German Medical Faculties and Medical Societies at Heidelberg on the 17th of September in the current year Meanwhile, we should feel sincerely obliged if you would kindly make this communication known among your medical circles, and add in the same time our cordial invitation to the Congress.—Von BERGMANN, VIRCHOW, WALDEYER.

CONGRÈS FRANÇAIS DE CHIRURGIE.—La quatrième session du Congrès Français de Chirurgie se tiendra du 14 au 20 Octobre 1889, à Paris, dans le grand amphithéâtre de l'École de médecine, sous la présidence de M. le baron Larrey. Séance d'ouverture Lundi 14 Octobre, à 2 heures. *Questions à l'Ordre du Jour.*—(Voir l'article III du Règlement.) Les questions suivantes sont mises à l'ordre du jour du congrès: I. Résultats immédiats et éloignés des opérations pratiquées pour les tuberculoses locales; II. Traitement chirurgical de la péritonite; III. Traitement des anévrysmes des membres.

ACADÉMIE DE SCIENCES, Séance du 22 Juillet 1889.—M. le Dr Fort communique un travail sur le mode d'action de l'électrolyse linéaire, dans le traitement des rétrécissements. (Note présentée par M. Bouchard.) Des faits nombreux (550) et de l'expérience que nous avons rapportée, dit l'auteur, nous sommes autorisé à tirer les conclusions suivantes:—1°. L'opération de l'électrolyse linéaire, par un courant continu peu intense, n'est pas une cautérisation produite par une température élevée. 2°. L'électrolyse linéaire est une destruction lente et moléculaire du tissu pathologique, s'accompagnant d'un dégagement d'hydrogène et du transport des alcalis du tissu électrolysé au pôle négatif. 3°. L'électrolyse linéaire, inoffensive, presque indolore, et durant à peine quelques minutes, se produit absolument à froid sans qu'il soit possible de constater aucune élévation de température, du commencement à la fin de l'opération, ainsi qu'il résulte d'une expérience faite par M. d'Arsouval, professeur au collège de France, au moyen de ses appareils thermo-électriques.

LIST OF THE SUCCESSFUL CANDIDATES at the recent Competitive Examination for Commissions in the Medical Staff of Her Majesty's Army:—

Order of Merit.	Names.	Marks.
1.	Waller, H. J.,	3085
2.	Bewley, A. W.,	3080
3.	Mould, W. T.,	3030
4.	Hughes, M. L.,	2910
5.	Stone, C. A.,	2800
6.	Way,	2730
7.	Moores, S. G.,	2640
8.	Winter, H. E.,	2630

NEW REMEDIES.—Burroughs, Wellcome, & Co., London, the well-known manufacturers of many of the new remedies, have sent us specimens of several of their recent preparations, and which we have much pleasure in highly recommending to the notice of the profession. 1. "Thirst Tabloids" of compressed bicarbonate of sodium, citric acid, white sugar, and gum arabic, flavoured with essence of lemon. These "tabloids" are most agreeable to take, melt slowly in the mouth, and have the power in an eminent degree of quenching thirst. They are specially to be commended when travelling or where water is scarce. 2. "Tabloids of Compressed Salicin, 5 grains." These tabloids will be found very convenient for the administration of a most valuable medicine. 3. "Tabloids of Compressed Borax, 5 grains." These are beautifully prepared. 4. "Tabloids of Compressed Compound Sulphur." Each tabloid contains 5 grains of precipitated sulphur and 1 grain of cream of tartar. This is an excellent form in which to administer a useful combination of two excellent medicines. 5. "Tabloids of Compressed Papain, 2 grains." These tabloids will be found most useful for dyspepsia due to or accompanied by a diminution of the gastric juice. The digestive action is much increased when taken along with a tabloid of bicarbonate of sodium. We have pleasure in highly recommending these tabloids to the notice of physicians. 6. "Soluble Compressed Hypodermic Tabloids of Apomorphine, some containing $\frac{1}{15}$ grain, others $\frac{1}{10}$ grain of the alkaloid." They contain a small quantity of common salt to give them proper bulk. Each tabloid is soluble in 5 minims of water. We can confidently recommend these tabloids as at once the most efficient and best way of administering apomorphine as an emetic. The alkaloid appears to keep better in this than in any other form. 7. "Unguentum Lanolini." It consists of anhydrous lanolin 65 parts, liquid paraffin 30 parts, and ceresin 5 parts. This ointment is an excellent emollient, and is specially recommended as a basis for ointments. It is sure to be extensively used instead of the bases which are at present recognised in our Pharmacopœias. We have much pleasure in highly recommending it to the notice of chemists and medical practitioners.

LIQUOR CAULOPHYLLIN ET PULSATILLÆ CO. (OPPENHEIMER'S).—We have received from Oppenheimer Brothers & Co., London, a specimen of this preparation, which combines and intensifies the actions of two powerful and useful medicines. It will be found most efficacious in many forms of uterine affections, such as amenorrhœa, difficult and painful menstruation, menorrhagia, etc. It will also give good results in irritable conditions of the vagina or bladder. It will also prove useful as a sedative to the sexual organs in the male. We can confidently recommend it to the notice of the profession, feeling sure it will give satisfactory results.

WE have received from Wilcox & Co., chemists, London, a box containing specimens of medicines prepared by the French chemists Rigaud & Chapoteaut, Paris, beautifully prepared and in forms easily administered. These include—"Peptone Wine;" each wineglassful contains the pure peptone of 10 grammes of beef. This is an excellent preparation. The others are contained in capsules.—"Morrhuol;" each capsule is said to contain the curative principles of a teaspoonful of the oil. This is an easy way to administer a most useful drug.—"Valerianic Ether;" each capsule contains 4 drops of this powerful antispasmodic.—"Santal;" each capsule contains 5 drops of the oil. This is an excellent form in which to administer a most powerful and valuable remedy.—"Dialyzed Pepsine," in pearls; these are of the size of pills, and each contains 4 grammes of dialyzed pepsine. These will be found very useful, and are easily carried about by invalids.—The same firm has also sent a box of "Joy's Anti-asthmatic Cigarettes," which often prove so useful in cases of difficult and oppressed breathing.—They have also sent specimens of their "Dessicated Malt Extract." This is an elegant preparation, and one we can confidently recommend.

Antipyrin and Lanoline have just been adopted as officinal in the Austrian Pharmacopœia. This is perhaps a most important endorsement of the value of these two new additions to modern Materia Medica.

PUBLICATIONS RECEIVED.

Army Medical Department Report for 1887. Vol. XXIX.

Atlas of Illustrations of Pathology. Fasc. vii. New Sydenham Society, Lond., 1889.

H. F. Blanford, F.R.S., F.R.Met.S.,—A Practical Guide to the Climates and Weather of India, Ceylon, and Burmah, and the Storms of Indian Seas. Macmillan & Co., Lond., 1889.

Prof. J. M. Charcot,—Clinical Lectures on Diseases of the Nervous System. New Sydenham Society, Lond., 1889.

Julius Cohnheim,—Lectures on General Pathology. New Sydenham Society, Lond., 1889.

Congrès pour l'étude de la Tuberculose chez l'homme et chez les Animaux. 1re Session, 1888. G. Masson, Paris, 1889.

Geo. Foy, F.R.C.S., etc.,—Anæsthetics, Ancient and Modern. Baillière, Tindall, & Cox, Lond., 1889.

George Herschell, M.D., etc.,—Health Troubles of City Life. John Wright & Co., Bristol; Hamilton, Adams, & Co., 1889.

Samuel Hyde, L.R.C.P., etc.,—Buxton: its Baths and Climate. John Heywood, Lond., 1889.

C. E. Jennings, F.R.C.S., etc.,—Cancer and its Complications. Baillière, Tindall, & Cox, Lond., 1889.

Norman Kerr, M.D., etc.,—Inebriety: its Etiology, Pathology, Treatment, and Jurisprudence. H. K. Lewis, Lond., 1889.

Greville Macdonald, M.D.,—On the Respiratory Functions of the Nose, etc. Alex. P. Watt, Lond., 1889.

C. A. MacMunn, M.A., M.D.,—Outlines of the Clinical Chemistry of Urine. J. & A. Churchill, Lond., 1889.

C. N. Macnamara,—Leprosy: a Communicable Disease. J. & A. Churchill, Lond., 1889.

C. N. Starcke,—The Primitive Family in its Origin and Development. Kegan Paul, Trench, & Co., Lond., 1889.

Louis Starr, M.D.,—Hygiene of the Nursery. H. K. Lewis, Lond., 1889.

J. B. Sutton, F.R.C.S.—Dermoids. Baillière, Tindall, & Cox, Lond., 1889.

H. H. Tooth, M.D., F.R.C.P.,—Secondary Degenerations of the Spinal Cord. J. & A. Churchill, Lond., 1889.

Transactions of the American Surgical Association. Vol. VII. Philadelphia, 1889.

Tuberculous Meat,—Proceedings at Trial, The Glasgow Local Authority *v.* Hugh Couper and Charles Moore. Glasg., 1889.

Part First.

ORIGINAL COMMUNICATIONS.

I.—THE SURGERY OF THE LIVER.

By Lawson Tait, F.R.C.S., LL.D., etc., Professor of Gynæcology in Queen's College, Birmingham.

The diseases of the liver which have, up to the present time, yielded to the persistent determination of the modern school of Surgery to leave no stone untouched in the advancement of the art, are gall-stones, hepatic abscess, and hydatid tumours. There are others, and, I am ashamed to say, unknown diseases which seem to yield to surgical treatment applied to them by accident, and these unexpected successes may some day lead us on the road to other victories, but how these are to be achieved we cannot as yet say. Certainly nothing looked less attractive as a field for surgical research during my student life, some quarter of a century ago, than that which I now regard as the most fruitful upon which I have engaged—the liver.

At that time I was infiltrated with the glamour of John Hughes Bennett and his school, and I saw, without wincing, the ghastly experiments made upon dogs' livers and gall-bladders—experiments as futile as the calculations of the astrologer and as hindersome of real advance as the ante-Copernican system of astronomy. Seeing the poor dogs die almost invariably after operations of the kind spoken of, and seeing conclusions derived from the experiments which were diametrically opposed to every day clinical experience, it did not dawn upon me for years that Bennett was all wrong, that his vivisection was not only useless, but, and this was far worse, misleading and obstructive.

If we had read the teachings of the masters, and had looked at the experiments in Nature's own laboratory, disease ; had we taken the conclusions of these experiments as they stared us in the face, we should have been at least a century in advance of our present position. I do not say that our operations would have been very numerous or very successful, for we still wanted the great gift of anæsthesia, but our principle would have been sounder and more consolidated, and our errors and delays would have been far fewer.

Jean Louis Petit, in the sixty-third volume of the *Mémoires de l'Academie de Chirurgie*, tome i. p. 155, fully describes the operation of cholecystotomy as it is now done, and yet this brilliant conception lay hidden away for a century and a half until put into actual practice by Dr Marion Sims in 1878. Petit's proposal is clearly based on the process, every now and then fulfilled, of a natural cure by the process of suppurative adhesion of the gall-bladder and discharge of the gall-stones. The literature of pathology is strewn with such cases as far back as 1570, and it is only a matter of surprise that Petit's proposal was not anticipated. But without chloroform such advances were not possible. Neither surgeons nor patients could, in great numbers, stand the agonies of abdominal surgery without an anæsthetic, and the hurry and mental strain upon even the boldest and most dexterous operator would surely have resulted in many mistakes in operations performed upon conscious sufferers. The advances of modern Surgery are therefore greatly to be credited to the discovery of the anæsthetics. In 1869 I saw a patient suffering from suppurating gall-bladder and discharge of gall-stones at the umbilicus, and I slit up a long sinus and dug out some ten or twelve gall-stones, with immediate and permanent relief. In the same year I saw a woman die of the agony of gall-stone colic, a diagnosis verified on *post-mortem* examination. In the same year I saw with Mr Walker and Dr Wright of Wakefield, a lady brought to the very verge of death's door by the passage of a gall-stone as large as a pigeon's egg. Between that and August 1879 I have no recollection of ever seeing a case which led me further into the subject. Fortunately my attention was rivetted upon the subject by Dr Marion Sims' attempt in Paris in 1878—brilliant, but unfortunately unsuccessful. The want of success in this, the pioneer case, was clearly due to the tinkering delay which is fatal to so much good surgery, and against which it is the duty of good surgeons unceasingly to protest. The historic interest of Dr Sims' case is so great that I give here an abstract of his account of it.

The patient was an American lady, aged 45, and had enjoyed perfect health until the previous year, during the latter part of which time she became suddenly jaundiced, the colour reaching a deep mahogany tint. She was seen by a number of physicians, who discovered a swelling of the lower border of the liver. She began to pass quantities of blood in the stools, suffering from severe pain with intense itching all over the skin, and hyperæsthesia. In January the tumour began to increase markedly, and she also suffered much from nausea and vomiting. The symptoms increased so that she was rarely free for a day. She suffered severely from insomnia, and became thin and emaciated ; the urine became scanty, and in March the tumour reached far below the umbilicus.

On the 17th of March the tumour was aspirated, and 32 ounces of a dark brown fluid were evacuated. This fluid on analysis was found to contain no bile. The relief by tapping was very remarkable; she slept all night and a good deal of the following day, and the vomiting ceased. But the symptoms rapidly returned, and nine days after the aspiration she had a hæmorrhage from the nose, fainted, and all her old symptoms came back. By the middle of April the pain in the tumour was so much worse, there was so much tenderness on pressure—such evidence of inflammatory action, with such a degree of prostration—that Dr Bremond and Dr Sims, "looking upon the case as hopeless," agreed to cut down on the tumour, to open it and empty its contents, and attach its incised border to the edge of the abdominal incision. This was done upon April 18th, constituting the first operation of cholecystotomy on record. A part of the hypertrophied gall-bladder was removed, together with a large number of gall-stones. She lived eight days after the operation, and at first it seemed as if she would recover.

On *post-mortem* it was found that 16 sacculated gall-stones had not been removed. The *post-mortem*, conducted with great care, revealed nothing except calculous cholecystitis, with dropsy of the gall-bladder. There was no malignant disease, and there is no doubt in my mind that had the operation been performed reasonably early in the case, success would have crowned the efforts of the distinguished American surgeon.

He sums up in the following words the great lesson that his case teaches us:—"In dropsy of the gall-bladder, in hydatid tumours of the liver, in suspected abscess of the liver, and in gall-stones, we should not wait till the patient's strength is exhausted, nor till the blood becomes bile-poisoned, producing hæmorrhages, but we should make an early exploratory incision, ascertain the true nature of the disease, and then carry out the surgical treatment that the necessities of the case may demand."

To my good fortune it has fallen to be the first to follow out Petit's proposal and Dr Sims' plan successfully.

E. M., aged 40, was admitted to the hospital on August 18, 1879, having been sent to me by Dr Abraham Colles, of Bridgnorth, on account of an abdominal tumour.

She had been married eighteen years, had borne six children, her menstruation had always been normal, and she had enjoyed perfectly good health until the summer of 1878. At that time she began to suffer from severe spasmodic pains in the right side, these being always aggravated by walking or by lifting even slight weights. In September she noticed a swelling at the seat of pain, and this slowly increased. During last winter her pain became much more intense, her appetite failed, she lost strength and flesh rapidly, and on admission she presented an emaciated and almost cachectic appearance. She also suffered at that time from incessant headache and sickness, and obstinate constipation. The seat of pain was over the right kidney, where there was a heart-shaped tumour, firm and elastic, in which no fluctuation could be detected, and which was extremely tender to the touch. On examination under ether, this tumour was found to be perfectly movable toward each side; indeed, it could be pushed completely across the middle line to the left side. All round it a note of intestinal resonance could be produced. When pushed over to the left side, its heart-like shape became very apparent, and when it lay on the left side of the vertebral column, with its apex directed downward and to the left, its base evidently retained a connexion with the right side.

A careful examination of the urine gave only negative results, though she spoke vaguely of its having been occasionally dark in colour, muddy, and deficient in quantity.

At the consultation held upon the case, a variety of suggestions were made for diagnosis, the chief of which were cystic enlargement of a floating kidney, a tumour of the head of the pancreas, and dropsy of the gall-bladder. But no decided diagnosis was attempted, and my proposal to open the abdomen, and thus ascertain the nature of the tumour, was agreed upon.

On August 23rd I opened the abdomen in the middle line to the extent of four inches, the umbilicus forming the centre of the incision. It then became at once evident that the tumour was a distended gall-bladder. I passed the needle of an aspirator into the apex, and drew off a quantity of white, starchy-looking fluid, probably amounting to between twelve and fifteen ounces, but I cannot speak positively as to its amount, as it was unfortunately thrown away by a nurse immediately after the operation. I then opened the gall-bladder at the point of puncture, so as to admit my finger, and came at once upon a large, round gall-stone, lying loose in the cavity. This I easily removed, and on further search I found another of rather larger size, and probably of pear-shape, at the entrance of the duct, impacted in it, and evidently the cause of the dropsical distension of the gall-bladder. The removal of this stone was a matter of very great difficulty; in fact it took a very much longer time to effect than all the other steps of the operation put together. From the long, narrow, funnel-like cavity in which it was lodged, and from the mobility of the bladder, it was very difficult to seize, and when at last I did get hold of it, I found it adherent to the mucous surface. I had then to consider the extreme likelihood that in removing this impacted stone I might tear the walls to which it was attached, and thus certainly kill my patient. I therefore performed a very careful and protracted lithotrity, chipping little fragments off the stone regularly all over its exposed surface till I had the satisfaction of lifting out its nucleus. I then passed a blade of a fine pair of forceps on each side of it, and by a gentle squeeze broke up the remainder, and was then enabled to lift it all out. The weight of the stone removed entire is 4·2 grammes, and that of the fragments I could gather of the broken stone is 2·9 grammes, but of the latter stone as much again must have been lost on the sponges which were packed into the wound during the process of crushing, and upon which I had constantly to wipe my instruments. I washed the cavity out repeatedly, and took every precaution that I could to secure that no fragments were left. I then stitched the wound in the gall-bladder to the upper end of the wound in the abdominal walls by continuous sutures, leaving the aperture into the bladder quite open, and then I closed the rest of the abdominal opening in the usual way. The operation was performed with complete antiseptic precautions, and the anæsthetic employed was ether.

She rallied from the operation completely in a few hours. I dressed the wound antiseptically the same evening at 11 P.M., and found the dressings stained with healthy bile. In the further progress of the case there is very little to report, save that the flow of bile from the wound continued till September 3rd, when the dressings were discontinued, and zinc ointment was used in their place. The stitches were removed, and the wound was completely healed on September 9th, when she began to take solid food, up to that time her diet having been restricted to milk and beef-tea. On the 14th she sat up for the first time, and on the 30th she went home quite restored to health, free from pain and all her former symptoms, and having gained at least fourteen pounds in weight.

She has since remained in perfect health. After the operation she told us that one of her neighbours had said to her one day that she thought the patient was jaundiced, but beyond this no history could be got at of any symptoms pointing clearly to the true nature of the case.

As will be seen from the table embodied in this paper, I have performed the operation of cholecystotomy fifty-five times, and fifty-two of the patients have recovered. One old woman died of suffocative catarrh some weeks after the wound was healed, and two others died of the cancer of the liver which was in all probability the cause of the distended gall-bladder in one of them, for I found no gall-stones which I could remove. In the second, the death of the patient four months after the operation revealed extensive cancer with suppuration of the liver. He obtained very great relief from the opening of a suppurating gall-bladder and the removal of a large quantity of gall-stones. As a matter of fact, in not a single instance did the patient die from the operation. The remainder of these cases are in perfect health, and as well as they ever have been, with one exception, and that exception has taught me a great lesson—amongst others, it has completely satisfied the conclusion which I had previously arrived at, that my late friend Dr Marion Sims laid down principles from which we are not likely to depart with any advantage, and which, it is true, were to a large extent anticipated by Petit, and the more I see of the operation the more satisfied I am that he practically perfected it, though he did not meet with the success with his own case which he merited.

Three modifications of the operation have been suggested, and have been actually put in practice; two of them are based on altogether fallacious reasonings, and have not been successfully adopted in practice.

The first is a proposal made, I believe, by Sir Spencer Wells, to open the gall-bladder, remove the calculi, and then to close the bladder by continuous sutures without attaching it to the abdominal wound. So far as I know, this proceeding has been but seldom put in practice, and has been fatal in every instance. In one case the post-mortem records of it have been made public. A large quantity of bile was found in the peritoneum, as might have been expected; and whether this was the cause of death or not, it certainly was not a condition which we might expect would be contributory to success. But the arguments against this proceeding are *à priori*, and are of the strongest possible nature, and do not depend merely upon the want of success in a few preliminary cases; in fact, the arguments against it are so strong that it is difficult to see how it ever came to be suggested.

The gall-bladder is an organ subject to periodic filling and emptying, the latter process being accomplished by contraction of the muscular walls of the organ, and this contraction is far more powerful than is generally believed. The gall-bladder also secretes a quantity of clear albuminous fluid from its mucous surface, and this fluid contains some kind of fermentative principle. It is this fluid which distends the gall-bladder in cases of impacted calculus—may distend it, in fact, to an enormous size, as I shall afterwards narrate. If, however, the duct happen to be occupied by a calculus at the time of operation, the wound at the base of the gall-bladder would not remain closed, but would speedily be torn open; and it is a matter of extreme difficulty—in fact, it is to me a matter of absolute impossibility—to be perfectly certain during the operation of cholecystotomy that all the stones are removed from the duct.

The canal may have been distended by the passage of a calculus urged forward by the pressure of the secretion of mucus from the walls of the gall-bladder. So long as the stone is in the cystic duct, the contents of the distended gall-bladder consist entirely of this clear mucus. After the stone has passed the mouth of the liver duct the bile flows into the gall-bladder, its passage into the duodenum is prevented, it is reabsorbed into the system, and jaundice is produced. It is the only condition in which jaundice is possible from the passage of a gall-stone, but, as the common duct is wide and easily dilated, these attacks of jaundice are very evanescent, or are rarely seen. So short a time, in fact, is required by the passage of the gall-stone from the common duct, that in the great majority of instances it is perfectly certain jaundice never appears; but suppose that the operation was performed whilst the stone was in the duct—and I shall shortly relate a case in which this occurred—the bile would at once tear the incision open and pass wholly into the peritoneal cavity. The cystic duct in its normal state is of much smaller diameter than the common duct and more difficult to distend, and the agonizing pain of a passing gall-stone seems to be limited in a great measure to this part of its journey; and here, again, Sir Spencer Wells' proposal is manifestly a dangerous one. Exactly the same argument is to be urged, and with still greater force, against Langenbuch's proposal to remove the gall-bladder; in fact, this proposal may be regarded as intrinsically absurd, for there can be no reason for removing any organ merely because it has some calculi in it.

It has generally been supposed that gall-stones are formed in the gall-bladder, but this is not true; it is no more true than as a rule urinary calculi are formed in the urinary bladder. As a matter of fact, the stones are formed in the kidney, and the nuclei of gall-stones are certainly formed in the streams of bile as they flow through the substance of the liver. They are washed as small groups of cholesterine crystals, some into the gall-bladder, but most of them out into the duodenum, where they are harmless. Those that remain in the gall-bladder increase in size by further deposits of cholesterine on their rough surface. Gall-stone is, therefore, not a disease of the gall-bladder at all. The only argument of any urgency which has ever been offered for the removal of the gall-bladder is that it would prevent the recurrence of the gall-stones. This is true in the sense that it would prevent the growth of the gall-stones if another shower of them happened to be scattered down from the liver; but this is no argument at all for removing the gall-bladder, for even if the stones grew in it, if it were fastened—as in Petit's operation—to the abdominal wall, a very slight incision into its cavity would enable us at once to remove the offending contents. But, in addition to all this, in many of the cases that we have to operate upon where the gall-bladder is suppurating and greatly thickened, opening of the gall-bladder, removal of the gall-stones, and drainage of the cavity for a week or two, completely cures the inflammatory condition and restores the patient with a healthy organ. If there can be justification for removal of the gall-bladder at all it would be in these cases, and in these cases I venture to say it would be an absolutely impossible operation.

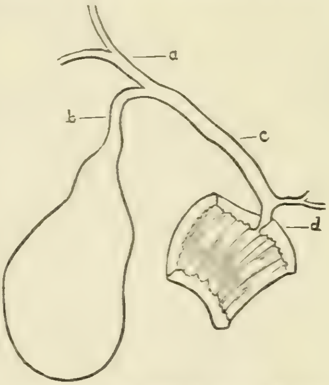


FIG. 1.—*a*. Hepatic duct; *b*. Cystic duct; *d*. Duodenal orifice of common duct after junction with pancreatic duct.

The one argument against cholecystotomy—that biliary fistulæ remain occasionally and permanently—is an argument of much greater force against the removal of the gall-bladder and the so-called operation of cholecystectomy, for such a fistula after cholecystotomy must be due to the fact that the operation had been performed at a time when a gall-stone or gall-stones had become impacted in the common duct. In three or four cases I have crushed this obstructing gall-stone, and have succeeded thereby in getting a free road through the common duct. In one case I succeeded in crushing one stone, but, as will be seen from drawings taken from the post-mortem preparations seven years after, I could hardly be expected to make my way through a series of calculi which obstructed both the cystic and the common ducts from one end to the other, and the result was that the patient had a permanent fistula. She lived perfectly comfortably for four or five years, and finally died of phthisis, which, I understand, was in the family, and perfectly independent of the biliary fistula,—an assertion which, I think, is proved by the fact that two years after the operation the patient had gained nearly two stones in weight. In such a case it must be evident to the least skilled observer that removal of the gall-bladder would be, of all possible proceedings, the very worst, for the bile must at once have regurgitated into the peritoneum and caused the patient's death.

It may be concluded that the case where biliary fistula has been known to remain permanently after the operation of cholecystotomy really constitutes the strongest of all arguments against the rival operation of cholecystectomy, and the greatest argument in favour of the operation of Petit, the more by reason that we now know perfectly well that we can overcome the discomfort and trouble of a biliary fistula very readily by making an artificial junction between the gall-bladder and an adjacent coil of intestine. In this way we can secure the return of the bile to its proper route, and free the patient from the discomfort of the fistula. This has recently been proved to be a successful operation by Mr Mayo Robson of Leeds.

In all probability, however, the larger number of cases of biliary fistula resulting from obstruction of the common duct by a stone lodged there at the time of operation may be completely relieved by the operation of cholelithotrity,—that is, to make a fresh opening in the abdomen and crush the stone outside the walls of the duct by means of padded forceps. I have already indicated my belief that gall-stones are formed in the substance of the liver; indeed, it is so proved by many of the observations by Dr Thudicum and others, and it has also been proved by the fact that in more than one instance I have cut gall-stones out of abscesses in the substance of the liver; in one instance the gall-stone was more than one inch in length.

But the great majority of the cases of gall-stones with which we have to deal surgically are contained within the cavity of the gall-bladder, and, for clinical purposes, it seems to me necessary to divide them into two varieties. The symptoms they give rise to, and, to some extent, the details of the operation required for their removal, differ considerably in the two varieties. I am inclined to believe that the pathology of the two forms is not quite alike, but I have not an opportunity of following out the research which this view suggests as being necessary. Suffice it for the present for me to repeat that, for clinical purposes, there are two kinds of gall-stones, which we may call the solitary gall-stones and the numerous gall-stones. The solitary gall-stone is not always quite solitary, but it has rarely more than one companion. The numerous gall-stones, on the other hand, are practically indefinite in quantity, and are usually of a pretty uniform and not large size. It may be that the solitary gall-stone is a mere remainder of the multiple formation, as may be explained by-and-by, but I have not found any evidence of this being the case, and it certainly is to some extent disproved by the fact that a large solitary gall-stone can be shown to be constructed of layers as definite as those of the uric acid calculus in the bladder; but any such examination clearly indicates that the majority of the cases are multiple gall-stones.

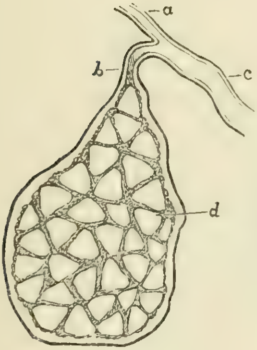


FIG. 2.

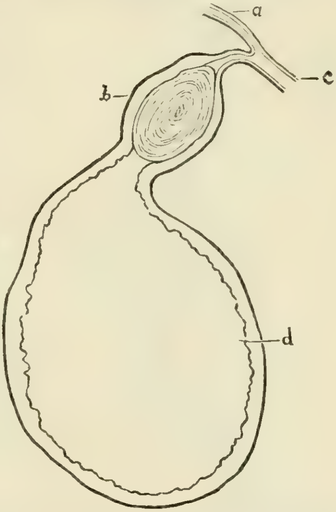


FIG. 3.

FIG. 2.—Gall-bladder containing numerous equal-sized gall-stones angulated and facetted by mutual compression and friction.

FIG. 3.—Solitary gall-stone impacted in neck of gall-bladder.

A typical instance of the operation necessary in a case of solitary gall-stone has already been described in the instance of the first operation which I performed. I now give an equally characteristic instance of the "numerous gall-stones:"—

On December 20, 1881, Dr Lycett of Wolverhampton brought to me a lady, aged 28, suffering from severe paroxysmal pain in the right side, associated with a tumour, which appeared and disappeared. It was found to be movable, situated on the right hypochondrium, and was, in my opinion, distinctly cystic. In spite of the fact that it had been pronounced to be a "floating kidney" by several distinguished authorities, I gave the opinion that it was a gall-bladder distended by an impacted gall-stone, and I advised the operation of cholecystotomy.

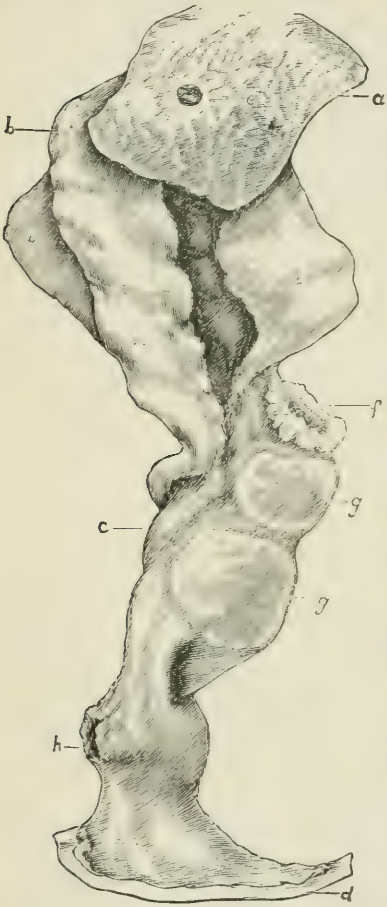


Fig. 4.—*a.* Skin with biliary fistula *e*; *b.* Gall-bladder; *f.* Distended entrance of hepatic duct; *g.* Large gall-stones occupying common duct; *h.* Post-mortem opening into common duct to exhibit another group of gall-stones *in situ*; *d.* Cut edge of duodenum.

Dr Lycett has favoured me with the following history of the case:—At the time of puberty she began to suffer from pain in the hepatic region, varying in degree and duration, but generally sudden in its onset, apt to be induced by exercise of any kind, and in this way it prevented her from engaging in dancing and other pastimes. She first came under Dr Lycett's care in 1878, at which time he came to the conclusion that she was suffering from stone in the gall-bladder. One remarkable feature of the case has always been a perfect freedom from jaundice. She had her second child in 1880; and, after that, Dr Lycett saw reason to alter his view of her case by the discovery of a freely movable tumour in the upper and right part of the abdomen. It seemed solid, and was shaped like a kidney. It was tender on pressure, and, when the hips were raised above the shoulders, it could be made to disappear. The patient stated that sometimes it could be felt below the umbilicus, but Dr Lycett was never able to satisfy himself of this. He came to the conclusion that it was the right kidney floating, and Dr George Johnson expressed a belief that, in addition to this, there must be a calculus in its pelvis to account for the paroxysmal pain. No abnormal condition of the urine was ever found.

After the birth of her third child she became much worse. Her sufferings were often intense, so that she suffered from serious symptoms of collapse, and she became very thin and anaemic. "Under such circumstances," writes Dr Lycett, "I advised her to seek your advice, with the object of submitting to abdominal section, in the hope of that possibly affording relief. To this measure, however, she took some six months to make up her mind; but finally consented when she had become a chronic invalid, and almost bedridden."

I saw her with Dr Lycett in December 1881, and was fortunate in discovering the tumour at once. It seemed to me to be cystic, to be attached above, and therefore I pronounced it to be the gall-bladder distended, on account of the occlusion of the duct by a calculus. I put the floating kidney theory altogether on one side. In fact, I have but a scant belief in its existence as a pathological incident.

I proposed to open the abdomen and remove the calculus, but, as Dr Lycett says, she took six months to make up her mind to the operation. She came back to me early in June very much reduced in health, and on the 15th I opened the abdomen by a vertical incision over the tumour. I came at once upon it, and found it to be the gall-bladder distended. I emptied it by the aspirator, removing about a pint of thick glairy mucus; then laid it open, and removed about eight gall-stones of small size, the largest weighing 15 grains. They were removed chiefly by the use of a curette. I then stitched the aperture in the gall-bladder to the wound in the abdominal wall, carefully closing the peritoneum, and leaving a drainage-tube in the gall-bladder. The patient's recovery was uninterrupted; the highest temperature recorded was 100°·4, and in the highest pulse record 84. The stitches were removed on the eighth day, the drainage-tube on the twentieth day, and in ten days more only a small sinus was left, from which some mucus still continued to be discharged. The patient has gained flesh since the operation, and has been entirely free from pain.

As an illustration at once of how impossible accurate diagnosis within the abdomen is, and of the extraordinary distension to which an organ may be subjected, let me give the following case:—

M. S., aged 40, was sent to me in November 1886 by Dr Bond of Brinklow, with a large abdominal tumour, which presented all the appearances and physical signs of a parovarian cyst, and as such I had no hesitation in regarding it. I opened the abdomen on the 20th of the month by an incision about 2 inches long. On emptying the tumour I found, to my amazement, that it was a gall-bladder enormously distended. I enlarged the abdominal aperture and the opening into the cyst to a sufficient size to allow my hand to enter, passing it up towards the liver. I found a gall-stone as large as a filbert nut impacted in the neck of the gall-bladder. This I removed with a great deal of difficulty, and I also found in the cavity two or three smaller calculi. I stitched the opening of the gall-bladder to the opening in the abdominal wall, which, of course, had been made in the original position in the middle line close above the pubis, and I then inserted a drainage-tube. Bile began to flow from the wound on the morning of the third day after the operation. I removed the stitches and drainage-tube on the sixth day, and the patient left the hospital with the wound perfectly healed on 12th December. I saw her on 17th January 1889, and she had remained in perfect health, with the wound perfectly closed, and not the slightest trace of her old trouble.

The interest of this case consists, first of all, in the enormous distension of the gall-bladder, for the measured quantity of its contents amounted to nearly 11 pints, consisting of the usual clear gluey fluid. It certainly is an astounding record to make that a distended gall-bladder was mistaken for a parovarian cyst, and that the operation of cholecystotomy was performed by an opening in the middle line below the umbilicus.

SEVENTEEN CASES OF EXPLORATORY INCISION.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Disease.	Date.	Recovered.	Died.
1	Hinckley	Dr Bradshaw Smith	27	M	Miliary abscess of liver	July 2, 1883	R	
2	Birmingham	Dr Van Vestrant	30		Tumour of liver	Jan. 12, 1884		
3	Sandowne	Dr Coghill	21	S	Hepatic colic	April 24, „	R	
4	Ontario	Dr Griffin	63	M	Tumour of gall-bladder	..	R	
5	Leamington	Dr Douglas	45	M	Tumour of liver	April 1, 1885	R	
6	Droitwich	Dr Fayrer	24	M	Cancer of liver	Aug. 24, „	R	
7	Leamington	Drs Thursfield and Haynes	53	M	Cancer of gall-bladder	Sept. 21, „	R	
8	Wakefield	Dr Horsfall	45	M	Tumour of liver	Feb. 6, 1886	..	D
9	Dublin	Dr Denham	54	M	Cancer of liver	Oct. 30, „	R	
10	Newton Abbott	L. T.	28	S	Tumour of liver	May 23, 1887	R	
11	Birmingham	Dr Nelson	53	M	Cancer of gall-bladder	June 8, „	R	
12	Lincoln	Mr Boulton	51	M	Chronic liver colic	Nov. 5, „	R	
13	Stratford-on-Avon	Dr Greene	53	S	Cancer of liver	Aug. 24, 1888	R	
14	Birmingham	Mr Patent	34	M	Tumour of liver	Jan. 18, 1889	R	
15	Leicester	Dr Johnstone	43	M	Cancer of liver	March 25, „	R	
16	Ilfracombe	Dr Toller	58	M	(?) Tumour of liver	June 26, „	R	
17	Tipton	Dr Underhill	..	M	Cancer of liver	July 19, „	R	

SEVENTEEN CASES OF HEPATOTOMY.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Disease.	Date.	Recovered.	Died.
18	Malvern	Dr Thelwell Pike	57	S	Hydatids of liver	July 16, 1880	R	
19	Birmingham	Dr J. P. Hadley	56	M	Cyst	Feb. 6, 1881	R	
20	Leamington	Dr Thompson	28	M	Hydatids	Feb. 9, „	R	
21	Leamington	Dr Tomkins	21	S	„	Feb. 15, „	R	
22	Birmingham	Dr Welch	7	S	„	May 20, „	R	
23	Maertiniog	Dr Roberts	63	M	„	Aug. 15, „	R	
24	Barmouth	Dr Williams	25	M	„	Nov. 4, „	R	
25	Birmingham	Mr Jenner Hogg	22	M	„	Oct. 7, 1882	R	
26	Smethwick	Dr Pitt	38	M	Tumour	Jan. 5, 1883	R	
27	Coventry	Dr Brown	59	M	Gall-stones	Feb. 14, „	R	
28	Nice	Dr Tarnier	38	S	Hydatids	May 7, „	R	
29	Pershore	Dr Grewcock	49	M	Cyst	Aug. 18, 1885	R	
30	Birmingham	Dr Suckling	18	S	Hydatids	Dec. 5, „	R	
31	Nottingham	Mr Brookhouse	36	M	Suppurating hydatid cyst	Dec. 2, 1886	..	D
32	Small Heath	L. T.	39	M	Hydatids	Sept. 14, 1888	R	
33	Huddersfield	Dr T. K. Clarke	..	..	Abscess	Nov. 7, „	..	D
34	Coventry	Dr Pickup	40	M	Gall-stones in the liver	Jan. 24, 1889	R	

FIFTY-FIVE CASES OF CHOLECYSTOTOMY.

No.	Residence.	Medical Attendant.	Age.	M. or S.	Disease.	Date.	Recovered.	Died.
35	Bridgnorth	Dr Colles	40	M	Gall-stones	Aug. 23, 1879	R	
36	West Bromwich	Dr Browne	55	S	Suppurating gall-bladder	Oct. 10, 1881	R	
37	Wolverhampton	Dr Lycett	24	M	Gall-stones	June 15, 1882	R	
38	Birmingham	Dr Hickinbotham	39	M	„	Oct. 13, „	R	
39	Oxford	Dr Smith	28	S	Distended gall-bladder	Jan. 5, 1883	R	
40	Birmingham	Dr Leech	35	M	Gall-stones	May 6, „	R	
41	Birmingham	Dr Drury	42	M	„	May 10, „	R	
42	Birmingham	Dr Foster	66	M	Distended gall-bladder	May 28, „	R	
43	Wotton-under-Edge	Dr Forty	44	M	Gall-stones	Nov. 14, „	R	
44	Birmingham	Mr Hugh Thomas	44	M	„	Dec. 20, „	R	
45	West Bromwich	L. T.	62	M	„	May 8, 1884	R	
46	Winchcombe	Dr Cox	36	M	„	June 5, „	R	
47	Wotton-under-Edge	Dr Forty	44	M	Gall-stone crushed	June 26, „	R	
48	Kettering	Dr Thomas	63	M	Gall-stones	Aug. 7, „	R	
49	Walsall	Mr Littlewood	50	M	„	Jan. 13, 1885	R	
50	Birmingham	Dr Drury	47	M	„	March 26, „	R	
51	Bristol	Dr Aust. Lawrence	31	M	„	June 11, „	R	
52	London	Drs Hoggan and Fyfe	48	M	Cholæmia	Oct. 3, „	R	
53	Birmingham	Drs Drury and Rogers	48	M	Gall-stones	Oct. 14, „	R	
54	Birmingham	Dr Hoare	32	M	„	Nov. 11, „	R	
55	Birmingham	Dr Quirke	57	M	„	Dec. 10, „	R	
56	Edinburgh	Prof. Grainger Stewart	43	M	„	Dec. 21, „	R	
57	Wotton-under-Edge	Dr Forty	47	M	Gall-stone crushed	Jan. 28, 1886	R	
58	London	Dr Swift	45	M	Gall-stones	May 8, „	R	
59	Stratford-on-Avon	Mr J. J. Nason	49	M	„	May 26, „	R	
60	Wolverhampton	Dr Lycett	53	M	„	June 4, „	R	
61	Birmingham	L. T.	60	M	„	June 23, „	R	
62	Rhyl	Dr Carstairs	56	M	„	July 12, „	..	D
63	Cardiff	Dr Vachell	24	M	„	July 17, „	R	
64	Stonehouse	Dr Watters	37	M	„	Aug. 3, „	R	
65	Stour Village	Dr Eager	61	M	Suppurating gall-bladder and gall-stones	Sept. 11, „	..	D
66	Birmingham	L. T.	39	M	Gall-stones	Oct. 26, „	R	
67	Coventry	Dr Bond	40	S	„	Nov. 26, „	R	
68	Sheffield	Dr Harrison	44	M	Cancer of gall-bladder	Dec. 6, „	..	D
69	Birmingham	Dr Thomas	62	M	Gall-stones	Dec. 14, „	R	
70	Wolverhampton	Dr Lycett	43	M	„	Jan. 17, 1887	R	
71	Hawick	Dr MacLeod	42	M	„	Feb. 19, „	R	
72	Birmingham	Dr E. L. Freer	46	M	„	March 4, „	R	
73	Londonderry	Mr Bernard	56	M	„	April 18, „	R	
74	Birmingham	Mr Hadley	40	M	„	May 20, „	R	
75	Kidderminster	Mr Stretton	55	M	„	June 29, „	R	
76	Birmingham	Dr Leech	43	M	„	July 26, „	R	
77	Willenhall	Dr Tonks	58	M	Suppurating gall-bladder and gall-stones	Dec. 14, „	R	
78	Co. Down	Dr Cuming	34	M	Gall-stones	April 29, 1888	R	
79	Coventry	L. T.	29	M	„	May 4, „	R	
80	Birmingham	Dr Nicholls	52	M	„	June 7, „	R	
81	Longton	L. T.	43	M	„	July 10, „	R	
82	Huddersfield	Dr T. K. Clarke	..	M	„	July 23, „	R	
83	Nottingham	Dr Michie	37	M	„	July 24, „	R	
84	Birmingham	Dr Ord	30	M	„	Sept. 18, „	R	
85	Wimborne	Dr Crespi	40	S	Malignant disease	Sept. 27, „	R	
86	Bridgnorth	Mr Rhodes	54	S	Gall-stones	Jan. 8, 1889	R	
87	Hull	Dr G. P. Hardey	49	M	„	Jan. 13, „	R	
88	Chesterfield	Dr Palmer	25	S	„	May 25, „	R	
89	Birmingham	Dr Kingsland	31	M	„	June 18, „	R	

(*To be continued.*)

II.—THE RATIONAL TREATMENT OF PRIMARY SYPHILIS.

By ALBERT WILSON, M.D., Leytonstone.

SYPHILIS is a subject always interesting for discussion, as there is such variety of opinion, and as yet no perfect method of treatment.

Dr Gowers's remark in his able lectures, "Once a syphilitic always a syphilitic," has excited some correspondence in opposition to his assertion. I cannot see how any one can profess to cure syphilis unless the patient is watched to the end of his lifetime and his progeny carefully examined. This, of course, is impossible. Yet how often, when we meet a case in which we suspect syphilis as the cause of the disease, or even in tertiary syphilis, the patient assures us of good treatment and satisfactory recovery from the syphilis.

Take, for example, a case I recently treated of syphilitic ulceration of the larynx, and the patient thought himself cured of syphilis twenty years ago. Our means of observation are therefore too limited to be able to contradict Dr Gowers's woeful statement.

The *treatment* of syphilis is practically no further advanced than in the days of John Hunter (*vide* Hunter's *Treatise on Venereal Disease*, 1810, "On the Cure of Chancres by Mercury given Internally.") It may be safely asserted that the treatment of syphilis is empirical.

As to the use of mercury we have a large variety of its salts at our disposal, and of course equal variety of opinion as to which is the best, and usually one particular salt has for a time a great run. One of the most efficient preparations I have seen is the peptone of mercury. Neumann and others in Vienna used it, injecting it hypodermically on the back two or three times a week, producing good effect without constitutional disturbance.

Bamberger's formula was—Peptone, 1 gram, dissolve in water; add 5 per cent. solution of corrosive sublimate till 1 gram of corrosive sublimate has been added. Dissolve the ppt. so formed by adding to the original a 60 per cent. solution of chloride of sodium. Add distilled water till the solution contains 1 per cent. of corrosive sublimate and 1 per cent. of peptone. Dose—·01 of a gram of corrosive sublimate.

Among the English remedies, the present fashion is gray powder. I wish to say a little about this modest little drug, which till lately was almost committed to oblivion.

What is this gray powder?

Dr Ringer in his large work on therapeutics says a great deal about this drug in infinitesimal doses, and also in connexion with syphilis. He says (Ed. 8, p. 264), "Those preparations which salivate quickest manifest the greatest power over the disease, and hence the *metallic* and mercurous preparations, as *gray powder* and calomel, are preferred by some to the mercuric, as corrosive sublimate." Ringer therefore considers gray powder as metallic mercury with chalk. Yet he does not give any account of the action of the metal mercury as a drug, although on the same page he says, "Mercury may accumulate, so it is said, in globules in the cancellous tissue of bone."

Scoresby-Jackson says (Ed. 3, p. 282) of gray powder that it contains "chalk, with finely divided and probably more or less oxidized mercury."

Scoresby-Jackson also says (p. 280), "Pure metallic mercury is probably inert in the system, and only becomes operative when it is oxidized or salified."

Such are the opinions of two of the greatest therapeuticians of the age. What, then, are the facts?

Metallic mercury can only be oxidized at a temperature of 300° F., and it is not acted upon by hydrochloric acid, therefore there is not much chance of its being oxidized or salified inside the human frame.

Then is gray powder a "metallic" preparation, as Ringer asserts? It so happens that though mercury is not oxidized by air at the ordinary temperature, yet if it be triturated with any powder which has no chemical action upon it, the minute globules of mercury become oxidized. Possibly the heat of the friction contributes to this chemical change.

Blue pill is made by rubbing mercury with powdered liquorice root. Therefore blue pill and gray powder do not contain mercury in a fine state of division, but an *oxide* of mercury. Now the oxides of mercury are unstable, and are probably acted upon by the hydrochloric acid of the stomach to form soluble oxychlorides. As the blue pill resembles the gray powder, it is quite possible that the former, if given in smaller doses, might again be favoured with an exalted position in medicine instead of being banished as at present.

I believe there is also an error about the action of calomel. It is commonly said that the hydrochloric acid of the stomach converts the calomel into mercuric chloride before absorption can occur. It happens that calomel is insoluble in hydrochloric acid, but is decomposed by alkalies. As the saliva is alkaline, containing potash salts—*e.g.*, potassium sulphocyanide—it is probable that the calomel is decomposed in the mouth; probably the suboxide is formed, and this in its turn may form an oxychloride. Thus ($2\ HgCl + KHO = Hg_2O + HCl + KCl$), the KHO being replaced by the particular salt which may occur in the saliva; if the sulphocyanide, then there would be sulphocyanic acid instead of HCl.

If the above is correct, it will be evident that calomel will act better if given dry on the tongue instead of in water or pill when it escapes the saliva. In such cases the calomel would remain *inert in the stomach*, but be decomposed by the alkaline pancreatic secretion.

This theory would also suggest that for rapid action calomel ought to be placed on the tongue and sucked. But further, it may lend some explanation about *salivation*, which may be largely due to the *irritant local action* of soluble mercury salts in the mouth. The tonsils have been shown to absorb rapidly the fluids in the mouth, and this may also lead to excessive mercurialism. If this is the case, then mercurial salts had better be introduced into the system hypodermically, or by vapour baths, or by coated pill, so as to escape the mouth, especially if calomel is the salt to be given.

The skin seems to have the power of absorbing mercury by inunction or by mercurial plasters, and this refers also to non-syphilitic affections, such as boils, painful "blind boils," or any hard inflammatory swelling. I commonly use the gray plaster, which contains the black subsulphide of mercury, and strongly recommend it to relieve the pain and disperse the swelling. How these drugs act no one can say, but such terms as absorbent and eliminative, alterative, etc., are still used, though they explain nothing.

If we wish to advance therapeutics as a science, we must expect more accuracy from some of our great teachers; also, more attention ought to be paid to terms. One would never find in a work on chemistry the term "mercury" used where it should be "a salt of mercury." If scientific men are accurate, why should not we be if therapeutics be a science?

In the year 1878, at Edinburgh, I ventured to depart from the orthodox treatment of primary syphilis. When viewing a hard chancre it occurred to me that apparently the full force of this dread malady resided in that small lump, and that by excising the chancre I might check the disease and deprive it of its virulence. I did so, and the man recovered without any bad symptoms, and for the next few months showed no trace of syphilis. Similarly I treated two, and I believe three cases. Since that year I have treated, I believe, three cases by removing the hard chancre. I have operated usually by circumcision, or by removing healthy prepuce by a V-shaped incision some distance from the chancre. I use an antiseptic wash, but more lately iodoform with powdered boracic acid, as I find great advantage in keeping the wound dry. The wound heals just as in an ordinary circumcision, without any tendency to ulcerate.

Some months ago I removed a very bad chancre where the irritating discharge had caused an indurated ulceration on the under surface of the penis. The circumcision healed quickly, and the ulcer beneath slowly disappeared with iodoform, which is a specific in venereal sores. The subsequent history in all these cases is negative. No secondary symptoms developed in the few months that the early cases were under observation, and the same may be said of the others. I followed no antisyphilitic treatment beyond some preparation of mercury while the wound was healing.

I most strongly recommend all chancres to be treated in this way if the situation permit. It can do no harm, and, for aught we know, may entirely eradicate the syphilis if done early. But my recent case was done three or four weeks after the chancre appeared and ten weeks after the syphilis was contracted, and yet at present he is well.

John Hunter mentions a chancre appearing twenty-four hours after impure intercourse, and another two months after. We are still in ignorance as to the nature of a chancre. Where is the syphilis during the incubation period? Is it at the seat of the inoculation, or is it in the blood? And where is the syphilis when the chancre is out? Is it general or local? Are we not led by analogy to hope that it is local by the study of anthrax, where the whole virulence of the disease is accumulated in the carbuncle? If we save a life by excising or destroying the carbuncle, which otherwise would act as the focus of a fatal disease, is it not probable that by excising the focus of syphilitic infection we may save the patient from subsequent syphilitic poisoning? It seems, at all events, the most rational method of dealing with primary syphilis, and, as far as I have been able to observe, has prevented the development of secondary symptoms, and if this be so, probably there would be no tertiary symptoms or nerve lesions; but on this subject there is no evidence as yet. It promises, however, to cure by arresting the development of a lifelong disease in the same way that excising the bite of a rabid dog cures by arresting the development of hydrophobia, and as such I most earnestly commend this method to everyone who is called on to treat a hard chancre.

III.—THE CŒLIAC AFFECTION IN CHILDREN.[1]

By R. A. Gibbons, M.D., M.R.C.P., Physician to the Grosvenor Hospital for Women and Children, Physician for the Diseases of Women and Children at the St George's and St James's Dispensary.

The subject of this paper is one which has not been described, to my knowledge, in any of the text-books, either on general medicine or on the diseases of children. As it is one of great importance, and one which is readily overlooked, even by excellent physicians, I have thought that it would be of interest to publish a few of the cases which have come under my own observation of this somewhat uncommon disease. These cases are very similar, and it is therefore unnecessary to burden my paper with more than four, which will serve as examples of all.

---

[1] This title is adopted by Dr Gee, who was the first to direct my attention to the disease, at the Hospital for Sick Children, Great Ormond Street, and I have therefore thought it best to retain it.

The name "cœliac" is derived from the Greek κοιλια, which means belly. The "cœliac flux" is a well-known and old expression signifying diarrhœa, in which the undigested food is passed from the bowel. The name of the affection under consideration does not necessarily imply diarrhœa, and is taken for want of a better one; it means merely that the disease pertains to the abdomen. It has, however, the merit of not conveying anything actually erroneous. The disease is not distinctly a diarrhœa, although undoubtedly children suffering from this affection are liable to attacks of diarrhœa. In the latter affection, if the larger bowel is affected, we get dysenteric symptoms; if the smaller bowel is the seat of trouble, it is generally considered that we have three different conditions affecting it, namely, increased movement, increased secretions, or a diseased condition of the mucous membrane. The cœliac affection corresponds with neither of these conditions, as will presently be shown.

The following cases are epitomized from notes taken of some of the patients who have been under my care in private practice:—

CASE I.—J. F., a boy, aged 2 years, was first seen on the 26th of April 1883. He was lying in his nurse's lap, looking flushed, and complaining of pain over the abdomen. He had been languid and vomiting during the morning. I ascertained that the motions had been copious, frothy, and offensive. He had been latterly losing flesh. As to the previous history, I learned that the child had been brought up by hand, and there was nothing of importance to relate until last December, since which time he had been out of health, and seemed to be gradually getting worse. In that month they had sought advice, and he had caused his parents anxiety ever since, sometimes being a little better, at other times worse. On examination I found that the temperature was over 99°, the pulse 108; but no definite disease could be made out. The examination of the chest yielded nothing. The abdomen was soft and doughy, but painful to the touch, and he cried out when any attempt was made to palpate. The spleen and liver were not enlarged, and I could detect no enlarged lymphatic glands. The flesh was soft and flabby. As he was having rather frequent actions of the bowels, a mixture of bismuth and compound tincture of camphor was ordered.

His diet consisted of cream, with two or three times as much water or scalded whey. For a drink at breakfast, at 11 o'clock in the morning, and at bedtime, whey made with essence of rennet; if mixed with cream, it was to be scalded. Pounded raw meat prepared in the ordinary way, and rubbed through a sieve; of this one, two, or three tablespoonfuls to be taken for dinner, with a little sifted sugar or fruit jelly. A tablespoonful of mashed floury potato. Rusks, plain biscuits, or toast with fresh butter; Savory & Moore's food made with whey, and a teaspoonful of cream added. Half a pint of asses' milk in the morning, and the same quantity in the evening. One or two teaspoonsful of raw meat in the after part of the day.

The following day I carefully examined some motion microscopically, and could detect no pus and nothing extraordinary.

Family History.—The mother was a healthy woman. She had had no miscarriage. She had no history of any important disease. There was no trace of tubercle on her side. Her mother suffered occasionally from gout. The child's father appeared a healthy man; he occasionally passed free uric acid, and had symptoms rather like those of renal calculus. There was no history of syphilis obtainable, and no reason to suspect it. There were two other children, a boy and girl, older than this patient. The girl had suffered from suppuration of the submaxillary glands.

The child improved considerably until 5th July, when he had an attack of watery diarrhœa, which prostrated him. The motions were extremely offensive. Dr Gee saw him with me, and suggested, instead of the compound kino powder which he was taking, that he should have minute doses of mercury and chalk every two hours; forty-eight hours later the diarrhœa had stopped.

Without going further into detail, it may be stated that this attack nearly proved fatal. He, however, gradually recovered, the first favourable sign being that the motion contained some solid material. There was no change in the diet, except that the meat was omitted and Brand's essence substituted.

The following month he left London for the seaside. He occasionally had attacks of diarrhœa there. Six months afterwards I again saw him, when he seemed in excellent spirits, looked fat, and, although flabby, ran about and played with the other children with apparent enjoyment.

This boy died of acute purpura hæmorrhagica, the details of which illness I have already published in the *Medical Times and Gazette* for 1885.

CASE II.—S. H., aged 2 years and 8 months. First seen on 10th May 1885. Her condition is as follows:—She is very anæmic in appearance. She is not thin or wasted, but her flesh is flabby, and her legs and arms, though apparently well covered with flesh, are soft to the touch. Nothing is to be discovered on examination of the chest, the heart sounds being normal, and the lungs acting well. On palpation of the abdomen, it is found to be doughy, blown out with flatulence, and looking very large. The liver can be felt about one finger's breadth below the edge of the ribs, and the spleen is slightly enlarged. No enlarged lymphatic glands can be made out anywhere. She walks with difficulty, and has a great disinclination to go upstairs. A very little exertion makes her tired, and break out into a profuse perspiration, especially about the head. Her appetite is poor, and it is with great difficulty that she can be coaxed to eat. Her motions are light in colour, frothy, and intensely offensive. Microscopically they contain nothing abnormal. Sometimes the actions vary from one to three in the twenty-four hours. The urine contains no albumen, sugar, or bile.

Previous History.—With the exception of an acute attack of laryngitis, there is nothing to report about her former health. She was nursed as a baby by her mother. She first began to complain in July last, but as the weather was very hot, it was thought that she was suffering merely from the effects of the heat. At that time her motions were clay coloured, and she was very pale and languid, with occasional attacks of diarrhœa; but in the beginning not much attention was paid to these symptoms. Soon after, becoming worse, advice was sought concerning her; and in a letter to me from her medical attendant, it was stated that he saw her for what he considered were symptoms of mesenteric disease, but that she greatly improved under treatment. She soon afterwards relapsed, and owing to extreme debility and pallor, her case was considered hopeless.

Family History.—Her mother is perfectly healthy, with no history of any importance. Her father is at present healthy; he suffered some years ago from abscess of the hip joint. From this he perfectly recovered, and is in excellent health. The other children, three sisters, are all healthy.

Treatment.—She was placed upon a diet exactly similar to the other cases related, but in her case it seemed necessary, on account of exhaustion, to give as an addition brandy once or twice a day. Attacks of diarrhœa were treated as they arose, but the medicine she principally took was iron in some form. Cod-liver oil could never be borne; it invariably caused looseness of the bowels. At one time she developed purpuric patches over the lower part of the abdomen, which, however, gradually disappeared; and she also had an attack of stomatitis, which lasted for about ten days. She was under observation for a considerable time during each year, and I constantly heard about her when she was not in London. The diet and general treatment were carefully attended to, and she was allowed to have chicken, eggs, tapioca, milk, and everything of a plain description. When I last heard, her condition was most satisfactory, and she had gained considerably in weight.

CASE III.—E. H. W., aged 2 years and 1 month. Her mother came to consult me about her on the 10th of May 1887, complaining that the child's digestion was out of order, and the first thing she noticed was that the motions were offensive and copious.

Previous History.—The mother declared that she considered the child had not been thoroughly well ever since she was born. She was only suckled a fortnight, after which she had cows' milk and water, then donkeys' milk for a time, and then again cows' milk.

Family History.—The patient is an only child. The mother is a healthy woman; has never suffered from any miscarriage; there is no history of tubercle or of gout. The father is a healthy man, with no history of tubercle. There is no history of syphilis obtainable, and no reason to suspect it.

The child has always been nervous and easily frightened. For several months past she has been dieted by the mother. Sometimes she would give her a little meat, and sometimes not. She often has attacks of sickness; the least thing causes her to vomit, and the food is very easily brought back in an undigested state. The motions are very large in quantity, pale in colour, and extremely offensive. On examination the child is found to be generally flabby, and does not look strong; she is pallid, but not thin, and there is no yellow tint of the skin. On examination of the chest, the right side is seen to be slightly flattened anteriorly, and the percussion note is of a higher pitch as compared with the left, probably from thickened pleura. The breath sounds are natural; the heart seems normal. The liver is one finger's breadth below the margin of the ribs. The spleen is not enlarged, and there are no enlarged lymphatic glands to be discovered. On examination of the blood, there is observed to be an excess of white cells. The red are less coloured than is natural, and there are numerous lymphoid particles present. It is spanæmic, the number of cells being greatly diminished. The tongue is white and coated. She was ordered some perchloride of iron and cod-liver oil, with occasionally some decoction of aloes, and the diet was to consist of cream, whey, also asses' milk, raw meat, and potato.

A week later, I saw her on account of pain in the abdomen. Her legs were drawn up, and she complained of great pain all over the abdomen. Her medicines were stopped, and I substituted for them a mixture containing bismuth and compound tincture of camphor, under which she improved.

On the 11th of June she had an attack of sickness. She vomited frequently, and had three copious loose motions, the last being watery. This attack was probably because there had been some mistake about the milk, the child having had cows' instead of asses' milk during the morning.

When I last heard of her from the country, I was told that she seemed very much better and improved in every way. She had, however, to be extremely careful about food, and could not make any change with impunity,—in fact, the diet had had to be altered to an ordinary one with the greatest care.

Case IV.—D. G., aged 2 years and 8 months. First seen on 9th September 1886. Her mother brought her to me, complaining of her not passing water for many hours, sometimes as many as 10 or 12 elapsing without any inclination on the part of the child to relieve herself.

Previous History.—The child had been brought up by hand, and there is nothing of consequence to report concerning her, with the exception of a severe attack of laryngitis, from which she suffered a year previously.

Family History.—The mother is a delicate-looking woman, thin, and by no means strong. There is no distinct history of phthisis on either side, and there is no syphilitic history obtainable, and no reason to suspect it. There is one other child, older than the patient, and perfectly healthy. She also had a severe attack of laryngitis when a baby, but nothing else of consequence.

On examination, she was seen to be a rather delicate-looking child, but apparently well developed, although her flesh was soft and flabby. No signs of rickets could be detected. The abdomen seemed large and doughy. The heart and lungs appeared to be normal. The spleen and liver were not enlarged, and no enlarged lymphatic glands could be felt. The urine proved to be distinctly albuminous, and to contain crystals of uric acid. The mother, who had come with her from the country, had not brought the nurse, and could tell me nothing about the motions, or whether the bowels were regular or not. She was given a mixture containing some sulphate of soda and citrate of potash. I soon afterwards received a letter telling me that the motions were copious, frothy, whitish, and intensely offensive, and that the bowels acted once or twice daily. On the 26th October I again saw her, and then heard from the mother that the child had been very poorly a fortnight previously, owing to what was considered due to a chill. She told me that the child was seized with what she termed "spasms of the abdomen," that she drew her legs up, and seemed in great pain. This attack of pain apparently lasted for several hours, and the mother states that she was not feverish. At that time the motions were too light in colour; they were very large, and extremely offensive. She passed a slight amount of blood with the motion, but the mother is not quite sure whether this was merely from straining at the first, or whether it was mixed with the motion; probably the former would account for it, for I never again heard of the presence of blood. The nurse said that the motions were so large in quantity that they quite astonished her, considering the amount of food eaten; that they were intensely offensive, and that there were never more than two in the twenty-four hours, although usually there was but one.

There being no doubt as to the diagnosis in this case, I ordered the same diet as in the previous cases, viz., cream and whey, pounded raw meat with sifted sugar and floury potato, and to have some compound decoction of aloes and cod-liver oil. On the 4th of November I saw her in what her mother described as her "stomach attacks," and I then found that she appeared to have considerable abdominal pain, with the legs drawn up. I therefore omitted the compound decoction of aloes and the cod-liver oil, and gave a mixture containing some compound tincture of camphor. On a microscopical examination of the motions, it was found that they contained no pus. There were many long, rod-like vibrios, and very numerous spores, like the pencilium glaucum.

It may be briefly stated that she was kept upon this diet for many months; that the attacks of pain from which she originally suffered gradually diminished in intensity until they entirely ceased, and that her general condition greatly improved. The albumen disappeared from the urine; she became more muscular and walked without fatigue, and the motions gradually assumed a natural colour. This improvement, however, did not take place rapidly, but occurred during a period of about two and a half years. When I last heard of her, her condition was satisfactory, and although she could not be considered to be a perfectly robust child, there seemed to be nothing amiss with her. She ran about and played like other children, and seemed well. Her motions have always been natural for months past, and she has gradually returned to ordinary every day diet; but the nurse has been extremely careful in giving her only what has been ordered for her, and she has at all times been cautious not to give her anything which she feared would in the least disagree with her. The child has, therefore, had every possible care regarding her treatment.

The cœliac disease, amongst hospital patients, is popularly called "consumption of the bowels," meaning by this one of the several affections to which they apply this term. This disease is characterized by the passage of large, loose, white or whitish, frothy, intensely fœtid motions; by pallor of the skin; wasting; loss of muscular strength, and by the fact that it is most frequently met with in children between 1 and 5 years of age. The youngest child I have seen attacked by it was between 10 and 11 months old. I have seen it in children with perfectly healthy parents, and, indeed, with sisters and brothers in every way healthy. In only one family can I trace any history of struma on the side of the father. No history of tubercle can be traced in the family of any of the patients whose cases are related, except one, and in that, consumption occurred in a paternal uncle of the patient, and in all there is no history of syphilis obtainable, and no reason to suspect it. In most of the cases there is a history to be obtained of gout, either on the side of the father or mother, or in distant relations. The examination of the fæces themselves throws no light upon the matter. In those I have submitted to careful microscopical examination I have never been able to discover anything abnormal, and I have failed to detect any tubercle bacilli although carefully searched for.

Pathology.—Under this head there is really nothing to be detailed. Examination of the bowel after death has led to the discovery of nothing. There is no sign of wasting of the mucous membrane, of engorgement, or of ulceration, and this statement refers to the whole of the intestinal tract. I am not aware that any wasting of Lieberkühn's follicles has been detected. The liver and the spleen are not remarkable for anything abnormal. The spleen, however, is occasionally enlarged. The blood usually has signs of anæmia, and except for the excess of small lymphoid particles, might pass for ordinary anæmic blood. As, however, these small particles are not peculiar to this affection, they really need hardly be noticed, for I have myself found them, on many occasions, especially in children suffering from empyema. Therefore, taking all the facts into consideration, it is quite clear that up to the present moment nothing has occurred to throw any light upon the pathology of this extraordinary affection.

Diagnosis.—About this there can be no difficulty whatever, for the signs and symptoms are so clear that they cannot be mistaken. It is, however, of the greatest importance to examine the motions for one's self, for I am certain that if this had been done in several of the cases which have come under my notice, I should not have been the first to discover that the child was suffering from anything beyond a little general debility. About this point one cannot be too strong, for, as there are usually no signs of ordinary diarrhœa, and possibly but one action a day, the nurse, if not alarmed by the fœtor, is apt to think nothing of the bowel, but to be more struck with the pallor and the generally increasing weakness of the child. The signs are so clearly indicative of this one affection, that there is no other with which it could be confounded.

Prognosis.—Unfortunately this is usually grave, and the majority of children suffering from this disease die. I have, however, with the exception of one, in the cases that have come under my observation, had the good fortune to witness their improvement; and I think it was partly due to the careful nursing that they had in their own homes, and to the fact that they have been the children of people who have been well off, and who have been able to give them everything that has been ordered, regardless of expense. I feel certain that this has a great deal to do with the well-being of the children, for, in one case, the second on the list, I am sure that without the most constant care and watching on the part of the mother and nurse, she would have died. As to the time at which death will take place, it is impossible to say, for, however bad these little patients are, they frequently have extraordinary rallies, and seem better for a time; but, without any warning, relapses take place, and after apparently progressing most favourably, they suddenly become exhausted, and rapidly fade away, usually from attacks of watery diarrhœa.

Symptoms and Progress.—The onset is usually gradual, and there is nothing definite to call attention to the disease. The nurse or the mother generally states that the child has been a little out of health for some weeks, or possibly months, and yet there has been nothing special to cause alarm, with the result that no treatment has been adopted. The child becomes pale and anæmic, the blood evidently being affected, and sometimes there is a puffiness about the face. Possibly amongst the first indications, the motions have been remarked by the nurse, and this has generally been owing to the colour being different, but above all, on account of their intensely fœtid odour. This mal-odour is sometimes overpowering, and those who have not encountered can hardly realize it. The motions themselves, as already pointed out, are very large, soft, whitish in colour, and frothy. They resemble very much ordinary oatmeal porridge. The actions may be two or three in a day, but usually there is only one large one, and that may take place at a time to which the child is accustomed. Now and then there are attacks of so called diarrhœa, that is to say, the child may have frequent evacuations of the bowels, that they may be very loose, and that they may be occasionally watery if the attack is severe, but these attacks of diarrhœa are not an essential part of the disease, except in the cases of some children who have these constant attacks of diarrhœa throughout the whole of the illness. The appetite is poor, usually capricious, sometimes large. The tongue is frequently coated with a whitish fur, at other times perfectly clean. In some cases there are attacks of crampy abdominal pain, possibly due to ordinary colic. These attacks were well marked in case 4. The abdomen is generally soft and doughy, although occasionally distended and hard. The liver is generally unaffected: now and then the spleen is found to be enlarged: no enlarged lymphatic glands can be detected. As a rule, the urine has nothing remarkable about it; occasionally it contains a trace of albumen. The temper of the child seems variable, most frequently he is extremely irritable, fretful, capricious, or peevish. Nothing seems to please him, and altogether he is quite unlike himself. Sometimes he is heavy and languid, complaining of headache, and wishing to lie down a good deal. The special senses seem to be normal. In all the cases that I have examined the various tendon reflexes were unaffected.

The most remarkable sign about these children is that of loss of muscular power, with apparently no great loss of flesh; that is to say, to look at, the children do not seem to have wasted much, but on feeling the legs or arms, it is at once evident how soft and flabby and out of condition they are. A curious point is that they have a great objection to going upstairs, and in one case, one of the first symptoms which struck the nurse as being extraordinary was that the child did not care to mount the staircase, and that she had invariably to be carried up. The progress of this affection is as a rule slow. The children seem to gradually become more feeble, to waste away, and to lose power. Occasionally an attack of watery diarrhœa, in those children who have been accustomed to suffer from diarrhœa, may carry off the patient. In the case of colicky attacks I have seen a child in imminent danger from collapse. In non-fatal cases recovery is extremely gradual. An important point to note in this connexion, is the tendency to relapses, each relapse taking a considerable time to make up for lost ground. When improvement begins to take place, it is usually shown in the healthier condition of the evacuations. The colour becomes better, the motions are more formed, and above all, less copious and less offensive. The child gains in strength, but is a long time before improving in colour. The muscular strength is the last to return, and mounting the stairs, or jumping, are amongst the very last things that can be done. The appetite generally improves but slowly, and is always capricious, or nearly so. Another important point concerning these patients is that the least thing apparently upsets them, and the smallest error in diet will bring on copious actions of the bowels, cause vomiting, or make the child quite ill again.

(*To be continued.*)

IV.—HYPNOTISM, OR PSYCHO-THERAPEUTICS.

By R. W. Felkin, M.D., etc., Lecturer on Diseases of the Tropics and Climatology, Edinburgh.

(*Continued from page* 249.)

Having now cleared the way, it is time that we should explain how persons are hypnotised at the present day, before proceeding to point out the therapeutic uses and possible dangers of hypnotism, and to investigate as far as possible the theories which have been brought forward to explain its action.

We must, however, first inquire as to the number of persons who are susceptible to the hypnotic power. It has often been asserted that hysterical people, or excitable and nervous races, provide the majority of hypnotisable subjects. This is a mistaken idea, for it is found that, in all races and at all ages, the majority of persons can usually be hypnotised, although not all with the same facility. English, Germans, Dutch, and Swedes are more difficult to hypnotise, as a rule, than are the French, Spaniards, or Italians; but this refers to hypnotism acting immediately, or, in other words, on the first attempt, for it is found that in numerous cases in all countries three, four, or five attempts have sometimes to be made before success is attained, and, be it noted, a good deal depends upon the experience of the mesmeriser, as the following figures will show. When Forel, professor of psychology at Zurich, published his first report on treatment by hypnotism, he stated that of the 41 persons he attempted to hypnotise he had failed with 14. In his second report on 58 cases, he had failed in 11, but in his third report on 29 cases he had only failed in 3.

Baierlacher found 121 persons susceptible to hypnotism out of 146 he tried to hypnotise, and in nearly all of his failures he had only one sitting. As he justly remarks, two or three sittings are in many cases required before some persons can be hypnotised.

In giving statistics as to the percentage of persons capable of being hypnotised, authorities vary. Wetterstrand, in Sweden, found only 17 persons who could not be hypnotised in 718 cases in 1887.[1] Van Renterghem reported to the Medical Congress at Amsterdam in October, 1887, that he had only failed to hypnotise 9 out of 178 patients.[2]

Of 1014 patients submitted to hypnotisation by Dr Liébault in Nancy in 1880 only 27 failures occurred.[3] Beaunis in 1884–85, only failed to hypnotise 60 out of 753 persons.[4] Bottey says that 30 per cent. of all cases can be hypnotised; Morselli, 70 per cent.; Liébault, 92 per cent.; and Bernheim states that hospital surgeons who cannot at least hypnotise 80 per cent. of their patients have no right to express any opinion on the subject of hypnotism, with which statement Forel agrees completely.[5]

Age has certainly a great influence in rendering a person susceptible or otherwise to an hypnotic sleep. In an interesting table which Professor Beaunis has drawn up, we find that all patients under 14 can be hypnotised, and that failures occur after that age in the following proportion. From 14 to 21, 10·3 per cent. of failures; from 21 to 28, 9·1; from 28 to 35, 5·9; from 35 to 42, 8·2; from 42 to 49, 12·2; from 49 to 56, 4·4; from 56 to 63, 14·4; and above 63, 13·5. It must not be understood from this, however, that this great percentage refers to the production of absolute somnambulism (somnambulism, as will be fully explained later on, is the last stage to which hypnotism can be pushed); for although 55·3 per cent. of the cases between 7 to 14 years of age exhibited the symptoms of somnambulism, yet from 14 onwards the average number of cases in which that phenomenon was observed was 10 per cent. In other words, the greatest proportion of somnambulists is found in childhood and youth; 26·5 per cent. from 1 to 7 years, and 55·3 per cent. from 8 to 14 years. In old age the number of somnambulists decreases, but it remains at the relatively high figure of 7 to 11 per cent It has often been stated that somnambulism can more easily be induced in women than in men, but this statement does not tally with the facts, for we find that the proportion of cases in which somnambulism is observed is pretty equally divided, it being met with in 18·8 per cent. of men and 19·4 per cent. in woman.[6]

---

[1] *Wetterstrand om den hypnotiska suggestionens anvændning i den praktiska medicinen Hygiea.* Stockholm, 1888.

[2] *Beitrag der therapeutischen Verwerthung des hypnotismus.* Leipsic, 1888.

[3] *Psycho-therapeutics*, Tuckey, p. 31.

[4] *Ueber Hypnotische Suggestionen*, Sallis. Berlin, 1888.

[5] *Der Hypnotismus*, Moll. Berlin, 1889.

[6] *Suggestive Therapeutics*, Bernheim. New York, 1889, p. 20.

With regard to these statistics, it is only fair to remark that they deal with French patients, and that they occurred at Nancy, where, owing to the long-continued researches of Liébault and Bergheim, hypnotism is well understood, and the people knowing all about its action are not frightened at it. Still, almost the same percentage is obtained by Van Eeden at Amsterdam, and one cannot suggest that the Dutch are either excitable, easily impressionable, or hysterical people.

In order for a person to be hypnotised, certain things are requisite. In the first place, the patient must be willing to be hypnotised, and have perfect confidence in the hypnotiser. We shall refer later on to the possibility or not of a person being hypnotised suddenly or unwillingly. Secondly, the patient must be comfortable or at perfect ease; and, lastly, and most important of all, the hypnotiser must possess perfect confidence in himself, and the more he knows of hypnotism and the greater his experience, the more chance he has of success. All observers agree that the longer they practise hypnotism the more easily they induce hypnotic sleep in their patients, and the fewer failures they meet. The statistics given above of Forel's practice illustrate this, and it is only natural to suppose that first attempts are likely to be unsatisfactory; and just as a surgeon becomes, as a rule, a better operator the more experience he possesses, so too a hypnotiser lives and learns.

The popular idea that only hysterical persons can be hypnotised, or at any rate are far more easily hypnotised than healthy individuals, is quite wrong. Hysterical, nervous persons are, as a rule, difficult to hypnotise, and in many cases it is almost impossible to bring them under the influence of hypnotism, owing to the instability of their mental powers. Charcot's work at the Salpetrière does not cause us to modify this opinion.

It should be distinctly understood that we are referring to the ordinary hypnotising of ordinary individuals,—individuals who are not suffering either from hysteria, any nervous disease, or diminished brain power.

Although persons may be hypnotised in many ways, all the methods can be conveniently divided into two classes,—1. Hypnotism produced by physical impressions; 2. Hypnotism produced by physical suggestion; although probably in both methods a certain amount of expectation is necessary in order to obtain a satisfactory result. Dealing with the first method, we find that hypnotic sleep may be induced by the subject fixing his eyes upon an object held in such a position as to cause a convergence of the eyes, or a strain upon accommodation. Although this method is often successful, it is not free from objection, as it may easily produce excessive stimulation of the brain, giving rise to either dizziness, headache, or even convulsions.[1] Again, the object may be held above the eyes in such a position as to compel the patient to tire the eyes by the effort to keep them open. Hypnosis may just as easily be induced by getting the subject to look at the operator's finger, or to gaze steadily into his eyes.

---

[1] *Die Suggestions Therapie*, Baierlacher. Stuttgart, 1889.

Impressions produced by sound can also hypnotise a person: the attentive listening to a loud ticking watch or clock,—the monotonous, long-continued, but gentle beating of a drum—or the sudden striking of a loud gong, may all produce the same effect as will also, in some cases, the sudden flashing of the electric light into the patient's eyes. It is also possible to hypnotise a person by exerting an influence upon the tactile sense. The eyes may be closed by the fingers, and gentle pressure be exerted upon the eyeballs, or by gently stroking the skin. It is possible, too, to produce hypnotism by making passes with the hands close to, but not touching, the patient. This may act either by inducing currents of air, or by inducing an alteration in the temperature, but in all probability this method of procedure induces the sleep in a complex manner, both physical and mental stimuli being utilized at one and the same time. We may here mention in passing that Pitres holds that in many cases individuals possess what may be called hypnotic points, the pressure upon which may induce almost instantaneous sleep. These spots vary in extent from 1 to 4 c.m. in diameter. They may be on one or both sides of the body, their number varying from 3 or 4 to 30 or 40 in the individual. We will not refer more to this, as further observations are required to confirm the statements, but we may say that De la Tourette credits it, and gives a remarkable case in which a girl was accidentally hypnotised by pressure on one of these spots which was situated on her elbow.[1]

These points are found mostly in hysterical patients, and we certainly believe that mental impressions play a not unimportant part in the production of hypnotism by this method.

We now come to the method of hypnotising which we have called psychical suggestion. It is probably the best of all methods, and it is the one which is so largely practised by Liébault, Bernheim, Forel, and others. Hypnotism is induced as follows:—The patient is either lying comfortably in bed or reclining easily in a comfortable arm-chair. He is told to think of nothing, to keep the mind as blank as possible, but to fix his eyes and attention on some special object,—almost anything will do, from the operator's face or hand to a mark on the ceiling or the pattern of the carpet. Then the phenomena which attend the oncoming of natural sleep are gradually suggested to him: "Your sight is growing dim and indistinct; your eyelids are becoming heavy; a numbness is creeping over your limbs; my voice seems muffled to you; you are getting more sleepy; you cannot keep your eyes open." Here the eyes close of themselves, or are closed by the operator, and it is generally found that the patient is indeed asleep. About two minutes of this "talk about sleep" will usually produce the hypnotic effect; and on subsequent visits even less time is required.[1]

---

[1] *Untersuchung ueber der hysterogenen Punkte.* Gaube, 1882, vi. p. 58. *Der Hypnotismus*, de la Tourette. Hamburg, 1889, p. 79.

Dr Liébault, in order to give his patients confidence, and also to arouse their imitative faculties, usually allows them to see several persons hypnotised before their own turn arrives. The patients are awakened, as a rule, by simply telling them to awake. If the sleep has been very profound, however, as it is when the stage of somnambulism is reached, the order may have to be repeated several times, or the patient may be fanned, or the operator blows gently into the eyes. Liébault's explanation of his method is simple in the extreme. The patient's attention being fixed, the accommodation of the eyes is strained, the sight tired; the pupils consequently dilate, vision becomes dim, the eyes being tired naturally close, and the monotonous tones of the operator suggesting sleep to him, what wonder that it occurs?

It should be remembered that some people have no inclination to sleep, or show no drowsiness. In such cases Bernheim says, "I take care to say that sleep is not essential; that the hypnotic influence, whence comes the benefit, may exist without sleep, and that many patients are hypnotised although they do not sleep. If the patient does not shut his eyes or keep them shut, I do not require them to be fixed on mine or on my fingers for any length of time, for it sometimes happens that they remain wide open indefinitely, and instead of the idea of sleep being conceived, only a rigid fixation of the eyes results; in this case closing of the eyes by the operator succeeds better. . . . . Passes, or gazing at the eyes or fingers of the operator, are only useful in concentrating the attention, and are not absolutely essential."[2]

According to the experience of Bernheim and Liébault, common people, those of gentle disposition, old soldiers, artizans, people accustomed to passive obedience, are easier to hypnotise than preoccupied people, and those who often unconsciously oppose a certain mental resistance. The idea of being hypnotised must be present, and the patient must submit to the hypnotiser, using no cerebral resistance. It is often impossible to influence people who make it a point of honour to show that they cannot be hypnotised; in fact they oppose a kind of counter suggestion.[3]

Faria simply fixes the attention of the patient, and suddenly in a commanding voice says, "Sleep," and the patient sleeps.

Although for the sake of clearness we have separated these two methods from one another, we are inclined to think that the division is practically more imaginary than real, and we doubt very much whether it is possible, except, perhaps, in the case of very young children, to induce the hypnotic sleep, until a more or less vivid idea of the result of the method is mentally grasped by the subject.

---

[1] *Psycho-therapeutics*, Tuckey, p. 23.

[2] *Suggestive Therapeutics*, Bernheim, p. 4.

[3] *Ibid.*, p. 5.

Before passing on to mention the various degrees of hypnotic sleep, we may perhaps refer to auto-hypnotism, and also to hypnotism at a distance. With regard to auto-hypnotism, there can be no doubt it is of very rare occurrence, and it is chiefly met with in persons who have been very frequently hypnotised. It may be simply explained by stating that it is a mere effort of will on the part of the individual in question, the intense concentration of the mind abstracting it from its surroundings, and thereby inducing the hypnotic sleep. In much the same way we can explain those cases in which persons have been hypnotised through the telephone, or have fallen asleep at times suggested to them by the hypnotiser. Conscious or unconscious expectation has been aroused with the expected result.

Bournville and Regnard published, both in their *Iconographique Photographique de la Salpetrière* and in *Le Progrès Medical* (1881), their methods of producing hypnotism, and they state that in predisposed patients merely extending one hand over the head is sufficient for the purpose, and that hypnotism at a distance is the result of the imagination. On one occasion M. Bournville told a patient that from his own house he would cause her to fall asleep at three o'clock, and on the following day he learnt that at three she fell asleep. He states that he did not even think of the patient at the time when she supposed him to be exercising his mysterious influence over her, and that the effect produced was due to her own imagination.[1] When writing on the dangers of hypnotism, we shall refer to this again.

Persons may be hypnotised in varying degrees. Charcot distinguishes three degrees in hypnotised hysterical patients.[2] He says they are sharply divided, and he names them the cataleptic, the lethargic, and the somnambulistic. We will do no more than refer to them, as we do not intend to deal in this paper with the subject of hypnotism in relation to these hysterical cases.

The Nancy school, for purposes of description, divides the intensity of hypnotic influence into six degrees. This division, however, is purely theoretical, and it is merely used in order to classify each patient influenced without making a long description necessary.

1*st Degree.*—This occurs in the smallest number of cases. The patient experiences a more or less pronounced dulness, heaviness of the eyelids, sleepiness. This may last from several minutes to an hour; usually, however, it passes off as soon as the operator's influence is withdrawn. Some patients remain motionless, others move a little and change their position, but still remain sleepy.

---

[1] *British Medical Journal*, 1881, vol. i. p. 1010. See also a novel, entitled *In his Grasp*, by Esme Stuart; and *Herr Paulus*, by Walter Besant.

[2] It may be interesting to note that practically there are two distinct schools of hypnotists: the Paris school, which is headed by Charcot, and adhered to by Richer, Dumont, Pallier, Voisin, Bérillon, Binet, Féré, Tamburini, Haidenhein, Grützner, Preyer, etc., who chiefly hypnotise by physical stimulation of the central nervous system; and the Nancy school, headed by Liébault, and adhered to by Bergheim, Beaumis, Wetterstrand, Pelbouf, Liégeois, Forel, Berger, Mendel, Moll, Krafft-Ebling, etc., who hypnotise by the pyschical stimulation of the central nervous system.—*Zur Einführung in das Studium des Hypnotismus*, Maack. Berlin, 1888.

2nd Degree.—The eyes are closed, the limbs relaxed, the patients hear what is said to them or about them, they have an inclination to sleep, their brain is in the condition called hypotaxic or charmed. This degree is characterized by suggestive catalepsy.

3rd Degree.—The drowsiness is more pronounced; apart from suggestive catalepsy, the patient is capable of making automatic movements, *i.e.*, rotation of the arms, etc., suggested to him by the operator. He hears everything that is said around him.

4th Degree.—This is characterized, in addition to the preceding phenomena, by the loss of relationship to the outer world. The patient hears only what is said to him by the operator. His senses are only in communication with the operator, but are, however, susceptible of being put into relationship with any one.

5th Degree.—Light somnambulism. Patients remember, in a vague sort of way, on awaking, what has happened to them. Sensibility is destroyed; suggestive catelepsy is present; there are automatic movements; and hallucinations may be caused by suggestion.

6th Degree.—The state of deep somnambulism. It is in reality the fifth degree intensified. The memory of all that has happened during sleep is absolutely destroyed, and cannot revive spontaneously. In both the fifth and sixth degrees the patient is reduced to docile or automatic obedience. Though inert or passive if left to himself, he does as he is told,—walks, stands still, speaks, sings, laughs, dances, operations may be performed upon him without feeling, and it is in these stages that suggestive therapeutics may be employed with most advantage.[1]

One may note here that Bernheim thinks that hypnotism should not be defined as induced sleep. He believes that expression gives a too narrow meaning to the word, and leaves out of count many phenomena which suggestion can bring about independently of sleep. He defines hypnotism as "the induction of a peculiar psychical condition which increases the susceptibility to suggestion. Often, it is true, the sleep that may be induced facilitates suggestion, but it is not the necessary preliminary; it is suggestion that rules hypnotism." He believes that suggestive sleep differs in no respect from natural sleep; the same phenomena of suggestion can be obtained in natural sleep if one succeeds in putting one's self into relation with the sleeping person without waking him. This view of the subject enables him to include under the term hypnotism all the various methods which, acting upon the imagination, induce the psychical condition of exalted susceptibility to suggestion with or without sleep.

In order to give as clear an idea as possible of the symptomatology of hypnotism,—although all the symptoms are, in point of fact, due to the selfsame cause, namely, an alteration in the central nervous system,—it is well to try and divide the subject into the physiological and psychical symptoms. This, however, is not always possible, and even at the present day it is doubtful how far many of the physical symptoms are really due to hypnotism itself and not to the means used to induce it. For example, when hypnotism is induced or attempted by the concentration of the eyes upon a given point, the eyes may water, and it is an open question whether the tears thus produced are to be regarded as a symptom of hypnotism or only the consequence of the strain upon the eyes.

---

[1] Abbreviated from *Suggestive Therapeutics*, Bernheim, pp. 5 12.

It must also be distinctly understood that various symptoms may be cultivated, and therefore it is of no use to expect that each symptom we shall presently mention will be fully demonstrable in any given case. It is necessary to bear this in mind, because in so many of the cases of which accounts are published the remarkable results obtained are certainly due to repeated practice and development. One observer will be interested in observing one symptom, and in the subjects he hypnotises he will educate or cultivate that symptom, and at any demonstration he may give he will as naturally bring it prominently forward, and call attention to it, as a fond parent would get his child to repeat the best known poem in his *repertoire* if he wished to impress assembled friends with the capability of the infant prodigy.

It is not possible to assert that any symptom or symptoms will be invariably present in a hypnotised person, and it is only by knowing what hypnotism is and by having, as it were, a mental picture of the subject, that a correct opinion can be formed as to whether a person is hypnotised or not. We should naturally doubt the diagnosis of a layman,—say, in a case of typhoid fever or in an obscure case of insanity,—although he might have a fairly good book knowledge of the subject. It requires an educated and trained observer in many cases to make a diagnosis. A clear conception of a disease must be in the mind's eye, a knowledge of the methods of investigation must be at the finger's end, before a certain diagnosis can be come to with reference to any disease. It is just the same with hypnotism; it would be surprising, indeed, were it not so. We shall probably do best by saying that the first symptom of the hypnotic state is an increase of consciousness, or an increase in the power of receiving impressions or suggestions, —" vermehrte Suggestibilität," the Germans call it. Every one is capable of receiving suggestions, is capable of being influenced by either surrounding objects or impressions conveyed by gestures, tone of voice, or, more readily still, by verbal communications. In the early training of children this influence will be recognised by all. It is well known, too, that experimenters, wishing for certain results, are apt to read their experiments in a way satisfactory to their preconceived ideas. A good illustration of this may be taken from Creighton's views on vaccination; he well exhibits auto-suggestion; he has a fixed idea that vaccination is useless, and therefore all he reads or sees confirms him in his theory.[1] Again, too, expectation may exert a great influence upon the mind. Suggest an effect, and the effect is the more easily produced. It is a matter of common experience that conversation turning to unpleasant subjects will induce various sensations, from nausea on the one hand to faintness or mere creepiness on the other. Bonniot uses the following example with reference to this point:—A person is embarrassed. You say to him, "You are blushing," whereupon the blush is induced. Considering these facts, we must remember, throughout the study of the symptoms of hypnotism the possibility of imitation, suggestion, and expectation.

Passing on to the physiological phenomena witnessed in hypnotised persons, we notice that voluntary muscles are affected. The degree to which they are affected depends partly upon the method which has been used in producing hypnotism, partly on the susceptibility or otherwise of the patient. When hypnotised, some persons can move, others are apparently as passive as in deep sleep. Both conditions, however, can be modified or abolished by order of the operator. Now, not only can all the muscles be affected, but isolated groups of muscles,—for example, paralysis of a leg or an arm may be induced, analogous to Erbs's "Lähmung durch Einbildung," or Russell Reynold's "paralysis dependent on idea." This paralysis is characterized by the following clinical signs,—absolute loss of motion and sensation, exaggerated reflexes and ankle clonus. There is also a complete loss of muscular sense and an alteration in the reaction of muscles to electricity, as well as vaso-motor disturbances. This kind of paralysis can be produced both during and subsequent to hypnotism (by post-hypnotic suggestion). It is also possible to cause rigidity of the muscles, a kind of tetanic contraction being produced. According to the relaxation or otherwise of the muscles, the character of the hypnotic state may be differentiated into an active and a passive state. The passive state is that in which the hypnotism most nearly approaches natural sleep; the active form is so different that a casual observer might at first think that the individual was quite awake. Now, these effects are undoubtedly produced by psychical means, but at the same time it is quite possible, by the pressure upon a nerve during the hypnotic sleep, to produce the contraction of a muscle, and in the last degree of hypnotism this contraction may be brought about by simply rubbing the skin over the muscle. This proves that the effect can be produced without psychical action, and without the patient knowing which muscle or group of muscles is to contract.

With regard to muscular strength, differences of opinion are held. Some authorities maintain that the grasp of the hand, for instance, is more powerful during hypnotic sleep than in a normal condition; others again doubt this. It appears, however, that in the majority of cases, if it is suggested to the patient that he is powerful, he certainly exhibits more power than he would do under ordinary circumstances, and there are many cases on record where weak girls have been rendered exceedingly powerful when under the hypnotic influence.

---

[1] *Edin. Med. Journal*, Aug. 1889, p. 142, etc.

With regard to the electrical reaction of nerves and muscles, many authorities state that the strength of the stimulus required to produce a muscular contraction is only about half that required under normal conditions. Of this opinion, at least, are De la Tourette, Morselli, and Mendelsohn, as well as Rosenthal, but Haidenhein, Berger, and Moll apparently doubt this statement.

(*To be continued.*)

V.—CASE OF INTRAUTERINE HYDROCEPHALUS.

By William F. Murray, M.D., F. & L.R.C.S.

I was much interested in Dr Underhill's paper on Intrauterine Hydrocephalus published in your September number. As hydrocephalus occurring in primiparæ is so rare, the following case, which lately happened in my practice, may be of interest, especially to country doctors, who may be called upon at any moment to act, as I did, without the advice of an expert.

On Saturday, 2nd June 1889, about 4 P.M., I went about six miles into the country to attend Mrs H., 21, a healthy primipara, who was in labour. The os was dilated to the size of a five-shilling piece, and very flaccid. With difficulty I detected the head above the brim. I ruptured the membranes, and although the pains were strong, the head would not enter the pelvis. Suspecting I had either to deal with a case of contracted brim or hydrocephalus, I applied the forceps. There was great difficulty in locking them, and the handles were wide apart, and on traction the instrument slipped along the head, which never moved. I saw I should either have to turn or perforate, and sent for my craniotomy instruments and my friend Dr Cable. Before perforating it was agreed to attempt turning, chloroform being administered. On introducing my hand along the sacrum, I found it very prominent, narrowing the pelvis antero-posteriorly, so far as I could roughly judge, to little more than three inches. I passed my hand over the right ilium, and found an enormous head filling the lower segment of the uterus. With great difficulty I passed my hand between it and the uterus, and, seizing a foot, attempted to turn, but failed. As there was no alternative, I proceeded to perforate with Simpson's perforator, but the anterior lip of the os hung low down in the pelvis, and I could not reach the head without the risk of injuring it with the sharp corners of the instrument. By good luck I had a small old-fashioned scissor perforator, which answered admirably, and was far more manageable. On perforating, a large quantity of watery fluid came away, and the labour was slowly completed with the craniotomy forceps, and latterly on the scalp giving way with the fingers. The body was that of a full-grown, fat, well-nourished child. The whole labour was ended in little more than twelve hours.

Notwithstanding the unavoidable bruising, the mother made a good recovery, the temperature only once rising beyond normal, and she is now quite well. She had suffered much mental annoyance during the first months of pregnancy, and to this fact the husband attributed the hydrocephalus. I could get no family history of hydrocephalus on either side.

VI.—ON EXCISION OF THE ANKLE-JOINT.

By Arthur Neve, F.R.C.S. Ed., Mission Hospital, Kashmir.

I. Excision of the ankle-joint for disease has never attained such favour as that of the knee or hip. The reason is not far to seek. Disease of the ankle is often complicated by caries of the tarsal bones, and the radical cure by amputation at this joint scarcely carries with it the opprobrium of mutilation. No doubt there has been some advance during the last decade, but not as much as there ought to have been.

Mr Holmes, in 1871, wrote favourably of the operation[1] as applicable to a limited class of cases. M. Ollier wrote:[2] "It is only for epiphysary osteitis of the bones of the leg, with extension of the suppurative inflammation to the articulation, that I believe this resection plainly indicated."

Professor Spence, at the time of his second edition (1876), seems to have had almost no experience of excision of the ankle for disease.

Dr P. H. Watson appears to have been one of the pioneers of tarsal surgery.

Coming down to the present time, I find that while one writer in Heath's *Dictionary of Surgery*, Mr Jacobson, apparently practises extensive resections,[3] another writer, Mr Warrington Haward, limits the application of the operation in the same way and almost in the same words[4] as Mr Holmes did fifteen years before.

In the days when an excision merely meant the removal of the articular surfaces of the bones, the operation for a joint like the ankle was obviously of limited usefulness. At the present time also a formal excision of that joint, even including the diseased synovial membrane, will only find a sphere in a limited class of cases, for disease of sufficient severity to require excision is but rarely confined strictly to the joint. But the term excision of the joint must now be used in a wider sense, so as to include all the ramifications of the joint disease, whether this be primary or secondary to disease of the neighbouring bones of the tarsus.

---

[1] *System of Surgery*, vol. v. p. 710.

[2] *Ibid.*

[3] Heath's *Dictionary*, vol. ii. p. 567.

[4] *Ibid.*, vol. i. p. 854.

II. There are many cases of disease in which extensive removal of bone may be done with every prospect of leaving a useful foot. There are other cases in which with even limited disease an amputation may be considered advisable.

The tubercular diathesis underlies many of the cases. If its manifestations are clear and the lungs are diseased, excision of a large joint will seldom be justifiable. One of my earliest cases developed pulmonary phthisis after excision of the ankle. Two years later Syme's amputation was performed, but she died of the lung disease just as the stump was healed.

Where some general weakness appears to be the direct result of the local disease, the latter should be removed, and, if limited in extent, removed by excision ; if otherwise, by amputation.

Where the general health is good, I believe that very wide resections may be practised with the hope of curing the disease, but that only in the favourable age under twelve will the resulting foot be useful. In a case last year, in which the active disease was in the anterior part of the tarsus and metatarsus, I began to resect these, but finding the other bones in a condition of fatty degeneration, I amputated the foot, which would have been, after removal of the whole tarsus and metatarsus, less useful for progression than a good stump. In another case last year the social conditions of the patient (a Kashmiri lady) did not permit of amputation. The whole tarsus, with the heads of the metatarsal bones, was removed. Healing was very slow, and the resulting foot not useful.

In the region of the ankle anything short of a good result is a failure. The part is a dependent one, it is exposed to injuries, and it has to bear the weight of the whole body. Bony ankylosis is no drawback in progression, as the tarsal joints soon compensate for the fixation of the astragalus. Thus successful resections of the ankle-joint with portions of the tarsus may result in a thoroughly useful undeformed member. Numerous cases have been published of such success. Perhaps in none had the disease been so extensive as one in which I removed, in two stages, 5 inches of the tibia and the os calcis and astragalus.[1]

III. The application of the principle now acted on—that the diseased soft parts should be removed as well as the articular surfaces (and in many cases without any removal of bone)—has led to further modifications in the methods of excision. Those incisions are practised which give the best view of the whole interior of the joint. They are usually on its convex aspect.

---

[1] *Lancet*, vol. ii., 1888, p. 138.

A consideration of the methods now in vogue for the elbow and knee led me to think that the ankle-joint would be best viewed from the back. The os calcis is the analogue of the olecranon and the tubercle of the tibia, the tendo Achillis of the patellar ligament and the triceps tendon. The following case was thus operated on—

Unira, æt. 12. Disease of three months' duration; cause unknown. General condition of the boy was good, though he was thin. The left ankle was much swollen, and a discharging sinus opened below the ext. malleolus. Through this a probe passed into the joint. There was tenderness over the lower epiphysis of the tibia, also movement with crepitus at the tibio-fibular articulation, and the lower end of the fibula was swollen and soft. It was diagnosed as caries of the epiphysis of the fibula with secondary affection of the joint.

Operation.—A large posterior curved incision was made down the anterior surface of the fibula for the lower three inches, then back across the heel at the insertion of the tendo Achillis, and again up the posterior aspect of the tibia for 3 inches. On the outer side the flap thus marked out was carried down to the bone, and at the back it included the tendo Achillis, but on the inner side it was only cutaneous. The deeper structures on the inner side—tendons, vessels, and nerves—were dissected off the bone, and turned forwards just as is done to certain structures behind the internal condyle of the humerus in excision of the elbow-joint; in the ankle with less difficulty. With the foot powerfully flexed, the posterior surface of the ankle-joint and astragalo-calcaneal joint was then easily accessible. The lower end of the fibula being removed with bone forceps, and the lateral ligaments cut, the whole joint could be inspected.

The synovial membrane, which was much thickened, was dissected off. Diseased tissue extended into the astragalo-calcaneal joint, and up behind the bones; there were also foci of disease at the posterior and outer surfaces of the astragalus, requiring to be gouged, also on the front of the articular surface of the tibia. The pouch of synovial membrane at this point was the only part difficult to dissect off, not being safely accessible with scissors.

From the small cavity gouged in the tibia a trench was cut backwards, enabling one to examine the state of the bone, and giving space for a drainage-tube. The cartilaginous surfaces were shaved off superficially with a knife.

The drainage-tube passed from an anterior button-hole incision and out at the back of the flap, which was now adjusted, each tendon being separately sutured. There was little bleeding, and few ligatures were applied. The foot was fixed to an anterior suspension rod. The operation was performed antiseptically. I was much aided by Dr E. F. Neve in the tedious process of dissecting off the synovial membrane, etc.

The patient suffered no pain and had no fever. He was dressed at first at intervals of four or five days. The discharge was almost *nil*, except at the old outer sinus. Owing to the rapid growth of granulations, there was once or twice a little check to the drainage, but the progress was very good, and within three weeks the wound might be regarded as healed. Within six weeks he was able to walk without pain, and could move his toes. A fortnight later he went home.

I have narrated this case at some length with the object of bringing out certain points; and with regard to these it should be compared with the operation by lateral incisions and the anterior incision recommended by Jacobson.

1. It gives good visual access to every part of the ankle-joint.

2. Also to the bones composing the joint.

3. As well as to the astragalo-calcaneal joint and upper surface of the os calcis.

4. If the astragalus requires to be excised, no incision would serve the purpose better.

5. The anterior portion of the incision may be made as a diagnostic measure before cutting the flap.

6. The structures cut can be sutured without materially interfering with the strength, utility, or vascular supply of the foot.

7. The wound drainage is dependent.

IV. There may be special indications for other incisions, as in a case now under the care of Dr E. F. Neve, in which there was extensive caries on the anterior surface of the tibia, extending to the joint, with numerous sinuses and a large ulcer in front. Here an anterior ⊤ incision gave the best access to the disease.

In other cases in which the synovial membrane is not materially diseased, and there is no tarsal disease, operation by lateral incisions may be indicated, as involving the smallest amount of disturbance to normal tissues, and yet giving ample room for excision.

Again, where there is disease of the joint with signs of disease in the scaphoid, etc., an anterior incision from malleolus to malleolus, as recommended by Mr Jacobson, would seem the best way of dealing with the entire disease, whether by excision or, if finally it prove too extensive for conservative surgery, by amputation. But where the disease has begun in the astragalus, the tendency is more to spread to the os calcis by one or other of the articular surfaces. *Vice versa*, it may spread from os calcis to astragalus.

In either case the posterior incision gives the best access, but it need not be prolonged upwards to the extent described. But with our modern methods of wound treatment, there is no need to work in narrow holes. It is more important to remove all the disease than to spare tendons which can subsequently be sutured. A curved incision, such as the one described, is a simple, easy procedure. But if an anterior incision be indicated, I believe that it should also be more or less in the form of an oval flap, extending forward to the anterior limit of the disease.

Thus if it be expedient to remove the anterior tarsal bones as well as the joint, the flap should be carried from 2 inches above either malleolus and forwards to the tarso-metatarsal joint. In such a case it would be a wise precaution to unite each tendon as it is cut by a long ligature, so as while not interfering with the subsequent manipulation, yet to secure rapid identification of their respective ends.

The foregoing appears to me the most workable method of obtaining complete removal of all diseased structures in the region of the ankle with a minimum amount of mutilation.

VII.—THE MINUTE ANATOMY OF THE PLACENTA IN EXTRAUTERINE GESTATION.

y D. Berry Hart, M.D., F.R.C.P.E., F.R.S.E., Lecturer on Midwifery and Diseases of Women, Surgeons' Hall, Edinburgh; Assistant Obstetric Physician, Royal Maternity and Simpson Memorial Hospital, Edinburgh; Assistant Gynæcological Physician, Royal Infirmary, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 3rd July* 1889.)

I have first to define the limits of the present paper. I do not take up the anatomy of the extrauterine placenta in the various varieties of this condition, but consider merely its development in what I have termed the extra-peritoneal form. I must, therefore, briefly explain what is meant by this.

As all know, the Fallopian tube is, in the vast majority of cases, the starting point of an extrauterine gestation. The most common result of this is that rupture occurs, usually at the second month, through some part of the tube covered by peritoneum, a result almost invariably fatal if left alone, and as invariably curable if operated on in time by abdominal section.

A rarer termination in Fallopian tube gestation is, that further development takes place between the layers of the broad ligament, which become separated to accommodate fœtus and placenta. Even here rupture into the peritoneum may occur; but if the peritoneum remain intact we may get a continuance of development as follows:—

If in the early Fallopian tube gestation the fœtus lies above the placenta, we may get both it and the placenta developing between the layers of the broad ligament. The placenta in its growth has its downward displacement soon limited by the pelvic muscles, while the fœtus may rupture through the peritoneal lamina and lie in the peritoneal cavity free among the intestines. This gives us, therefore, a mixed form of gestation, the fœtus being intra-peritoneal, the placenta extra-peritoneal. The placenta thus

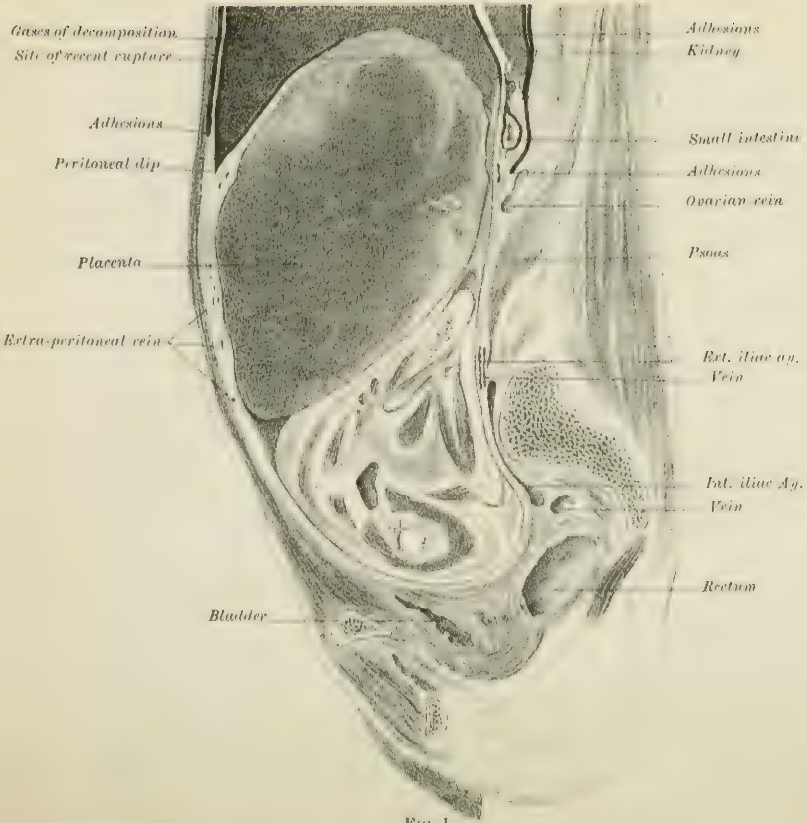


Fig. I.

Sagittal lateral section of advanced extra-peritoneal extra-uterine gestation (Hart and Carter).

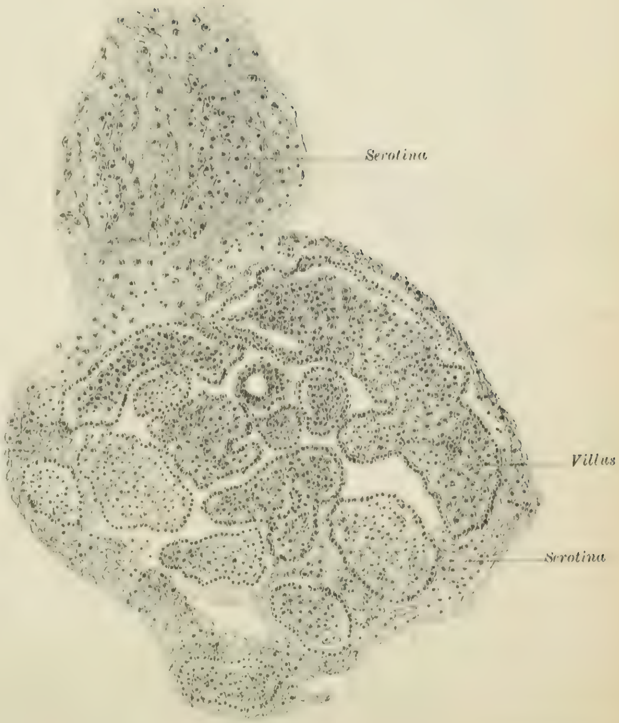


Fig. II.

Villi and serotina of two months Fallopian tube gestation (× 75).

develops in the broad ligament and pelvic connective tissue, and has in addition been displaced downwards by the fœtus, the amount of displacement being necessarily small. If, however, in an early tubal gestation the fœtus lies lower in the tube, the placenta thus occupying the part of the tube covered by peritoneum, we may then get a development between the layers of the broad ligament as in the previous instance, but with this special difference. The growing fœtus pushes up the upper part of the broad ligament with placenta, and thus displaces the placenta up until it may lie attached to anterior abdominal wall opposite the level of the lumbar vertebræ. The fœtus and placenta too, lie, therefore, in extra-peritoneal tissue, the peritoneum being lifted up as the pregnancy goes on. This extra-peritoneal development of extrauterine gestation is remarkable, but is not unique in pathology, being analogous to the development of a burrowing pelvic abscess or of a papillomatous ovarian cyst. (Fig. I.)

We may sum up the stages, therefore, as, (*a*) Fallopian tube; (*b*) broad ligament in the first place; then (*c*) either placenta extra-peritoneal, fœtus intra-peritoneal; or (*d*) both extra-peritoneal.

If we now take up specially what is more properly the subject of to-night's communication, the minute anatomy of the extra-uterine placenta in its extra-peritoneal development, we must keep in mind the following points. While the placenta begins its development in the mucous membrane of the Fallopian tube, it soon develops in connective tissue; and during its development here undergoes a slow displacement. In the case where both fœtus and placenta are extra-peritoneal, the fœtus lying below, the placenta must have been displaced upwards for at least 10 inches. Inasmuch as this amount of displacement was accomplished in about as many months there was never any gross separation, but a slow microscopic progress which, as we shall see, causes slow blood effusion and organization.

In investigating the minute structure of the extrauterine placenta I had the following material:—

1. Tube with placenta attached from a ruptured tubal gestation at the second month.
2. Broad ligament gestation at fourth month.
3. Abdominal gestation, entirely extra-peritoneal, and at nearly full time.
4. Abdominal gestation, with fœtus probably intra-peritoneal, and placenta certainly extra-peritoneal.

The last preparation I owe to the great kindness of Dr Barbour, and no one appreciates more than I do the self-denial which gave me such a valuable specimen.

The tubal gestation was cut in paraffin, the others in celloidin, and the investigation was carried on in the laboratory of the Royal College of Physicians.

The following is a brief summary of the results:—

In the Fallopian tube gestation the villi lie embedded in decidual cells, and no intervillous sinus system seems to exist. Large sinuses, however, have formed in the muscular wall. The villi are well formed, and are covered with perfect epithelium. The decidual cells are large, and have large nucleus and nucleoli. (Figs. II., III., and IV.)

In the broad ligament placenta we begin to see the serious damage caused to placental structure by its development in connective tissue. The villi are less perfect in contour, blood extravasation is present, blood crystals are in abundance, while the decidual cells are few and less perfect.

In the advanced abdominal gestation where the placenta is in the pelvic connective tissue, and therefore less displaced, we see fairly perfect villi, decidual cells, blood crystals with large areas of extravasated blood, which may be the remains of an attempt at a placental sinus system.

The most remarkable alteration in placental structure is found, however, in the abdominal gestation with the extreme displacement of the placenta. Here the placenta is converted into a mass of organizing blood clot, with large areas of blood crystals, great compression and distortion of the villi, entire absence of decidual cells, and no recognisable intervillous sinus system; although the enlargement of the veins in the abdominal wall at the placental site points to some such arrangement existing. (Figs. V., VI., and VII.)

We may, therefore, say broadly that the development of the placenta in the extra-peritoneal form of extrauterine gestation is a destructive one, reduces the placenta practically to compressed villi, where the serotina is destroyed and replaced by blood crystals and organizing blood clot. The least damage is done to the placenta, which from its more favourable position in the lower part of the tube with the fœtus above, is, as I have already explained, displaced least.

In the last place, I have to point out the important bearing of these facts on the life of the fœtus. One of the most common terminations of an extrauterine gestation is to have the fœtal bones discharged per rectum, or in fact to terminate as a pelvic abscess. Such are evidently extra-peritoneal developments of a Fallopian tube gestation, where the destructive process going on in the placenta has killed the fœtus, and where the proximity of the whole gestation to bowel unprotected by peritoneum has led to an endosmosis of intestinal gases, or passage of micro-organisms such as to determine putrid suppuration.

A more favourable termination is where, from the higher position of the fœtus, we get its escape into the peritoneal cavity, less displacement of the placenta, and thus less interference with its functions. These are the cases where a full time fœtus can be removed, and where the placenta, from its extra-peritoneal position, is not recognised at the time and is left undisturbed for ultimate

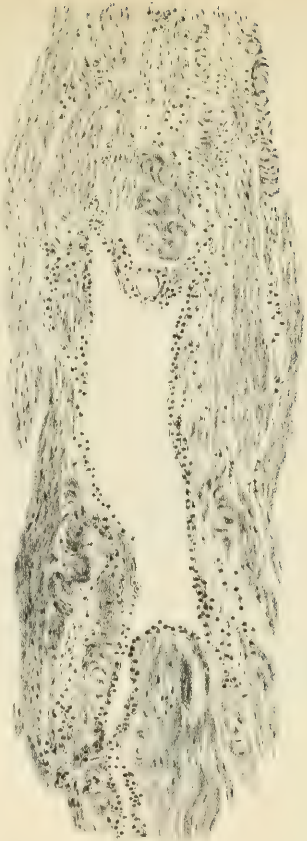


Fig. III

Enlarged vein in muscular wall of Fallopian tube gestation (× 75).

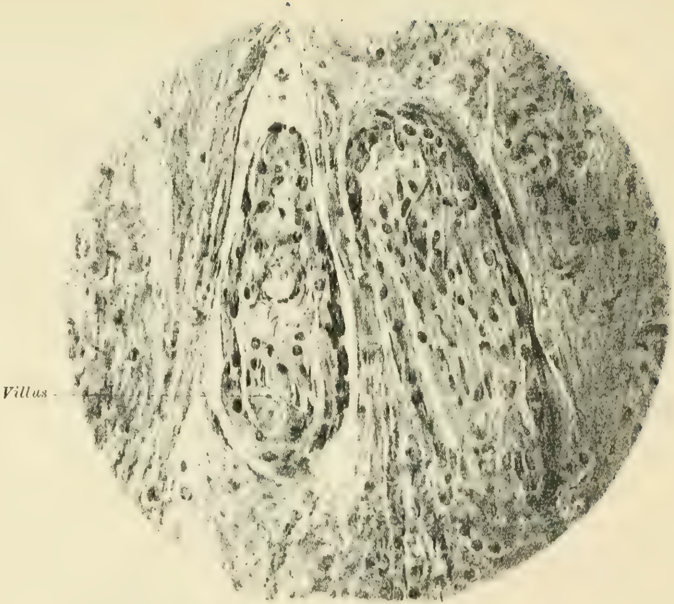


Fig. IV.

Villi and serotina of two months Fallopian tube gestation (× 300).

[illegible]

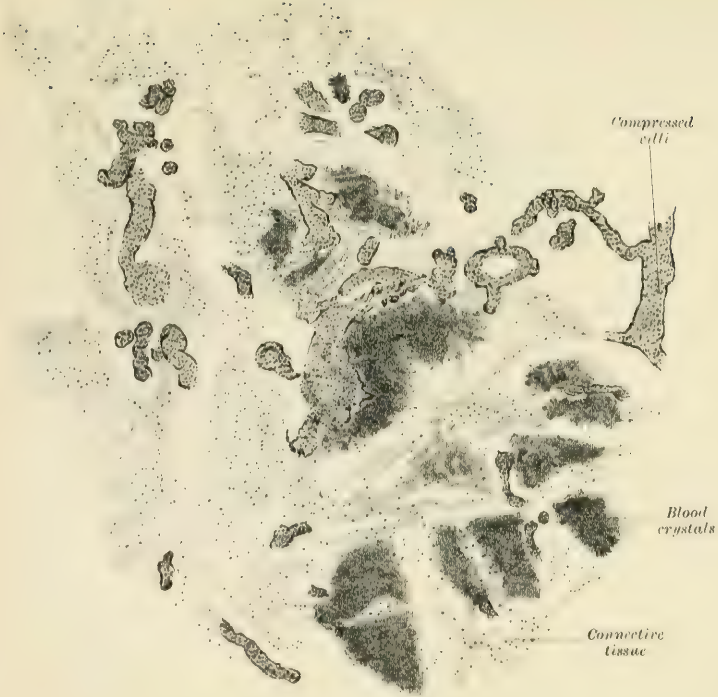


FIG. V.

Placenta in advanced extra-peritoneal extra-uterine gestation (× 25).

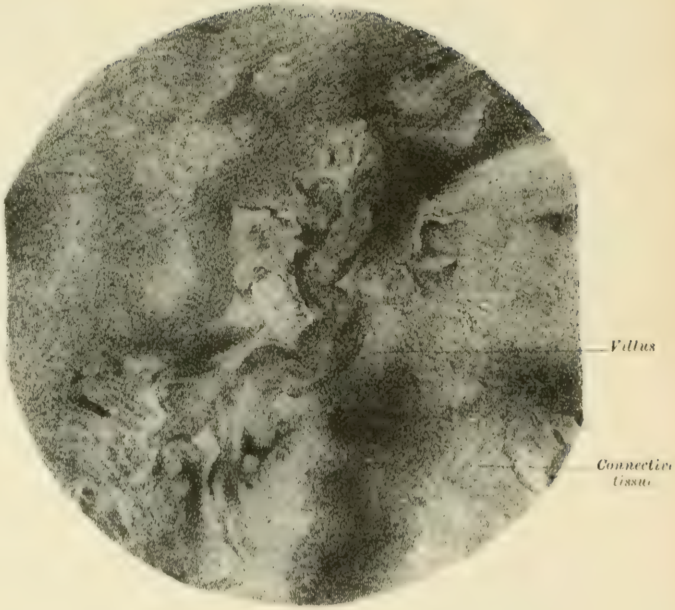


FIG. VI.

Villus in organized blood clot of advanced extra-uterine gestation (extra-peritoneal) showing compression and distortion (× 300).

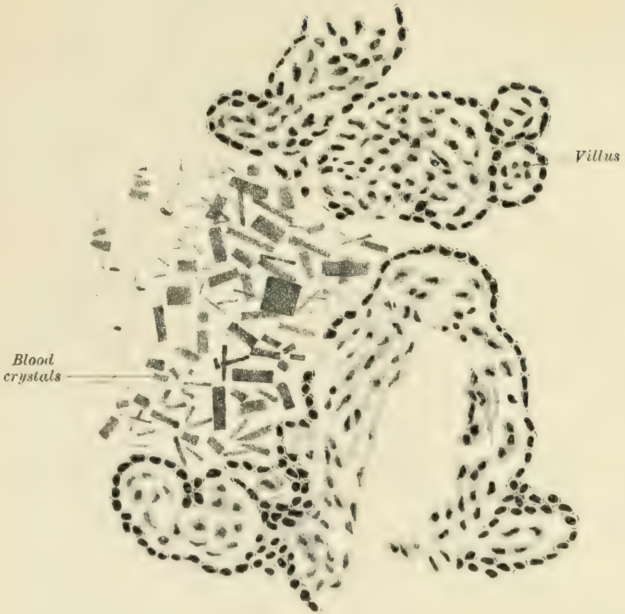


Fig. VII.

Villi and blood crystals of advanced extra-peritoneal extra-uterine gestation (× 280).

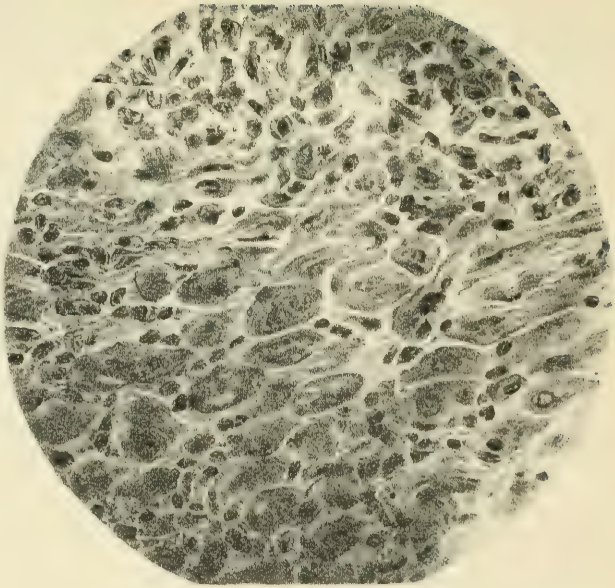


Fig. VIII.

Decidual cells from uterine decidua of broad ligament gestation ($4\frac{1}{2}$ months) (× 375).

W. & A. K. Johnston, Edinburgh & London

absorption if away from bowel. Proximity to bowel may lead to its putrefaction and breaking down.

DESCRIPTION OF FIGURES.

FIG. I.—Sagittal lateral section of advanced extra-peritoneal extrauterine gestation (Hart and Carter).
FIG. II.—Villi and serotina of two months' Fallopian tube gestation.
FIG. III.—Enlarged veins in muscular wall of Fallopian tube gestation.
FIG. IV.—Villi and serotina of two months' Fallopian tube gestation.
FIG. V.—Placenta in advanced extra-peritoneal extrauterine gestation.
FIG. VI.—Villus in organized blood-clot of advanced extrauterine gestation (extra-peritoneal).
FIG. VII.—Villi and blood crystals of advanced extra-peritoneal extrauterine gestation.
FIG. VIII.—Decidual cells from uterine decidua of broad ligament gestation (4½ months).

Part Second.

REVIEWS.

The Natural History of Cow-pox and Vaccinal Syphilis. By CHARLES CREIGHTON, M.D. London: Cassell & Company: 1887.

"Vaccination." Article in the *Encyclopædia Britannica,* Ninth Edition, Vol. XXIV. By the same Author.

Jenner and Vaccination: A Strange Chapter of Medical History. By the same Author. London: Swan Sonnenschein & Co.: 1889.

(*Continued from page* 256.)

THERE is indeed no mystery about Jenner's observations on the proper manner of performing variolous inoculation. He knew that at a late stage of the pock, pus might be inoculated as well as small-pox, and he had reason to think that preservation of the matter for days at the temperature of the human body, as in the pocket of the inoculator, might have a deleterious effect. What he desired was that his new practice, which he hoped would be "essentially beneficial to mankind," should be tested by the subsequent insertion of variolous virus alone, which would have every chance of producing small-pox in an unprotected person, but would have no effect on a system rendered incapable of receiving it. He did not want vaccination, by which his reputation was to stand or fall, to be subjected to a fallacious test, as by the inoculation of the products of purulent decomposition, which might, even in a person proof against small-pox, set up symptoms capable of being confused with some of those resulting from successful variolation. His statement was that due "attention to the state of the variolous matter previous to the experiment of inserting it into the arms of those who had gone through the cow-pox . . . .

might present much subsequent mischief and confusion." His attitude needs no justification, for his request simply was that his new project should have a fair field and no favour.

This, however, is not Dr Creighton's whole argument on the variolous test, and it will be necessary to follow him into some further details. He complains that the *Inquiry* contained only three or four cases in which the variolous test had been applied after vaccination. But, in addition, Jenner mentions about twenty cases to which the test had been applied sooner or later after accidental cow-pox. Then Dr Creighton takes up and animadverts on Jenner's first case of vaccination from a human subject. James Phipps was vaccinated in 1796, and subsequently tested by variolation, which "produced no effect." Our author's remarks on this are as follow: "Poor Phipps, as Jenner used to call him, was inoculated some twenty times after that, and never 'took;' he was Jenner's show case of resistance to small-pox; he was a poor consumptive or scrofulous youth, with his lymphatic glands so clogged (after the cow-pox?) that any subsequent inoculation of virus on the arm had no chance of being absorbed" (p. 126). And (p. 149) he speaks of "scrofulous children with clogged absorbent glands," of whom "his own show case James Phipps was a good instance." This statement as to Phipps seemed to me singularly at variance with Jenner's description of him in his *Inquiry* (p. 32) as "a healthy boy, 8 years old," and I therefore turned with interest to the reference given by Dr Creighton, "Baron, ii. 304." Here is the passage, which is detailing the incidents of a country drive: "A short time afterwards we passed Phipps, his first vaccinated patient. 'Oh, there is poor Phipps,' he exclaimed, 'I wish you could see him; he has been very unwell lately, and I am afraid he has got tubercles in the lungs. He was recently inoculated for small-pox, I believe for the twentieth time, and all without effect.'" It is not possible accurately to get the date of this incident; but the narrative immediately proceeds to mention "a subsequent visit" in October 1818, and the occurrence in question could hardly have been very long previous—not many years previous, at any rate. Now, in 1818 Phipps was a man 30 years old, and we may safely take it that he had reached the age of manhood at the time when Jenner feared he had got tubercles.

What Dr Creighton has done, therefore, is this:—(1), To absolutely ignore Jenner's statement that Phipps was a healthy boy; (2), to alter Jenner's *fear* of tubercle into the positive statement, "he was a poor consumptive or scrofulous youth;" (3), to transfer the illness from (almost certainly) manhood to childhood; (4), to introduce the entirely new statement that the axillary glands were diseased; (5), to suggest that this (imaginary) disease of the glands was due to vaccination at the age of 8 years; (6), to assert that it was so permanent as to make successful small-pox inoculation impossible, even when attempted for the twentieth time; and (7), to buttress the whole story by stating that Jenner was in the habit of speaking of "poor Phipps," whereas there is no evidence that he ever employed the expression except on the occasion narrated.

Comment is needless. But I may be allowed to point out that it is Dr Creighton who complains (pp. 107, 108) that "the suspicion of having been edited"—by Jenner himself—attaches to much of Jenner's writings.

The reference to clogged glands leads us to another argument, namely, "That the vaccine infection itself caused a swelling and obstruction of the absorbent glands in the armpit and neck, and to that extent made them incapable for the time, and in some cases for long after, of taking up and passing into the lymphatic circulation another virus inoculated under the skin at the same place" (p. 149).

This, of course, finds its reply in the passage quoted above from Dimsdale (whose instructions, Woodville tells us, were followed in Jenner's day), that small-pox inoculation "is generally performed in both arms." Even in 1806, Lipscomb speaks (in condemnation) of what he describes as "the practice" in question. But even if only one arm was sometimes inoculated, then, to give Dr Creighton's argument any weight, it is necessary to assume that the operator fatuously selected for his test that one which had newly been vaccinated. The question of how long any such barrier would exist need not detain us. Neither need we delay over the unimportant contentions (1), that as cow-pox was often "a discharging sore," it would prevent for the time successful variolation; and (2), that the vaccinal exanthem, "which was a frequent incident of the early days of vaccination," would have a similar effect. But I may note in passing that the latter point furnishes Dr Creighton with the opportunity of falling into another blunder, in the course of his curiously unsuccessful endeavours to "authenticate the facts by full references."[1]

He says that in Woodville's practice at the Inoculation Hospital, the vaccinal exanthem got mixed up with the true small-pox eruption, which many of the patients had. "But it was often observed in the country practice of vaccination, where concurrent small-pox was out of the question. Thus, of seventy cases vaccinated by Evans of Ketley, near Shiffnal, commencing in May 1799, no fewer than thirty-nine had an eruption." But on verifying the reference, what do we find? That there was an abundance of "concurrent small-pox;" for Evans says, "At the same time I inoculated fifty patients with variolous matter, that I might have an opportunity of observing the different effects of the two diseases. And whenever I had it in my power, I inoculated one part of a family with vaccine and the other part with variolous virus" (*Med. Phys. Jour.*, vol. ii. p. 312).

Dr Creighton further gives a series of cases to show that even where the variolous test failed, that is, even where small-pox was successfully inoculated on vaccinated subjects, the profession was so blinded for the time that the very man who performed the inoculations which proved the uselessness of vaccination "congratulated mankind" on the success of cow-pox. The cases are a series of fourteen, published by M. Ward, Surgeon to the Manchester Infirmary. They are given individually by Dr Creighton (pp. 130, 131, *Jenner and Vaccination*), and having given them, he comments on them thus:—

---

[1] Preface to *Jenner and Vaccination.*

> "Ward was highly pleased with this record of the variolous test. What are we to think of the temper of the profession at this time, when a respectable practitioner congratulates the world upon a great discovery, with failure staring him in the face from the record of his own experience? Only one of all his cases resisted variolation after being cow-poxed, namely Case VIII.; three cases took small-pox in the clearest way after being cow-poxed (Nos. I., II., and IX.); four cases resisted vaccination, and likewise resisted variolation; one case resisted vaccination, and received the subsequent variolous infection; and two cases were apparently variolated in the first instance by misadventure."

Dr Creighton's notes of these cases are so much at variance with what seem to me to be the facts, that I must trouble the reader to follow the details of at least the first case, as condensed from Ward's paper in the *Medical and Physical Journal.* It was that of a girl, aged 7, whom Ward operated on on 16th April. She had a very serious illness. On the 4th day the punctures considerably inflamed, one discharging slightly. 8th day—Efflorescence round punctures lessened. 12th day—Feverish from the morning; had a restless night. 13th day—One puncture healed, the other consisting of an oblong vesicle with limpid contents and increased efflorescence; breath offensive, tongue white, anorexia, eyes dull and heavy. 14th day, 12 noon—Some spots appeared on face, "which relieved her." 15th day—Plentiful crop on every part of body. 16th day—Pustules on tonsils and mucous membrane of throat; eruption confluent in some places on face and arms. 17th day—Rather hoarse. 18th—Very weak. 19th—"Pustules become more confluent daily;" several on tongue; eyes nearly closed. 20th—Maturation nearly complete; eyelids closed; inoculated arm loaded with pustules; puncture covered by small dry scab. 21st day—Somewhat better; throat well. The improvement continued irregularly, till on the 31st day "her health is restored." That was on 17th May. Then on 4th July she was "inoculated with variolous matter," and on 7th July it is noted, "she has not taken the infection." He adds, in reference to the eruption above mentioned, "As nearly as I can ascertain, she had not fewer than from 1600 to 1800 pustules."

That is Ward's statement; here is Dr Creighton's—

> "CASE I.—16*th April*—Girl, aged 7, successful vaccination (oblong vesicle on 13th day, full of limpid fluid and surrounded by areola); was thereafter inoculated with small-pox, and had the disease in the confluent form (1600 to 1800 pustules)."

Thus Ward tells us " she has not taken the infection," while Dr Creighton says she "had the disease in the confluent form."

Ward's statement does not seem difficult of interpretation. He was deceived as to the nature of his first inoculation. He had used small-pox matter, not cow-pox matter, and the result was a somewhat delayed attack of confluent small-pox; therefore the second inoculation failed. But Dr Creighton says the girl was successfully vaccinated, and then she was variolated, and to make the history complete, he transfers the 1600 or 1800 pustules from the first to the second inoculation, though Ward tells him the latter failed. That he has got fairly lost as to the sequence of events is shown by his statement that " two cases were apparently variolated in the first instance by misadventure." But these two cases were X. and XI., inoculated on 30th April (the 14th day) from Case I. How possibly could they be *variolated* from the " oblong vesicle " of a " successful *vaccination?* " Thus, after all, Dr Creighton seems at one moment to recognise the fact that the original infection was small-pox, and at the next moment he forgets all about this, and speaks of it as cow-pox. Indeed, in his notes on Case X., he speaks of it as " inoculated from Case I., evidently with the co-existent small-pox matter mistaken for cow-pox." It is not worth spending time over the other thirteen cases. Cases II. to VIII. were inoculated from the same source as Case I., and the other five were inoculated from Case I. There was indeed no vaccination in the business. Ward, who wrote in July 1799, had doubtless been misled into expecting pustules after cow-pox by the publication, in May of the same year, of Woodville's experience in the Small-pox Inoculation Hospital, where, as Jenner afterwards pointed out, he had for the time confused the two diseases.

Blind as Ward was to the nature of his cases, he does not deserve the further misreading to which his critic subjects him. Dr Creighton says Ward " sent the following series of cases, and 'congratulated mankind' on the success of cow-pox." As a matter of fact, Ward's congratulations are not based on his own experience, but on the opinions which he gives from Woodville, " that those persons who have had this disease by inoculation are thereby secured from having the small-pox," and " that the inoculated cow-pox is seldom accompanied by pustules " if the virus be taken from mild cases. He then says, " Considering the above observations as established facts, and should it also appear that the cow-pox is not so liable to be propagated by contagion as the small-pox, may we not indulge a hope that the era is probably not far distant when we shall be able to congratulate mankind at large on their having a fair prospect of being exempted at no very remote period from that most destructive malady." But Dr Creighton's reader is led to believe that Ward's congratulations of mankind are based on his own experiments.

Regarding another series of cases, which occurred at Stroud, Dr

Creighton says,—"The Stroud doctors put all their ten cases religiously through the test, with the singular result that the only one of the ten, an adult, whose vaccination had not held, was the only one who stood the test, while the other nine all had the small-pox in one degree or another, in the usual inoculated form—the two who had the worst vaccinal ulcers having stood the test rather better than the others" (p. 127).

Dr Hughes, of Stroud, along with Mr Darke, had to do with five of these cases, and Dr Thornton with the other five. The first-named gives a fairly full account of what occurred. In T. V. there was a slight inflammation the day following the vaccination. . This diminished, and on the sixth day hardly any appearance of the puncture remained but a small scab; in the evening a slight redness came on, with a small pimple or two near the puncture, which disappeared next day. Cases II. and III. were practically similar. Dr Creighton accepts this as successful vaccination, and the subsequent successful variolation of the same cases is part of his argument in the above quotation. Dr Hughes thought differently. He speaks incidentally of Cases II. and III., "who did not take the infection of the cow-pox." The other two cases, a child 2 years old and a servant, W. K., 15 years old, reported on by Hughes, had very considerable local symptoms. The child was decidedly the worse of the two. They had been reported by Thornton to Dr Beddoes of Bristol, who first published the cases, as "alarming and dreadful." But Hughes says No—the more severe case was only "troublesome and disagreeable." Hughes, moreover, gives the details, which Dr Creighton condenses, chiefly by omitting the favourable points (as that "from this time the induration lessened," and that "K.'s arm was so little troublesome that he went on with the usual business of the house the whole time"), and in conclusion he accepts the description which Hughes had rejected, that both cases had "alarming" and "dreadful" symptoms. And finally, as to the variolous inoculation, Hughes states that,—"In both, the punctures in the right arm inflamed earlier than they usually do after a first inoculation (as is common where a second inoculation is made so soon after the first, where variolous matter is used): they became small pustules, without any previous accompanying illness, without any affection of the axilla or subsequent pustules." Thus Hughes says in effect that the variolous inoculation failed, while Dr Creighton is of the opposite opinion. In regard to the symptom spoken of by Hughes—the early appearance of inflammation after variolation—I may mention that the literature of the period shows it to have been an accepted evidence that the operation would not succeed.

In connexion with this letter from Hughes, Dr Creighton makes another attack on Jenner. Hughes sent his letter dated—

> "9th May 1799 to Jenner, who forwarded it to the *Medical and Physical Journal*, with the explanation that it had arrived too late for him to include

in his second pamphlet. But he had already been told the main facts by Drake; and in the second pamphlet he had deliberately omitted all reference to them, merely stating that 'Mr D., a neighbouring surgeon,' had taken some matter from the arm of the child on 13th December. The trial, however, had made some noise in Stroud, Gloucester, and Bristol, and it would have been too risky for Jenner to have suppressed the second and fuller relation of facts by Hughes" (p. 95).

The preface to *Further Observations* is dated April 5th, 1779, and the work is reviewed in the June number of the *Journal*, which also contains Hughes's letter, as Dr Creighton mentions, of May 9th, 1799. Clearly, therefore, Jenner is likely to have been telling the truth in saying that it had arrived "too late for insertion in his work." Indeed, Hughes seems to have expected this, for he writes to Jenner, "You are at liberty to insert it in your next publication, or transmit it to the editors of the *Medical and Physical Journal*." But why it should be supposed that Jenner desired to suppress a communication which, as we have seen, amounted to a defence of cow-pox against published assertions as to its "alarming and dreadful" results, probably no one but Dr Creighton knows.

In the course of his chapter on the variolous test, Dr Creighton naturally endeavours to lessen as much as possible the importance due to the very extensive way in which the test was applied. He says, "The total of some two thousand successful English tests, which got extensively quoted abroad and helped greatly to recommend the new practice, was made up of Woodville's hundreds, of Marshall's two hundred odd, of Pearson's scores of cases, and of other large aggregates for which the details were never given," and that where scrutiny of the event is possible, "in most cases a full and correct variolous pustule" resulted; and further, that the test "was not applied at all generally in England after the first weeks of cow-poxing in the spring and summer of 1799." These statements are not borne out by the facts.

In the first place, Woodville's cases, which in 1799 were hundreds, ultimately amounted to thousands. To the Committee of the House of Commons he stated that, up to 1st June 1802, there had been done in the Hospital "7500; about one-half of which was since inoculated with small-pox matter, *in none of whom did the small-pox produce any effect*."[1] The italics are his own. By this time he seems to have got over his initial difficulty, for in answer to a question about "pustules like small-pox" following vaccination, he says, "I believe they never do over the whole body; I have seen in some instances a few pustules in the neighbourhood of the inoculated part, but these instances are very rare, one in five hundred."

To show how the test was applied, I may extract from the half-yearly volumes of the *Medical and Physical Journal* a few jottings referring, like most of Woodville's cases, to dates subsequent to "the summer of 1799:"—

---

[1] *The Evidence at Large*, etc., by the Rev. G. C. Jenner. London, 1805.

(1.) *Jan.* 31*st*, 1800.—Mr Stewart, surgeon, Plymouth, reports three cases. Case I.—*Nov.* 1*st*, 1799.—"Active variolous matter" inserted on forty-sixth day after vaccination, followed by inflammation, which at first suggested small-pox, but disappeared in five days. No indisposition nor pustules. Case II.—On the twenty-second day "inoculated with variolous matter without effect," and on the thirty-fifth day "limpid variolous matter was inserted into both arms, but no disease ensued." Case III.—On eighteenth day "inoculated with active variolous matter," and (twenty-fourth day) "she has not taken the infection."—(Vol. iii. p. 236.)

(2.) *March* 11*th*, 1800.—Rev. W. Finch, on 25th and 26th February, "inoculated with variolous matter twenty children," who had been vaccinated in November and in the beginning of December. "All entirely resisted its infection." Inflammation began on the second day, and began to disappear in three to five days In two cases a small-pock resulted, similar to "what nurses who have had the small-pox frequently experience."—(Vol. iii. p. 419.)

(3.) Writing on May 19th, 1800, Mr T. M. Kelson, Seven Oaks, records forty cases in a workhouse, "inoculated with the most virulent matter I could procure, but nothing ensued, except local superficial inflammation for the first six or seven days. I then introduced a wretched family, just recovered from very bad small-pox, their dirty clothes unchanged, and divided them in different beds among them, but to no purpose."—(Vol. iv. p. 23.)

(4.) Mr William Fermor, in his "Reflections on the Cow-pox," reviewed in the *Medical and Physical Journal* of September 1800, gives a list of 173 persons who were (after vaccination) inoculated with small-pox "without receiving the infection in any instance." Some of these, however, may have been previous to the summer of 1799.—(Vol. iv. p. 260.)

(5.) Mr Custance of Kidderminster, in September 1800, inoculated for small-pox Mrs W. and two of her children, who had been vaccinated by Dr Jenner himself fourteen months before, and subsequently tested, without result, by variolation. Custance's inoculation "totally failed in all three," though an unvaccinated infant, "who was inoculated with the same matter, sickened, and had an unusual number of pustules."—(Vol. iv. p. 421.)

(6.) The Parisian Vaccination Committee record that "more than seventy-two have been inoculated for the small-pox, yet none have taken the infection."—(Vol. v. p. 357.) That in certain cases there was a slight local effect is seen in the following extract from an earlier report (p. 101). The inoculation was done three months after the vaccination.

"The nineteen subjects submitted to the operation have been inoculated with fresh pus, taken every time from a variolous infant who was present. The Committee, for the purpose of rendering their experiments more decisive, employed in many of the subjects very deep incisions, such as, according to the inoculators, necessarily occasion a large eruption of pustules. They even proceeded so far as to introduce at different times a great quantity of variolous matter into the incisions, notwithstanding which, not one of nineteen who were inoculated had anything like a general eruption. In fourteen, the incisions were soon obliterated, without any symptom of complaint. In the remaining five, the inflammation can be considered in no other light than as the effect of local irritation, produced by the puncture of the skin. The inflammation began the very day of the insertion. This process has been more rapid and less regular than that of ordinary inoculation. Besides, instances of the same effect occur in persons who, after having had the small-pox, have again submitted to inoculation. In a word, if some preservative effect did not operate by the vaccine inoculation in those who have submitted to it, how should it happen that the variolous matter introduced into their incisions for the inoculation of the small-pox should have excited (in some at least) only a local and partial affection, whilst taken from this source to be transmitted to infants not before inoculated with the cow-pox, it has produced in the latter all the usual symptoms of general infection?"

(7.) "Cit. Ané, Paris, the well-known inoculator, in the presence of many medical practitioners," inoculated in three places each seven children vaccinated three or four months previously by Colon. The small-pox virus was taken from a child labouring "under a copious eruption of the natural small-pox." "Not one took the small-pox."—(Vol. v. p. 556.)

(8.) Mr Clement of Shrewsbury, who was "prejudiced against the vaccine inoculation," vaccinated three children in the autumn of 1799. Early in the following spring he tested the children "both by inoculation and exposure" during the prevalence of small-pox, and "without the least effect, excepting local inflammation."—(Vol. vi. p. 5.)

(11.) Lord Berkeley reports two cases in which the "inoculated part looked red and angry, as if it would fester, for a few days, but then died away, without producing any effect."—(Vol. vi. p. 105.)

(12.) Dr Cogan communicates a letter received from several medical men in Rotterdam. "In twenty cases variolous inoculation was superadded without producing the disease. In all these the wounds were inflamed on the first or second day, on the third day they were more inflamed and swollen; and, in some instances, to such a degree that we began to suspect a general infection would succeed; but these were, on the fourth day, less inflamed and swollen, and they disappeared on the fifth."—(Vol. vii. p. 251.)

(13.) Mr Thomas, Pimlico, tested sixty cases, "and never was able, in any one instance, to produce the least symptom of small-pox."—(Vol. viii. p. 168.)

In the above cases I have inserted such notes regarding local inflammation, etc., as are in part depended on by Dr Creighton to support his view that the small-pox inoculation had succeeded as well as it could do under the mild method of inoculation. But it will be seen that the method was not always mild; that there was not "in most cases a full and correct variolous pustule," and that the operators, even when led by the first appearances to look for variolous infection, had no hesitation in concluding that it had not occurred. It need hardly be insisted on that no such body of evidence could have come into existence had vaccination been nothing more than "a grotesque superstition."

JOHN C. M'VAIL.

(*To be continued.*)

Report on an Epidemic of Small-pox at Sheffield. By Dr BARRY. With an Introduction by the Medical Officer of the Local Government Board. London: H.M. Stationery Office: 1889.

WERE the vaccinationists in need of any further arguments and experience to support their views in the face of the temporarily increased activity on the part of the anti-vaccinationists due to the fillip their cause received from Dr Creighton's article in the *Encyclopædia Britannica*, these would be found in abundance and of a most convincing character in this volume, which is a monument to the skill and persevering industry of Dr Barry. The epidemic which raged in Sheffield during the winter of 1887–88 gave him the opportunity of studying afresh the whole question of epidemic small-pox in a large and densely populated town, and of judging of the effect produced by systematic vaccination. We do not intend to enter into this latter question, except merely to say that Dr Barry shows that, if the same rate of mortality had existed among vaccinated children as among unvaccinated, the rate of mortality would have been 4400 per 100,000 instead of 9 per 100,000. As it was, there were in all over 6000 people attacked by the disease, of whom close on 600 died. What the result would have been had Sheffield been an unvaccinated town makes one shudder to think of.

Among the many interesting questions discussed in connexion with the epidemic is that of the influence of one of the Borough Hospitals, situated in the centre of the town, as a disseminator of infection. Dr Barry, by means of numerous beautiful maps, shows how enormously the disease spread within a 4000 feet radius soon after this hospital began to be filled. Unfortunately the question of the aerial carriage of infection from small-pox hospitals to surrounding houses is not to be decided by the experience of Sheffield. It was a great opportunity missed, owing to the strange and unaccountable laxity shown in the administration of the Hospital. We read that "No special precautions were required to be taken by any of the officials—they were free to go in and out of the Hospital as they chose." The ambulance driver lived outside of the grounds; and if he were not available, a cabman was taken from a stance close at hand, and after harnessing his horse to the ambulance, drove *along with an attendant* to remove the case. No wonder that the Hospital proved a source of infection, and under the circumstances we are not surprised to find a very large number of cases occurring in the neighbourhood. An infectious hospital *in* a town has advantages and disadvantages, but that it necessarily spreads infection, when proper precautions are taken to prevent direct contagion, has still to be proved.

The futility of voluntary notification is well shown by the fact, that at the very commencement of the outbreak, when it was absolutely essential that every fresh case should be at once removed and isolated if an epidemic was to be avoided, only a quarter of the cases occurring at that time were notified to the authorities.

There are many other points of interest in the Report well worthy of attention, notably the working of the Small-pox Association.

We only trust that the price of the volume will not prevent its being studied by a large number of readers.

The Early Symptoms and the Early Treatment of Osteo-Arthritis, with Special Reference to the Bath Thermal Waters. By JOHN KENT SPENDER, M.D. Lond., Physician to the Royal Mineral Water Hospital, Bath. London: H. K. Lewis.

FEW diseases in their advanced stage have more persistently baffled the ingenuity of the physician than rheumatoid arthritis or osteo-arthritis, as the author of this essay prefers to call it, and Dr Spender's experience has been no exception to this. As physician to the Hospital at a popular spa, the waters of which are in high repute for affections of this class, he has had exceptional opportunities of becoming acquainted with the early manifestations of the disease, and is impressed with the necessity of energetically attacking them. The symptoms which he regards as indicative of the disease at its outset are given in order, and of these the most interesting and the most novel is the disturbance in the chromatogenous function of the skin, on which he places great diagnostic value. The treatment is rational, and to use his own words, he does not advance the quackish pretension that the benign springs of Bath are the pools of Bethesda. Altogether the author may be congratulated on his *brochure,* and the profession will find it useful. For scientific purposes the author would do well, however, to curb his tendency towards a too ornate style.

Select Methods in the Administration of Nitrous Oxide and Ether: A Handbook for Practitioners and Students. By FREDERIC HEWITT, M.A., M.D. Cantab., Instructor in, and Lecturer on Anæsthetics at the London Hospital, etc. London: Baillière, Tindall, & Cox: 1888.

THE author of this little book has made a trial of various methods of administering nitrous oxide and ether, and has used a modified Clover's closed ether inhaler, which can be used for one or both of these drugs. The directions given for the various procedures are short and clear, and bear the mark of personal experience. The apparatus recommended is very simple, and therefore ought to find general favour. The table of contents contains the following lists of chapters:—I. Introductory Remarks; II. The Administration of Nitrous Oxide; III. The Administration of Ether; IV. The Administration of Nitrous Oxide with a small quantity of Ether; V. The Administration of Nitrous Oxide as a preliminary to deep Etherization.

No reference is anywhere made to chloroform, nor to those cases in which prolonged anæsthesia is required, but where ether is contraindicated. Operators, however, who have decided to administer nitrous oxide and ether, either singly or in combination, will find in this little book a useful guide.

Massage and Allied Methods of Treatment. By HERBERT TIBBITS, M.D., etc. Second Edition. London: J. & A. Churchill.

IT seems somewhat absurd to suppose that massage, electricity, medical gymnastics, and many other subjects, can be treated effectively in a book of 120 pages of large type. One cannot expect to have more than the merest outline of these several methods, and the author admits that it is only a sketch. The lectures, of which the book is an abstract, were delivered to trained nurses and masseures at the School of Electricity, London. Like most other specialists, the writer is tempted to make too much of his own specialty, and cannot resist a passing sneer at the use of drugs. It must be admitted that the massage treatment has shown some remarkable results in suitable cases, but it is not the only "safe cure" for all diseases, as one might imagine on reading this book. The profession happily is not likely to abandon faith in appropriate drugs for the relief and cure of disease.

A chapter is added on the circulation of the blood, digestion,. and absorption. It is too elementary to be of use to medical readers, and too technical for those who have not studied medicine.

Altogether the work treats the subjects discussed in a most superficial style, although it is claimed that a proper knowledge of massage can only be acquired by long and persevering practice.

The Asclepiad. Second Quarter, 1889. By B. W. Richardson, M.D., &c.: London: Longmans, Green, & Co.

This number of Dr Richardson's well-known and interesting periodical contains several articles of characteristic originality and value. In the "Opuscula" is a reference to the importance of light in wards and sick-rooms. It is followed by a paragraph advocating decoration, and condemning the white-washed roofs and walls that are so common in hospitals. But the most valuable article is the first, on Intestinal Obstruction, consisting mainly of a paper read before the Medical Society of London in February last. This paper has been commented upon favourably in most journals already, mainly on account of its outspoken advocacy of early surgical interference. We do not hesitate to take the same view, though we do not agree with Dr Richardson in everything. Taking his "summary submitted in order to facilitate discussion," we think that it is not always safe to wait for fæcal vomiting, for this very plain reason, that vomiting is not always present, as, for instance, in cases of volvulus of the sigmoid. Secondly, Dr Richardson himself produces very strong arguments (the toxic influence of fæces in the stomach, etc.), against waiting till fæculent vomiting occurs. Under his third head we thoroughly agree with him that the worse or more complicated the case the more it demands interference, because the chance of life is so much less if the case is left alone. But we would go farther. We would say, "In all doubtful cases operate as soon as possible."

Dr Richardson, like the proverbial lady, puts a very important part of his paper in the postscript. He there tells us that the non-passage of flatus per anum is a most important symptom in deciding as to the necessity for operation. We think it may be a more reliable test symptom than fæcal vomiting. But we would prefer a time test. Time, we believe, is everything. In a case in which the diagnosis is made out, the treatment should be plain, and need not be discussed. But in a case in which there is doubt, operation is demanded on account of the doubt. In the words of Dr Richardson, "*because we are in the dark let us let in the light.*" And the sooner this is done the better for the patient. We think it might frequently be of value to the patient if the custom of looking on "obstruction" cases as first medical, and then, after failure of so-called milder remedies, surgical, were departed from, and all such cases placed under the care of the surgeon from the first.

Seventeenth Annual Report of the Local Government Board, 1887-88. *Supplement containing the Report of the Medical Officer for* 1887. 1888.

Supplement in continuation of Report of the Medical Officer, containing Reports on Diarrhœa and Diphtheria. London: H.M. Stationery Office, Eyre & Spottiswoode: 1889.

NOTHING but praise can be given to this volume, and to the medical officer who with such conspicuous skill and wise discrimination directs and summarizes the work of the various medical officials of the Board. We know of no publication which can show more laborious and conscientious work or a higher general degree of excellence, and in support of this statement one requires only to instance, among others, the report by Mr Spear on an epidemic of enteric fever at Mountain Ash, where he was able to discover the cause in a leakage of sewage into the public water supply; also a report by the same gentleman on "Meat Poisoning" at Retford, where no fewer than eighty persons were affected, of whom one died. As in similar previous inquiries, the exact cause of the poisoning was not discovered, although Dr Klein considers that he has separated a bacillus capable of imparting to the pies their morbific quality. Then, again, in the joint report of General Scott and Mr Power on "Eels in Water Mains," we have a capital example of the logical reasoning which is indispensable to the successful investigation and elucidation of sanitary inquiries. As Dr Buchanan says in regard to this report, "How boundless would seem to be the area of that knowledge which can be turned to account for the sanitary advantage of a community!" At the same time, we are rather doubtful about accepting Mr Power's view as to the mode of entry of the eels into the main, and would suggest that the point can be easily decided in this case by observation or experiment.

There are other special reports of more or less interest, but we will only refer to two, which have been published separately, in folio, on account of their importance and of the large number of plates and diagrams. These are the long expected report of Dr Ballard on the "Causes of the Annual Mortality from Diarrhœa;" and secondly, that of Dr G. B. Longstaff on the "Geographical Distribution of Diphtheria in England and Wales."

Dr Ballard's inquiry was begun at Leicester in 1880, and the primary intention was to confine it to that town; but it was soon found that in order to come to an accurate and useful result, the inquiry would need to embrace the annual outbreaks in other towns and districts. This Dr Ballard has done, and the report affords ample evidence of his painstaking and indefatigable zeal. As yet it is only provisional, and he expressly reserves to himself the power of altering any of the propositions laid down. His provisional hypothesis as to the causation of diarrhœa is to this effect, viz., that it is due to the action in the human body of a virulent chemical poison, manufactured by a micro-organism not yet isolated, and which resides ordinarily in the superficial layers of the earth.

Dr Longstaff's paper is full of interest, and shows how diphtheria has always tended to prevail in thinly populated districts rather than in towns, and that it is to infection brought in from the country that we must ascribe many of our town epidemics.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXVIII.—MEETING XI.

Wednesday, 5*th June* 1889.—Dr JOHN SMITH, *President, in the Chair.*

I. Election of Member.

The following gentleman was elected an Ordinary Member of the Society:—D. C. Benjamin Bell, L.R.C.P. and S.E., Kirkwall.

II. Exhibition of Patients.

1. *Professor Grainger Stewart* exhibited a case of SPASMODIC PARAPLEGIA which had existed from childhood. The patient was a young man of 23. The main facts of his family history are:—He is the second child of a family of six; there was no special feature about the labour; his father and mother and the other five children of the marriage are perfectly healthy; there is no indication anywhere of neurotic tendency in the family. In regard to the patient, his mother noticed that when a child in arms he never sat up, was always dull and listless, and inclined to be peevish. When he

began to walk it was observed that his limbs "were not right," and the doctor who then saw him advised that he should be sent to the country. The present condition:—The sensory functions are normal. The motor functions show marked peculiarity. Organic reflexes normal. The skin reflexes are exaggerated. On tickling the soles of the feet the muscles of the calf sharply contract. The patellar reflex is greatly exaggerated, and the movements are begun and ended in a very abrupt manner. Ankle clonus is absent. The elbow reflex is exaggerated. Co-ordination is very defective on the left side, and this deficiency is much aggravated when the patient closes his eyes. If the left arm be extended the fingers perform a series of rhythmical movements, and these are intensified if the patient direct attention to them. In the right arm co-ordination is practically normal. In the left leg there is a certain want of accuracy in performing definite movements, but ultimately all ordinary movements can be done. In the right leg the movements are better done than by the left, but the termination of the movement is usually brought about with a jerk. There are rhythmical movements of the toes of both feet, always worse if attention is drawn to them. The gait is spastic; the legs seem to be dragged. The moment the heels touch the ground there is a sharp contraction of the calf muscles. There is great difficulty in articulation; stammering, too, is present in very bad form. Both of these conditions are much aggravated by excitement. If the patient gets thoroughly interested in a subject and is not excited, he scarcely stammers at all. All the rhythmic movements cease during sleep. There have never been convulsions. In describing similar cases Dr Ross remarks that children are liable to be affected with a spasmodic paraplegia from disease of the vertebral column, or from any other variety of transverse myelitis. But besides these there are congenital cases occasionally met with, in which the gait is spastic, the muscles of the leg exhibit spastic features, the patellar tendon reflexes are exaggerated, and the feet generally maintained in a position of talipes equinus. The arms are not generally involved, but there is often difficulty of articulation; and the form of the head is often in one way or other altered. When the disease is confined to the legs the intelligence is often very good, but it may be diminished. There is no tendency to epileptic convulsions. Dr Ross gives four cases illustrative of the condition, two of them occurring in boys of eight, and two in boys of four years old, and in all of them the symptoms had been observed in infancy or in early childhood. In two, labour had been difficult. In two, convulsions occurred during infancy, and in the others there had been no such symptoms. Professor Stewart, after demonstrating his case, remarked that the process must be due either to a primary or secondary sclerosis of the antero-lateral columns, but he had not had any opportunity of investigating such a case after death.

Professor Grainger Stewart also introduced a patient suffering from a peculiar spasmodic affection, which may for convenience be designated as a variety of CHOREA MAJOR. Henry Hudson, a tinsmith, aet. 47, from Stockton-on-Tees. He states that his mother, who died at the age of 56, suffered from a disease like his for the last three years of her life; and a sister, who died at the age of 40, suffered from it for the last four years of her life. In her case the disease first appeared during her third pregnancy. He had six brothers, and other four sisters, none of whom exhibited any nervous disease. He was till five years ago very intemperate, but since his illness began he has been a teetotaler. His home is healthy. His work exposes him to great heat, but no poisoning occurred among his fellow-workmen. Before his illness he fell from a roof; he first landed on his head, which was cut; and he was unconscious for a time, but soon recovered. Two months later a similar fall occurred, but he lighted on his hand, and was not unconscious. To these falls he attributes the onset of his symptoms. The first symptom occurred five years ago, and consisted in slight shaking or jerking of the muscles of the leg on voluntary movement; at first slight, but gradually increased till he experienced difficulty in walking. The jerking could not be controlled by the will. He gradually got worse. The arms began to be affected two years after, and only on making effort. About six months ago jerkings in the arms became constant, and the head and face showed slight movements, and he became unsteady when his eyes were cloudy or when he was in the dark. Four months ago the movements began to persist during sleep. Speech has been affected for two years. The pupils react to light and accommodation. There is no nystagmus. There is slight deafness. He has had much mental worry and anxiety of late. He is healthy in all respects except for the nervous symptoms, and a localized eczema on the outer surfaces of both forearms. The nervous system shows no sensory change. The organic reflexes are natural. The plantar and other skin reflexes are well marked. There is no ankle clonus. The patellar tendon reflexes are well marked on both sides. There is no paralysis of any muscles, but the voluntary movements are interfered with by the jerking movements. Co-ordination is only disturbed by these involuntary movements, and the main directions of any movement attempted is attained. When he lies at rest in bed there is irregular and coarse choreic jerking, which now ceases during sleep, but he says at one time this persisted. The right arm is more affected than the left. He generally lies with his hands grasping the bar at the top of the bed. The movements are precipitated if he strokes his head. The other nervous functions are practically normal, but there is slight excess of sensitiveness to the hot sponge on the lower part of the lumbar region and over the sacrum. After pointing out the features of the case, Prof. Stewart said that he was not able at present to indicate the precise seat or nature of the lesion. The patient is taking arsenic, and having shower baths, bromides, and hyoscyamine. Other remedies having failed, it is intended to try and subdue the habit by the use of chloroform.

2. *Dr Allan Jamieson* showed a man, aged 32, suffering from a peculiar form of BALDNESS, evidently the result of a folliculitis extending from a common centre. This was the second case of the kind he had seen, the first being that of a little girl whom he had also shown to the Society. The present case, when five years old, suffered from a pustular affection of the head. Since then he had been subjected to repeated attacks of inflammation of the individual follicles, followed by the loss of hair on the parts affected. On the back of the neck the same condition had arisen, and had gradually extended, leaving a part bare, and with a cicatricial appearance. Under the use of ointments of sulphur and ammoniated mercury the condition had become quiescent, and no more hairs had fallen out. This condition had been drawn attention to by Quincke, who had described it as a *destructive folliculitis.* The same affection was not uncommon on the cheeks, causing a form of sycosis. There was reason to believe that there was a still slighter form in which there was almost no pustulation, and which had been confused with alopecia areata. Apparently anti-parasiticide remedies did most good. The girl got well under the same ointments as the man was using. There was nothing specific about it. Quincke had described a microbe which, when cultivated and rubbed into the skin of a rat, had produced baldness of the same character.

3. *Mr A. G. Miller* showed a boy suffering from LUPUS OF THE TONGUE.

4. *Dr Scott Lang* showed a woman who had been operated on by Dr P. M'Laren for VARICOSE VEINS. A long elliptical portion of skin, measuring 2 inches at its widest part, was removed from the back of either leg. The incision extended from the back of the knee to the tendo-Achilles. A natural elastic stocking had thus been formed with a very satisfactory result.

III. EXHIBITION OF SPECIMENS.

1. *Dr Maxwell Ross* showed a LARYNX and TRACHEA from a case of DIPHTHERITIC CROUP. The patient was a girl, aged 18 months, of a very strumous habit. She was seen in consultation with Dr Macdonald Robertson, and as the stenosis was rapidly increasing, it was resolved to perform intubation of the larynx. This was done by means of O'Dwyer's instruments, and the ease with which the operation was performed and the instant relief given contrasted favourably with the difficulties surrounding the operation of tracheotomy in children of that age. She wore the tube for sixteen hours, when a violent cough occurred, and it was expelled from

the mouth. It was replaced within half an hour, but the relief was not so great as on the first occasion, and death occurred in about an hour. At the post-mortem examination false membrane was found extending down to the bronchi. Though the case ended fatally, the experience gained was favourable to the operation of intubation. There was no difficulty with the swallowing of liquids. Brandy, water, and milk were taken with ease, and even greedily, by the child. Dr O'Dwyer had pointed out that many of the difficulties in swallowing were due to the condition of the epiglottis rather than the presence of the tube, and in this instance the epiglottis was unaffected. Malposition of the tube might, however, be a cause of difficulty, the apex of the triangular head being turned forward so as to project against the epiglottis instead of resting between the arytenoid cartilages, as intended by O'Dwyer.

2. *Dr A. Bruce* exhibited in the side-room a series of PAINTED CASTS of pathological specimens in Cathcart's medium.

IV. Original Communications.

1. *Dr G. A. Gibson* gave an abstract of a paper on CHEYNE-STOKES RESPIRATION, which is being published in full in the *Edinburgh Medical Journal.* He mentioned a great variety of affections in which it had been observed, all of which are characterized by a lowering of the activity of the nervous centres, and described the different symptoms with which this phenomenon is usually associated. In regard to the probable causation of this respiratory rhythm, he discussed the most important of the theories which have been advanced to account for its occurrence, classifying the modern views according as they attributed the periodic changes of the breathing to alterations in the cells of the nerve centres produced directly by the effects of exhaustion or of some analogous condition, or to alterations in these centres produced indirectly through modifications in the state of the circulation. The latter class of explanations, such as the hypothesis of Filehne, not only failed to account for many of the appearances present, but directly opposed the facts, and were therefore untenable. The former class, and more especially the theories of Luciani and Rosenbach, explained all the appearances present, and met all objections which had been urged against them; they were therefore alone worthy of acceptance. In conclusion, he called attention to the light which had recently been thrown on the whole subject by the researches of Marckwald.

The President said he felt sure he was only giving expression to the desire of the Society when he conveyed its thanks to Dr Gibson for his very valuable communication. The subject was an important and exceedingly interesting one. So far as the automatic and the reflex nature of the respiratory act seemed to be concerned in this affection, it appeared a point where some of the physiologists he saw present might render considerable assistance. The respiratory centre comprehensively, and without reference to the possible existence of a direct as well as a reflex region, so to speak, had been alluded to; but the characteristic symptoms of the disease suggested that they might perhaps to some extent be analogous to the irregular respiratory movements occurring after division of the sensory nerves, and supposed to be due to the passage of blood deficient in oxygen stimulating an inspiratory centre, and of blood surcharged with carbonic acid acting in the same way on an expiratory centre in the medulla, and independent of the function of the vagi. He trusted a full discussion on the paper would take place.

Professor Grainger Stewart mentioned the case of a gentleman he had known for a very long time who exhibited Cheyne-Stokes breathing regularly, and without any marked increase, for a period extending over twenty-five years. He had never met with another case that exhibited this in such prolonged and regular fashion. The patient died, at the age of 60, of fatty degeneration of the heart, with pulmonary apoplexy and a low type of pneumonia supervening. As to the theories, it seemed to him that the views of Luciani and Rosenbach were those to which most importance attached at the present day.

Dr James said his observations on Cheyne-Stokes breathing in connexion with sleep led him to believe that the waking period was towards the end of the pause. If they could get tracings of the respiration before it was well marked, they would get a good deal of information as to its ætiology. He believed the difficulties of breathing experienced by heart cases during the night were indications of the commencement of Cheyne-Stokes respiration. A pause occurred, the patient became uncomfortable as his blood became venous, and, wakening up, got relief and then went off to sleep. The time when these cases wakened up was at the end of the pause, when the blood was most venous.

Dr Clouston remarked that in the third stage of general paralysis of the insane, where they had the greatest amount of atrophy of the brain cortex, just before death they had the mode of breathing he had always called Cheyne-Stokes. It was essentially a rhythmical respiration. He was pleased to see that those who had most carefully studied this subject brought this form of respiration under the law of periodicity, which covered every one of the higher nervous functions, and showed it to be an irregularity in this periodicity. (He did not admit a distinction between rhythm and periodicity.) What this theory did not explain was why the normal periodicity of breathing should be interfered with and become an abnormal periodicity.

Dr James Ritchie said that Luciani had drawn a very definite line of distinction between reflex and automatic centres, and Dr

Gibson agreed with him. Luciani believed that the latter have the power not only of transforming impulses, but of originating action. Dr Ritchie believed an automatic centre to be an arrangement of nerve cells, whereby sensory impulses, whether from the higher centres or from peripheral end-organs, may be converted into *co-ordinated* motor impulses usually of a complex kind; but that the automatic centre has no autogenetic power, it can only liberate energy in response to stimuli, although these stimuli are not at first sight apparent, and in ordinary working are independent of the action of the higher centres. He thought that the series of morbid phenomena under discussion would be better understood if we recalled certain physiological facts. Certain elementary structures had peculiar functions,—the function of muscular fibre is to contract under certain stimuli; the function of the nerve cell is to give out a certain kind of energy when stimulated; the period of activity is in both cases followed by a period of rest. If the stimulation is repeated the discharge of nerve energy is renewed, the amount of energy being in proportion to the stimulus. But a nerve cell may be exhausted, and its power to respond to stimuli depends not only upon the nature and frequency of the stimulus, but also upon its nutrition from second to second by the blood circulating near it. In pathological states nerve cells vary as regards their irritability, and as regards the quantity and quality of nerve energy which they give off. He understood that the cells of the medulla do not differ from other nerve cells in their nutritional relations, and he believed that Filehne, Blaise, Brousse, and others, are completely in error when they assert that the cells of the medulla are stimulated to action by carbonic acid in the blood circulating round them. There is no evidence that carbonic acid does not act on these cells as on other nerve cells—lowering their vitality and diminishing their irritability. They are nourished and they respond to stimuli in the same way as other nerve cells. The special stimuli for the respiratory centre, in the relation in which we are considering it, viz., apart from voluntary and involuntary impulses from the higher centres, are impulses from the peripheral terminations of the vagi in the lungs and of the systemic nerves in the tissues. In viewing the affections in which Cheyne-Stokes respiration occurs as a symptom, one is struck with their variety; but there is one condition which seems to be common to them all, that condition is a lowered vitality of the central nervous system. The fact that Cheyne-Stokes respiration occurs during sleep, during hybernation, after exhaustion, during the action of certain drugs, goes to prove that a lowered nerve tone is required for the production of the phenomenon. Dr Ritchie had under his care for some years two patients in whom the variability of the symptoms was remarkable. In one of these, an old lady, the symptom was not very evident when her health was fairly good; but whenever her general tone was lowered, or if the blood became loaded with morbid material during attacks of gastro-intestinal disorder, the symptoms became very pronounced —so much so at night as to interfere with her daughter's sleep. He believed that Cheyne-Stokes breathing was only one of several symptoms which arose in connexion with a common cause, but which are not necessarily dependent upon each other. One or more of them may be absent—the periodic alterations in the pupils, of the pulse, muscular twitchings, changes in the mental state. He agreed with Rosenbach that these might be grouped under the name of the Cheyne-Stokes phenomenon. The conditions essential to their production are—a lowered tone of the nerve centres, an altered irritability, and an increased tendency to exhaustion of the nerve cells. The sequence of events in Cheyne-Stokes respiration would be thus explained (somewhat in accordance with Traube)—after a period of excessive respiratory action the peripheral stimuli diminish, the nerve cells in the respiratory centre are exhausted, and there is a period of apnœa. During this time the nerve cells are recovering energy, and at the same time waste products are accumulating round the extremities of the nerves afferent to this centre, most rapidly round those of the vagus. A gradually increasing stimulus is sent on to the centre, and it responds mildly; it is proved experimentally that no amount of stimulation by the vagus can produce dyspnœa; but the stimuli from the extremities of the systemic nerves now come into play, the combined impulse now reaches the recuperated nerve cells; these being in a debilitated condition act in an unusual fashion, and a greater amount of energy is sent out. As the blood becomes ærated, the peripheral stimuli firstly from the systemic nerves, then from the vagi diminish, they act upon exhausted cells producing a diminishing result, a period of apnœa succeeds, to be again followed by a similar cycle of events. The alternation from the rhythmic to the periodic type occurs in consequence of an altered condition of those nerve cells, the activity of which dominates the regularity of the function. When the heart of the frog is removed from the body the same alteration occurs, with an additional factor. The cardiac ganglia have their vitality lowered, and the peripheral stimuli from the walls of the heart to these ganglia are altered in consequence of the absence of the circulating fluid.

Dr Gibson said he had had several cases in which Cheyne-Stokes respiration had lasted over several years, and one case related to him by Dr Muirhead had gone on for over twenty. As to the waking of a patient exhibiting Cheyne-Stokes respiration, it might occur at any period, but periodic awaking almost invariably occurred at the height of the forced breathing. Dr Clouston's remarks went to the root of the matter, and he confessed that the explanations did not get at the real reason of the phenomena. He thought he had seen periodicity of consciousness occurring in cerebral meningitis without any Cheyne-Stokes respiration, and, if he were correct in this observation, the fact went a long way in proof of the views of Luciani and Rosenbach. He was, finally, inclined to agree with Dr Ritchie's ideas on automatism and reflex action.

2. *Dr Albert E. Morison*, Hartlepool, read his paper on the SURGICAL TREATMENT OF EMPYEMA, which appeared at p. 123 of this Journal.

Prof. Chiene congratulated Dr Morison on his results, and said he was glad to see him still hold to the earlier views with regard to the use of the spray in antiseptic surgery. He agreed with him that excisions of portions of several ribs was not required except in cases allowed to become septic; but in his experience of empyema in adults he found it sometimes difficult to get free drainage without excising subperiosteally a portion of one rib.

Dr Bedford desired to draw attention to one part in Dr Morison's paper, viz., the use of the hypodermic needle for making the exploratory puncture of the pleura. In the first of a series of cases under Dr Bedford's care at the Gesto Hospital, he had made such a puncture, *secundum artem*, with an extra long hypodermic needle, as he had been taught to do at Edinburgh. The puncture was made below the angle of the scapula posteriorly, and the case was one of purulent pleuritic effusion of some months' duration in a lad of 20. The result was the patient started, levered the needle against his more than normally approximated ribs, and broke it into the puncture. Although a cross incision was at once made over the needle, it was not secured till a few days later, when Dr Bedford opened and drained the pleural cavity. Professor Greenfield had informed Dr Bedford of the occurrence of the same accident to himself and to a friend of his, a well-known London surgeon, both of whom now used special needles. While, doubtless, a needle so introduced generally became encysted, and gave rise to no further trouble, yet the accident was an unpleasant one. Dr Bedford had since employed, without any danger of such an occurrence, a fine Dieulafoy needle, and found it worked very well.

Dr James drew attention to the value of repeated tapping in cases of empyema, which he had practised with much success. He asked where Dr Morison considered he was most likely to find pus, and at what point he made his free incision into the chest.

Mr Caird said empyema in children was very amenable to treatment, but the time given by Dr Morison for a successful result was very short. In nine or ten cases he (Mr Caird) had treated in adults he could corroborate Dr Morison in everything except that he found it of importance to excise a portion of rib.

Dr Hodsdon pointed out that the majority of Dr Morison's cases was in children, and in them it was exceptional to meet with failure if they were properly treated. In young children it was very difficult to get in a good-sized tube without removing a portion of rib. He had not seen the spray used in any of the cases that came under his notice, and all of them got well.

Dr E. Price asked if it was necessary to use a drainage-tube in children at all. Quite lately a case occurred in his practice in which the pus discharged through the opening made by an aspirator needle, and the child did well.

Dr Webster related a case of a man in which the tube came out on the tenth day, and could not be reinserted. The patient did well. He thought the case showed that drainage-tubes were sometimes allowed to remain too long in the wound.

Dr Morison said he had not found it necessary to remove a portion of rib in any of his cases. When seen before there was an external opening, and putrefaction was prevented. The accident which happened to Dr Bedford with the hypodermic needle had also happened to him. The needle broke, but did not pass into the pleural cavity. To prevent this a stouter needle than that used in ordinary hypodermic syringes was required. In reply to Dr James, he thought the chest should be incised as soon as pus was found. In a number of cases he tapped frequently, but with the result that he had to incise afterwards. The difficulty of getting a full-sized drainage-tube into the pleural cavity could be got over by the introduction of sinus forceps without excising a piece of rib. If left in too long the tube would certainly cause erosion of rib, but under antiseptic treatment the tube never required to be left in long enough to produce this effect. As to the spray, Dr Hodsdon and he differed, so that he would not discuss that point. He believed a drainage-tube was necessary in all cases. Ten days seemed a rather short time to keep it in.

CARLISLE MEDICAL SOCIETY.

SESSION V.—MEETING IV.

Thursday, 14th February 1889.—Dr DOUIE *in the Chair.*

Dr Meikle showed several cases—1. Epithelioma of the Scalp, of great size. 2. Case of Hæmatoma of both Ears after a severe burn. There was considerable discussion on this case, several members disputing the diagnosis of hæmatoma. 3. Case of an Enlarged Tongue, where the diagnosis between hypertrophy and cystic swelling was in doubt. 4. Case of Optic Neuritis.

The report of the Library Committee was adopted.

Dr Lediard showed the following specimens:—1. A Tongue removed three years ago. 2. Ovarian Cyst, with the opposite ovary, which was unusually large. 3. Encephaloid Cancer of Testicle, with microscopic slide. 4. Epithelioma of Cheek. 5. Caseous Gland of large size. 6. Sac of a Hernia with undescended Testicle.

MEETING V.

Thursday, 14*th March* 1889.—Dr DOUIE *in the Chair.*

Dr Haswell showed a case of Optic Atrophy.

Dr Walcot read a paper on Renal Surgery, and a discussion followed, in which Drs Barnes, Graham, Helm, Haswell, and Douie took part.

MEETING VI.

Thursday, 11*th April* 1889.—Dr DOUIE *in the Chair.*

The following were elected as office-bearers for next Session:—*President*, W. Douie, M.D., Carlisle; *Vice-President*, Norman Walker, M.D., Dalston. Dr Norman Walker was asked to retain the duties of Secretary for the present.

The meeting was mostly occupied with business matters relating to the Library and Rules of the Society.

Dr Lediard showed a case of Ichthyosis of the Tongue accompanied by a cancerous condition.

INFORMATION REGARDING MEDICAL EDUCATION AND EXAMINATIONS.

THE Royal College of Physicians of Edinburgh, The Royal College of Surgeons of Edinburgh, and the Faculty of Physicians and Surgeons of Glasgow, have made arrangements by which, after one series of Examinations, held in Edinburgh or in Glasgow, or in Edinburgh and Glasgow, the Student may obtain the Diplomas of the Three Co-operating Bodies.

The object of the Joint Examination is to give to Students facilities for obtaining, after one series of Examinations, the Qualifications in Medicine and Surgery of the three Scottish Corporations. Students passing the Final Examination, which is a qualifying Examination under the Medical Act, 1886, and entitles the holder to practise Medicine, Surgery, and Midwifery, will be enabled to register three Diplomas under the Medical Acts, viz.—Licentiate of the Royal College of Physicians of Edinburgh, Licentiate of the Royal College of Surgeons of Edinburgh, and Licentiate of the Faculty of Physicians and Surgeons of Glasgow.

Female Candidates are admitted to the Examinations for this Qualification equally with male candidates; and throughout these Regulations the masculine pronoun is to be read as standing for Candidates irrespective of sex.

The three Co-operating Bodies grant their *Single* Qualifications only to Candidates who already possess another and opposite Qualification in Medicine or Surgery, as the case may be. Thus the Royal College of Physicians of Edinburgh may grant its License on Examination to Candidates already possessed of a recognised British, Irish, Indian, or Colonial Qualification in Surgery, and the Royal College of Surgeons of Edinburgh, and the Faculty of Physicians and Surgeons of Glasgow, may each grant its License on Examination to Candidates already Licentiates in Medicine of any British, Irish, or Colonial College whose Preliminary Examination and course of Professional Study is proved to be sufficient, or Graduates in Medicine of any British, Irish, Indian, or Colonial University. Copies of the Regulations for the Single Qualification of any of the Bodies may be had on application to the respective Secretaries.

N.B.—Under the provisions of the Medical Act of 1886, these *Single* Qualifications do not confer the right to registration, but may be registered as additional Qualifications by those already on the Register.

GENERAL REGULATIONS.

Candidates shall be subjected to three Professional Examinations, herein called the First Examination, the Second Examination, and the Final Examination, to be conducted at separate times, partly in writing, and partly practically and orally.

Opportunities of entering for each of these Examinations will be presented six times in each year—four times in Edinburgh and twice in Glasgow. A Candidate at any of these Examinations may present himself either in Edinburgh or Glasgow, irrespective of the place at which he may have previously been examined.

Candidates who are unsuccessful, either at the First, the Second, or the Final Examination, shall be remitted to their studies for a period to be determined by the judgment of the Examiners, but not in any case less than three months.

All the subjects of the respective Examinations required from a Candidate shall be taken up at one period.

Candidates must pass in all the subjects at the Written part of the Examination before they can be admitted to the Oral Examination.

A Candidate who obtains not less than 65 per cent. of the marks at the Oral Examination, and who has gained not less than 55 per cent. in the same subject at the Written Examination, may obtain an absolute Pass in any subject of the First, the Second, or the Final Examination, although he may at the same time fail in other subjects. In such a case a Record shall be made of the subject or subjects passed, and the Candidate shall be exempt from re-examination in them.

Applicants for admission to any of the Examinations are required, on entering, to lodge with the Inspector of Certificates a schedule (forms for which are supplied) showing the Courses he has attended qualifying for admission.

No Candidate can be held to be entered for any Examination till he has sent in the proper schedule, proved his course of study, and paid the appropriate fee.

At the Oral Examinations Candidates shall be examined as far as possible in the order of their entry.

The subjects of the Second Professional Examination should, as far as possible, be studied only after the First Examination has been passed; and similarly, the subjects of the Final Examination should, as far as possible, be studied only after the Second Examination has been passed. By adhering to this course the student will have his attention occupied by preparing for only one Examination at a time.

Candidates, qualified by having attended the necessary courses of study, may however enter for two or more Examinations, to be passed successively at the same period, the condition being that failure to pass in all the subjects of any Examination precludes the Candidate from admission to the succeeding Examination. The necessary Schedule and Certificates must be lodged, and the fees for these respective Examinations paid, at the period of entering for the earliest of them. In the event of failure, the fee paid for any subsequent Examination to which a Candidate had not been admissible will be returned to the Candidate.

No Candidate shall be admissible to Examination who has been rejected by any other Licensing Board within the three preceding months.

SPECIAL REGULATION.

Any Graduate in Medicine and Surgery of a British or Irish University, and any Graduate or Licentiate in Medicine and Surgery of the Universities and Colleges described hereafter, who shall have passed such an Examination or Examinations as shall comprise the subjects of the First and Second Examinations of this Board, and be proved to have been co-extensive therewith, and who shall have completed not less than four years' Medical Study, including all the subjects of this curriculum, shall be admissible to the Final Examination of this Board. Any Candidate so admitted shall pay a fee of twenty-five guineas at the time of entering for the Examination. The sum of £20 will be returned to unsuccessful Candidates.

The following are the Names and Addresses of the Inspectors of Certificates:— In Edinburgh:— Mr James Robertson, Solicitor, 1 George Square. In Glasgow:—Mr Alexander Duncan, B.A., Faculty Hall, 242 St Vincent Street.

The following are the General Medical Council's Requirements for the Preliminary Examination of Medical Students:—

No person shall be allowed to be registered as a Medical Student unless he shall have previously passed a Preliminary Examination in the subjects of General Education as specified in the following list:—1. English Language, including Grammar and Composition; 2. Latin, including Grammar, Translation from specified authors, and Translation of easy passages not taken from such authors; 3. Elements of Mathematics, comprising (*a*) Arithmetic, including Vulgar and Decimal Fractions; (*b*) Algebra, including Simple Equations; (*c*) Geometry, including the first book of Euclid, with easy questions on the subject-matter of the same; 4. Elementary Mechanics of Solids[1] and Fluids, comprising the Elements of Statics, Dynamics, and Hydrostatics; 5. One of the following optional subjects:—(*a*) Greek; (*b*) French; (*c*) German; (*d*) Italian; (*e*) any other Modern Language; (*f*) Logic; (*g*) Botany; (*h*) Zoology; (*i*) Elementary Chemistry. These subjects may be passed at one or more Examinations.

The following List of Examining bodies are recognised for registration as Medical Students, provided that in all cases the subject of Mechanics, as above set forth, be shown to have been included in the Examination:—

(1.) UNIVERSITIES OF THE UNITED KINGDOM.

Oxford.—Examination for a Degree in Arts; Responsions; Moderations; Local Examinations (Senior), Certificate to include Latin and Mathematics; Local Examinations (Junior), Certificate to include Latin and Mathematics; and also one of the following optional subjects:—Greek, French, German.

Cambridge.—Examination for a Degree in Arts; Previous Examination; Local Examinations (Senior), Certificate to include Latin and Mathematics; Local Examinations (Junior), Certificate to include Latin and Mathematics, and also one of the following optional subjects:—Greek, French, German. Higher Local Examinations.

Durham.—Examination for a Degree in Arts; Examination for Students at the end of their first year; Examination for Certificate of Proficiency.

Oxford and Cambridge Schools' Examination Board.—Certificate to include the following subjects, an adequate knowledge of English Grammar and Orthography, as shown in the course of the Examination, to the satisfaction of the Examiners, being held as conforming to the requirements of the Medical Council in regard to those subjects:—(*a*) Arithmetic, including Vulgar and Decimal Fractions; (*b*) Algebra, including Simple Equations; (*c*) Geometry, including the first two books of Euclid; (*d*) Latin, including Translation and Grammar; (*e*) and one of the following optional subjects:—Greek, French, German.

London.—Examination for a Degree in Arts or Science; Preliminary Scientific (M.B.) Examination; Matriculation Examination.

Victoria University.—Preliminary Examination, Latin to be one of the subjects; Entrance Examination in Arts, to include all the subjects required.

Edinburgh.—Examination for a Degree in Arts; Preliminary Examination for Graduation in Science or Medicine and Surgery. Local Examinations (Senior Certificate); Certificate to include English Literature, Arithmetic,

---

[1] In the case of Students in Universities with a prolonged curriculum, where the Examination in Mechanics required for their Degree is taken at a more advanced period of study than before commencing Medical Education, Registration can be effected only on having passed the Examination in Mechanics, but their Registration may be then antedated to the period at which the Preliminary was passed.

Algebra, Geometry, Latin, and also one of the following optional subjects :—Greek, French, German. Local Examinations (Junior Certificate); Certificate to include English Literature, Arithmetic, Algebra, Geometry, Latin, and also one of the following optional subjects :—Greek, French, German.

Aberdeen.—Examination for a Degree in Arts; Preliminary Examination for Graduation in Medicine and Surgery; Local Examination (Senior Certificate); Certificate to include English Literature, Arithmetic, Algebra, Geometry, Latin, and also one of the following optional subjects :—Greek, French, German. Local Examinations (Junior Certificate); Certificate to include all the subjects required.

Glasgow.—Examination for a Degree in Arts; Preliminary Examination for Graduation in Medicine and Surgery; Local Examinations (Senior Certificate); Certificate to include English Literature, Arithmetic, Algebra, Geometry, Latin, and also one of the following optional subjects :—Greek, French, German. Local Examinations (Junior Certificate); Certificate to include all the subjects required.

St Andrews.—Examination for a Degree in Arts; Preliminary Examination for Graduation in Medicine and Surgery; Local Examinations (Senior Certificate); Certificate to include English Literature, Arithmetic, Algebra, Geometry, Latin, and also one of the following optional subjects :—Greek, French, German. Local Examinations (Junior Certificate), to include all the subjects required.

Dublin.—Examination for a Degree in Arts; Public Entrance Examination; General Examination at end of Senior Freshman year.

Queen's University (Ireland)[1].—Examination for a Degree in Arts; Entrance or Matriculation Examination; Previous Examination for B.A. Degree; Local Examinations for Men and Women; Certificate to include all the subjects required by the General Medical Council.

Royal University of Ireland.—Matriculation Examination; Examination for a Degree in Arts.

(2.) OTHER BODIES NAMED IN SCHEDULE (A) TO THE MEDICAL ACT (1858).

The Society of Apothecaries of London.—Examination in Arts.

Royal Colleges of Physicians and Surgeons, Edinburgh.—Preliminary (combined) Examination in General Education.

Faculty of Physicians and Surgeons of Glasgow.—Preliminary Examination in General Education.

Royal College of Surgeons in Ireland.—Preliminary Examination, Certificate to include Mathematics.

(3.) EXAMINING BODIES IN THE UNITED KINGDOM NOT INCLUDED IN SCHEDULE (A) TO THE MEDICAL ACT (1858).

College of Preceptors. — Examination for a First Class Certificate, or Second Class Certificate of First or Second Division, Algebra, Geometry, Latin, and a Modern Language having been taken.

Intermediate Education Board of Ireland; Junior, Middle, and Senior Grades.—Certificate in each case to include all the subjects required by the General Medical Council.

Queen's College, Belfast; Queen's College, Cork; Queen's College, Galway.—Matriculation Examination.

St David's College, Lampeter.—Responsions Examination to include all the subjects required.

Educational Institute of Scotland.—Preliminary Medical Examination.

Pharmaceutical Society of Great Britain.—Preliminary and Minor Examinations *pro tanto.*

Pharmaceutical Society of Ireland.—Preliminary Examination *pro tanto.*

Scotch Education Department.—Leaving Certificates in each Grade and in Honours.

---

[1] This University was merged into the Royal University of Ireland in 1881, but Certificates previously obtained are still accepted for registration.

(4.) INDIAN, COLONIAL, AND FOREIGN UNIVERSITIES AND COLLEGES.

Universities of Calcutta, Madras, and Bombay.—Entrance Examination, Certificate to include Latin.

M'Gill College, and Bishop's College, Montreal; Toronto.—Matriculation Examination.

Trinity College, Toronto; Queen's College, Kingston; Victoria College, Upper Canada.—Matriculation Examination.

Western University of Ontario.—Entrance Examination in Medicine, Certificate to include all the subjects required for registration.

College of Physicians and Surgeons of Ontario.—Preliminary Examination, Certificate to include all the subjects required for registration.

University of Manitoba.—Previous Examination.

King's College, Nova Scotia.—Matriculation Examination; Responsions.

University of Halifax, Nova Scotia.—Matriculation Examination.

University of Fredericton, New Brunswick.—Matriculation Examination.

Mount Allison College, New Brunswick.—Examination for a Degree in Arts.

Dalhousie College and University, Halifax, Nova Scotia.—Matriculation and Sessional Examinations.

Provincial Medical Board of Nova Scotia.—Matriculation Examination.

University of California.—Examination in Department of Letters.

University of Melbourne.—Matriculation Examination.

University of Sydney.—Matriculation Examination.

University of the Cape of Good Hope.—Matriculation Examination; Examination for a Degree in Arts.

University of Adelaide.—Matriculation Examination or its substitute, the Senior Public Examination, Junior Public Examination, Certificate to include all the subjects required for registration.

Codrington College, Barbados.—English Certificate for Students of two years' standing, and Latin Certificate, or "Testamur."

Tasmanian Council of Education.—Examination for the Degree of Associate of Arts, Certificate to include Latin and Mathematics.

Christ's College, Canterbury, New Zealand.—Voluntary Examinations, Certificate to include all the subjects required by the General Medical Council.

University of New Zealand.—Entrance Examination.

University of Otago, New Zealand.—Preliminary Examination.

Ceylon Medical College.—Preliminary Examination (Primary Class).

Germany and other Continental Countries.—Gymnasial, Abiturienten-Examen, and other Corresponding Entrance Examinations to the Universities.

Gymnasia of the Circuit of Dorpat.—Examinations of Maturity.

The following pages contain a tabular abstract of the regulations of the various Licensing Boards, as well as a list of the Hospitals, Dispensaries, etc., attached to our Scotch Medical Schools; also the regulations for the Army, Indian, and Navy Medical Services. The space at our disposal does not allow of more detailed information. But, in point of fact, the regulations of all Licensing Boards now correspond much more closely than they used to do, and the regulations of the General Medical Council afford a key to the general requirements of all of them. For special information, application should always be made to the Secretaries of the Licensing Boards; or, in the case of the Universities, recourse may be had to the published Calendars.

The *Preliminary Examinations* are usually held before the commencement, and at the end, of the Winter Session,—viz., in October, March, or April—sometimes also at the end of the Summer Session. The Professional Examinations in Universities are usually after the Winter and during the Summer Sessions; in Edinburgh, however, the first Professional is held in October as well as April. The other Licensing Boards' examinations are held at various periods throughout the year. Special examinations, under circumstances of urgency, can be held at almost any time; but they, of course, entail considerable addition to the expense.

COURSE OF STUDY REQUIRED BY THE VARIOUS BOARDS OF THE UNITED KINGDOM.

	Age.	Anatomy.	Dissections.	Chemistry.	Practical Chemistry.	Materia Medica.	Physiology or Institutes of Medicine.	Surgery.	Practice of Medicine.	Midwifery.	Medical Jurisprudence.	Pathology or Morbid Anatomy.	Botany.	Natural History.	Practical Pharmacy.	Clinical Surgery.	Clinical Medicine.	Hospital Attendance.	Practical Midwifery.	Dispensary or Out-door Practice.	Vaccination.
	Years.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	Mons.	
Edinburgh University, M.B. & C.M., . .	21	6	6	6	3	6	6	6	6	6	3	6	3	3	3	6	6	24	3 mo.,[‡] or 6 cas.	6	
University of Glasgow, M.B. & C.M., . .	21	6	6	6	3	6	6	6	6	6	3	6	3	3	3	6	6	24	do.	6	
University of Aberdeen, M.B. & C.M., .	21	6	6	6	3	6	6	6	6	6	3		3	3	3	6	6	24	do.	6	
University of St Andrews, M.B. & C.M., .	21	6	6	6	3	6	6	6	6	6	3	6	3	3	3	6	6	24	do.	6	
London University, M.B., ...	21	6	12	6	1 crs.	1 crs.	6	6	6	1 crs.	1 crs.	1 crs.			1 crs.	2 yrs.	2 yrs.	4 yrs.	20 cas.	6	
University of Durham, M.B. & M.D., . .	21												3								
Dublin University, M.B., ...		6	6	6	3	3	3	6	6	6	3		3				9	24	6 mo.		
" " Surgical Diploma, .		12	18	6	3	6	18	12	6	6	3		3			27	27	27			
The Queen's University of Ireland, M.D.,		12	12	6	3	6	12	6	6	6	3		3	3	3 mos.	12	12	24	3 mo.		
Royal College of Physicians, London, .	21	12	12	6	3	3	12	6	12	3	3	6	3		3	9	9	24	20 cas.		
Royal College of Physicians, Edinburgh,	21	6	6	6	3	3	6	6	6	3	3	3	3		3	3	6	24	6 cases		
King and Queen's Col. of Phys. Ireland, .	21	6	12	6	6	6	6	6	6	6	6		6			6	21	27	6 mo.		
Royal College of Surgeons, London,[*] . .	21	12	12	6		3	12[*]	12	6	3					3	27[*]	9[*]	33			Cert.
Royal College of Surgeons, Dublin, . .	21	18	18	6	6	3	18	18	6	3	3					18	9	27			
Royal College of Surgeons, Edinburgh, .	21	12[†]	12[†]	6	3	3	3	6[†]	6	3	3	3			3	6[†]	6	24	6 cas.	6	Cert.
Faculty of Phys. and Surgeons, Glasgow,	21	12	12	6	3	3	3	6 or 12	6	3	3				3	6 or 12	6	24	6 cas.		
For Triple Qualification by Royal Colleges of Phys. and Surgs. of Edinburgh, and Faculty of Phys. and Surgs. of Glasgow, ...	21	6	12	6	3	3	6	6	6	3	3	3			3	9	9	24	6 cas.	6	Cert.
Apothecaries' Hall, England, ...	21	12	6	6	3	3	6		12	3	3	6	3		Appr.		27	27	20 cas.		9 mo.
" " Ireland, ...	21	6	12	6	3	3	6	6	6	6	3		3		Appr.	9	18	27	20 cas.		

English Poor-Law Board.—Candidates for the appointment of Medical Officer are required to be registered under the Medical Act, and must be legally qualified to practise both Medicine and Surgery, in virtue of Diplomas or Licenses granted by competent legal authority in England, Scotland, or Ireland.

Information respecting exceptions to these regulations under various circumstances, and other details as to the order in which, according to some Licensing Bodies, the courses should be taken out, etc., must be obtained by consulting the published Charts of the Colleges, etc. Students should apply to the Secretary to each Board which they intend to pass for a detailed copy of its Regulations.

---

[*] Students from the Schools of Scotland are admitted to examination at the Royal College of Surgeons of England, if they have followed the course of study required by the regulations of the Royal College of Surgeons of Edinburgh. Students in Scotland, therefore, are not required to attend more than one course of Physiology, six months Clinical Surgery, six months Clinical Medicine, and twenty-four months Hospital.

[†] Or Candidates may take six months Lectures and eighteen months Dissections. A third Course of Surgery is required, but it may be either Principles and Practice of Surgery or Clinical Surgery.

[‡] In lieu of the above, the University of Edinburgh requires, as an alternative, attendance on at least Twelve Cases of Labour under the superintendence of a registered Medical Practitioner.

MEDICAL SCHOOLS OF SCOTLAND, 1889–90.

WINTER SESSION.

SUBJECTS.	UNIVERSITY OF EDINBURGH.	SCHOOL OF MEDICINE, EDINBURGH.	UNIVERSITY OF GLASGOW.	ANDERSON'S COLLEGE MEDICAL SCHOOL, GLASGOW.	ST MUNGO'S COLLEGE, GLASGOW.	WESTERN MEDICAL SCHOOL, GLASGOW.	UNIVERSITY OF ABERDEEN.	UNIVERSITY OF ST ANDREWS.
Anatomy, Systematic and Practical, with Demonstrations.	Prof. Sir W. Turner.	Dr J. Symington and Mr Macdonald Brown.†	Professor Cleland.	Dr A. M. Buchanan.	Prof. Clark.	...	...	...
Physiology, or Institutes of Medicine.*	Professor Rutherford.	Mr James Hunter & Dr N. Paton.†	Professor M'Kendrick.	Dr James Christie.	Prof. John Barlow.	...	Professor M'William.	Prof. Pettigrew.
Chemistry, and Practical Chemistry.	Professor C. Brown.	Dr Macadam, Mr King, Mr I. Macadam, Dr Drinkwater, Mr. R. Paterson, & Dr Aitken.	Professor J. Ferguson.	Professor J. Robertson Watson.	...	...	Professor Carnelley.	Prof. Purdie.
Materia Medica & Therapeutics.	Prof. T. R. Fraser.	Drs Craig & Stockman.†	Professor Charteris.	Dr Napier.	Prof. John Dougall.	...	Professor Cash.	...
Practice of Medicine.	Professor Grainger Stewart.	Drs Wyllie, Affleck, Bramwell, & G. A. Gibson†	Professor Gairdner.	Dr S. Gemmell.	Professor Robertson.	...	Prof. Smith-Shand.	...
Surgery.	Professor Chiene.	Dr MacGillivray, Mr Cathcart, Mr Caird, Mr Hodsdon, & Mr S. Lang.	Professor Sir G. H. B. Macleod.	Dr Dunlop.	Prof. Knox.	...	Professor A. Ogston.	...
Midwifery, etc.	Professor Simpson.	Drs Milne Murray and Matheson.	Professor Leishman.	W. L. Reid.	In Summer.	...	Professor Stephenson.	...
Natural Philosophy.	Professor Tait.	...	Prof. Sir W. Thomson.	Prof. Blyth.	...	...	...	Prof. Butler.
Natural History, and Practical Natural History.	Professor Cossar Ewart.	Mr J. Arthur Thomson.	Professor Young.	...	...	...	Professor A. Nicholson.	Prof. M'Intosh.
Practical Botany, Advanced and Elementary	Professor Bayley Balfour.	...	...	...	...	...	...	...
General and Practical Pathology.	Professor Greenfield.	Dr A. Bruce and Dr W. Russell.	...	At Royal or Western Infirmary.	Pathologist of Royal Infirmary.	At Western Infirmary.	Professor Hamilton.	...
Clinical Medicine.	Professors G. Stewart, T. R. Fraser, Greenfield, and Simpson on Diseases of Women.	Drs Muirhead, Brakenridge, Wyllie, Affleck, and H. Croom for Diseases of Women.‡	Prof. M'Call Anderson and Prof. Gairdner.	Physicians of the Royal or Western Infirmary.	Prof. M'Vail and Physicians of the Royal Infirmary.	Physicians of the Western Infirmary.	Drs Smith-Shand, Fraser, and Blaikie Smith.	...
Clinical Surgery.	Professor Annandale.	Mr Duncan and Mr Miller.†	Prof. George Buchanan and Prof. Macleod.	Surgeons of the Royal or Western Infirmary.	Prof. Macewen and Surgeons of the Royal Infirmary.	Surgeons of the Western Infirmary.	Drs Ogston, O. Will, and Garden.	...
Medical Jurisprudence and Public Health.	Prof. Sir Douglas Maclagan.	Dr Littlejohn.	...	Public Health—Dr Christie	Public Health, Prof. Glaister	...	...	...

In connexion with the University of Edinburgh, during the Winter Session classes of Practical Materia Medica (including Pharmacy), Practical Pathology, and Practical Hygiene are taught by the Professors of these subjects and their Assistants; lectures and clinical instruction on Diseases of Children, at Royal Hospital for Sick Children are given by Dr James Carmichael; on Comparative Embryology by Mr George Brook; and on Philosophy o Natural History by Mr G. J. Romanes, M.A., LL.D.

Lectures are given in the School of Medicine, Edinburgh, during the Winter Session on the Diseases of the E by Dr Kirk Duncanson; on Diseases of the Ear and Throat by Dr P. M'Bride; on Diseases of the Eye by Dr John Robertson and Mr George Berry; on Diseases of the Skin by Dr Allan Jamieson; on Diseases of Children by Mr J Bell and Dr Playfair; on Vaccination by Drs Husband and Buist; on Diseases of the Tropics and Climatology by Dr Felkin; on Systematic Gynæcology by Dr Halliday Croom; and on Practical Midwifery and Clinical Gynæcology by Drs P. Young and Brewis.

There are Special Classes for Women on Chemistry, Anatomy, Clinical Medicine, Clinical Surgery, Institutes o Medicine, and Practice of Medicine. Practical Physiology is taught by the respective Professors during the Winter and Summer Sessions in Edinburgh, and in Summer at Aberdeen. Practical Surgery is taught during the Winter Session in the University of Aberdeen by Professor Ogston and Assistant.

* This course is equivalent to that given under the name of General Anatomy and Physiology in the English Schools. Special schedules are issued by the London Boards for their Scotch students which should always be inquired for.

† These are not conjoint courses, but separate ones by the gentlemen named. ‡ This is a joint course.

MEDICAL SCHOOLS OF SCOTLAND, 1890.

SUMMER SESSION.

SUBJECTS.	UNIVERSITY OF EDINBURGH.	SCHOOL OF MEDICINE, EDINBURGH.	UNIVERSITY OF GLASGOW.	ANDERSON'S COLLEGE MEDICAL SCHOOL, GLASGOW.	ST MUNGO'S COLLEGE, GLASGOW.	WESTERN MEDICAL SCHOOL, GLASGOW.	UNIVERSITY OF ABERDEEN.
Practical Anatomy and Demonstrations.	Prof. Sir Wm. Turner.	Dr J. Symington and Mr M. Brown.*	Professor Cleland.	Dr A. M. Buchanan.	Professor Clark.	...	...
Botany.	Professor Bayley Balfour.	Mr A. N. M'Alpine.	Professor F. O. Bower.	...	...	...	Professor Trail.
Materia Medica and Therapeutics and Practical Materia Medica.	Prof. T. R. Fraser.	Dr Craig and Dr Stockman.*	Professor Charteris.	Dr Napier.	In Winter.	...	Professor Cash.
Midwifery, etc.	Professor Simpson.	Drs Croom, B. Hart, and F. Barbour.*	Professor Leishman.	Dr W. L. Reid.	Prof. James Stirton.	...	Prof. W. Stephenson.
Practical Hygiene, Medical Jurisprudence and Public Health.	Professor Sir Douglas Maclagan.	Dr Littlejohn.†	Professor Simpson.	Dr Eben. Duncan.	Prof. Glaister. P. H. in Winter.	...	Professor M. Hay.
Comparative Anatomy.	Prof. Sir Wm. Turner.	...	Professor Young.	...	Prof. Clark and Demonstrator.	...	...
Practical Physiology, incl. Histology.	Professor Rutherford.	Mr J. Hunter and Dr Noël Paton.	Professor M'Kendrick.	Dr James Christie.	Prof. Barlow.	...	Professor M'William.
Practical Pathology, incl. Histology.	Professor Greenfield.	Dr A. Bruce and Dr W. Russell.	Dr Joseph Coats.	At Royal or Western Infirmary.	Pathologist of Royal Infirmary.	At Western Infirmary.	Professor Hamilton.
Organic and Practical Chemistry.	Professor Crum Brown.	Dr Macadam, Mr King, Mr I. Macadam, Dr Drinkwater, Mr R. Paterson, and Dr A. P. Aitken.*	Professor J. Ferguson.	Professor J. Robertson Watson.	...	...	Professor Carnelley.
Operative Surgery and Surgical Anatomy.	Professor Chiene.	Dr C. W. MacGillivray, Mr C. W. Cathcart, Mr Caird, Mr Hodsdon, and Dr Scott Lang*	Professor Sir G. H. B. Macleod.	Dr Dunlop.	Dr Whitson.	...	Professor A. Ogston.
Mental Diseases.	Dr Clouston.	Dr J. B. Tuke.	Dr Yellowlees.	...	Dr A. C. Clark.	...	Dr Reid.
Natural History.	Professor Cossar Ewart.	...	Professor Young.	...	...	...	Professor Nicholson.
Clinical Medicine.	Professors G. Stewart, T. R. Fraser, Greenfield, and Simpson on Diseases of Women.	Drs Muirhead, Brakenridge, Wyllie, Affleck, and H. Croom for Diseases of Women.‡	Prof. M'Call Anderson and Prof. Gairdner.	Physicians of the Royal or Western Infirmary.	Prof. M'Vail and Physicians of the Royal Infirmary.	Physicians of Western Infirmary.	...
Clinical Surgery.	Professor Annandale.	Mr Duncan and Mr Miller.	Prof. George Buchanan and Prof. Macleod.	Surgeons of the Royal or Western Infirmary.	Professor Macewen & Surgeons of the Royal Infirmary.	Surgeons of West. Infirmary.	...

In connexion with the University of Edinburgh during the Summer Session, classes of Practical Botany, Practical Natural History, and Practical Hygiene, are taught by the Professors of these subjects and their Assistants; lectures and clinical instruction on Diseases of Children, at Royal Hospital for Sick Children, are given by Dr J. Carmichael: on Diseases of the Eye by Dr D. Argyll Robertson; and on Comparative Embryology by Mr George Brook.

* These are not conjoint courses, but separate ones by the gentlemen named.
† Dr Littlejohn gives courses during both the Winter and Summer Sessions.
‡ This is a joint course.
‖ In Winter.

⁂ For additional Summer Courses on special subjects, see the Prospectus of each School.

In connexion with the Summer Session of the Medical Schools in Scotland, instruction in Vaccination is given at the Royal Public Dispensary, Edinburgh, on Wednesdays and Saturdays at 12 and 1, both Summer and Winter, by Dr Husband; and in the Western Dispensary, on Thursday, at 3 o'clock, by Dr Buist; at the Faculty Hall, Glasgow, on Mondays at 2, by Dr Thomson; and at the Royal Infirmary, Glasgow, on Mondays and Thursdays at 12 o'clock, by Dr Thomson, and by Dr M'Vail, at Western Infirmary on Mondays at 12 o'clock. Medical Psychology and Insanity are taught in Summer by Dr Clouston and Dr J. B. Tuke in Edinburgh, in Glasgow by Dr Yellowlees and Dr A. C. Clark, and in Aberdeen by Dr Reid. Practical Medicine and Medical Diagnosis by Dr Gibson and Dr Byrom Bramwell in Edinburgh. Diseases of Children by Mr J. Bell and Dr Playfair in School of Medicine, Edinburgh; in St Mungo's College by Drs Black and Adams, Glasgow; by Prof. Stephenson at Children's Hospital, Aberdeen. Diseases of the Eye by Dr John Robertson and Mr George Berry in School of Medicine, Edinburgh, Dr M'Kenzie Davidson in Aberdeen, and by Dr Thomas Reid, Dr Fergus, and Prof. Wolfe in Glasgow. Diseases of the Ear by Dr Kirk Duncanson in School of Medicine, Edinburgh; and of the Ear by Dr Johnston Macfie and Dr Barr in Glasgow; of the Ear, Nose, and Throat by Dr Walker Downie in Glasgow; and in Aberdeen, Ear and Larynx Diseases by Dr M'Kenzie Booth. Diseases of the Throat and Nose by Dr Newman in Glasgow. Diseases of the Skin by Dr Allan Jamieson in School of Medicine, Edinburgh, and in Aberdeen by Dr Garden, and in Glasgow by Dr Provan. Dental Surgery by Dr J. C. Woodburn in Glasgow, and Dr Williamson in Aberdeen. Laryngology and Medical Ophthalmology in Edinburgh by Dr John Wyllie. Diseases of the Tropics and Climatology in Edinburgh by Dr R. W. Felkin. Lectures on Public Health are delivered during the Summer Session in Aberdeen by Professor Hay, in School of Medicine, Edinburgh, by Dr Littlejohn, and in Glasgow by Dr P. Caldwell Smith. Natural History in School of Medicine, Edinburgh, by Mr J. Arthur Thomson. Systematic Gynæcology in School of Medicine, Edinburgh, by Dr Brewis. Clinical Gynæcology and Practical Midwifery in School of Medicine, Edinburgh, by Drs Croom, Young, and Brewis.

Special Courses for Women in School of Medicine, Edinburgh, on Practical Chemistry, Practical Pathology, Materia Medica, etc.

LIST OF HOSPITALS, DISPENSARIES, ETC., IN CONNEXION WITH THE MEDICAL SCHOOLS OF SCOTLAND.

EDINBURGH.

Royal Infirmary, including Lock Hospital. 670 Beds and 30 Cots. Visits daily from 12 till 2 P.M. Consulting Physicians, Dr George W. Balfour and Professor Sir Douglas Maclagan; Dr Alexander Keiller (for Diseases of Women). Physicians, Professors Grainger Stewart, Fraser, and Greenfield; Drs Claud Muirhead, D. J. Brakenridge, John Wyllie, and J. O. Affleck; Professor Simpson and Dr J. Halliday Croom (for Diseases of Women). Assistant Physicians, Drs Andrew Smart, Alex. James, and Byrom Bramwell; Drs D. Berry Hart and A. H. F. Barbour (for Diseases of Women). Extra Physician, Dr Allan Jamieson (for Diseases of the Skin). Consulting Surgeons, Dr Gillespie, Dr P. H. Watson, and Mr Joseph Bell. Surgeons, Professors Annandale and Chiene, Mr John Duncan, Mr A. G. Miller, and Dr P. H. Maclaren. Assistant Surgeons, Drs Chas. W. MacGillivray, J. M. Cotterill, Chas. W. Cathcart, Francis M. Caird, and J. W. B. Hodsdon; Ophthalmic Surgeons, Dr Argyll Robertson and Mr George A. Berry. Surgeon to the Ear and Throat Department, Dr P. M'Bride. Assistant Ophthalmic Surgeon, Dr George Mackay. Dental Department, Surgeons of the Edinburgh Dental Hospital and School. Pathologists, Alex. Bruce and William Russell.

CONVALESCENT HOUSE, Corstorphine. Surgeon, Mr Cotterill.

CHALMERS HOSPITAL FOR THE SICK AND HURT (Public and Private Wards). 40 Beds for medical and surgical patients. In Private Wards the patients are charged 5s. per diem for board. Physician, Dr George W. Balfour. Surgeon, Dr P. H. Watson.

ROYAL MATERNITY AND SIMPSON MEMORIAL HOSPITAL. 30 Beds; over 300 in-patients and about 700 out-patients yearly. Consulting Physicians, Drs Moir and Graham Weir. Consulting Surgeon, Professor Annandale. Physicians, Dr Keiller, Professor Simpson, Drs Halliday Croom and Underhill. Assistant Physicians, Drs Berry Hart and Freeland Barbour. Secretary and Treasurer, Andrew Scott, C.A.

ROYAL HOSPITAL FOR SICK CHILDREN. 80 Beds; number of patients for 1888, 7215. Consulting Physicians, Dr Graham Weir, Dr Geo. W. Balfour, Dr R. Peel Ritchie, and Professor T. Grainger Stewart. Consulting Surgeon, Professor Annandale. Surgeon, Mr Joseph Bell. Pathologist, Dr G. S. Woodhead. Ordinary Physicians, Dr James Carmichael, Dr John Playfair, and Dr Burn Murdoch. Extra Physicians, Dr R. Mackenzie Johnston, Dr G. H. Melville Dunlop, and Dr John Thomson. Surgeon-Dentist, Dr John Smith. Ophthalmic Surgeon, Dr Argyll Robertson. Resident Physicians, Drs Francis D. Boyd and A. Lockhart Gillespie.

ROYAL PUBLIC DISPENSARY AND VACCINE INSTITUTION. About 12,000 patients annually. Consulting Physician, Dr George Balfour. Consulting Surgeon, Dr Littlejohn. Medical Officers, Drs W. Husband, D. Wilson, Andrew, P. Young, A. Black, Dunlop, Blaikie, Webster, Wallace, Haultain. Consulting Physicians-Accoucheur, Drs Keiller and Charles Bell. Midwifery and Diseases of Women, Drs Andrew and Young. Vaccination, Dr W. Husband. Apothecary, Mr W. Duncan. Clinique daily at 3 P.M. Vaccination, Wednesday and Saturday, 12 noon. Midwifery and Diseases of Women, Tuesday and Friday, 1 P.M. Laboratory, 3 P.M. and 7 P.M. Secretary, R. C. Gray, S.S.C. James Andrew, M.D., Secretary to the Medical Officers.

NEW TOWN DISPENSARY, 17 Thistle Street. About 10,000 patients annually. Medical Officers, Drs Gibson, Scott Lang, Thyne, Ferguson, Cumming, and K. M. Douglas. Acting Physicians-Accoucheur, Drs Brewis and Wood. Superintendent of Vaccination, Dr Cadell. Diseases of the Ear and Throat, Dr Johnston. Diseases of the Eye, Dr Mackay. Diseases of Women, Dr Brewis. Diseases of Children, Dr Thomson. Mental Diseases, Dr John Tuke. Clinique daily at 3 P.M. Vaccination on Tuesdays and Fridays at 12.

THE EDINBURGH PROVIDENT DISPENSARY, Marshall Street. Upwards of 8500 patients annually. Consulting Physician, Dr Andrew Smart. Consulting Surgeon, Dr Joseph Bell. Dental Surgeon, R. Urquhart. Medical Officers, Dr Charles Bell, Dr F. Troup, Dr A. Matheson, Dr Robertson, Dr E. Price, Dr G. Price, Dr G. Bell, Dr Booth, and Dr Stalkart. Assistant Medical Officer, Dr W. Keiller. Secretary, R. Urquhart. Vaccination (Tuesday and Friday at 1 o'clock), Dr C. Bell and Dr Matheson. Diseases of Women and Children, Dr C. Bell and Dr Matheson (Tuesday and Friday at 1 o'clock). Out-door practice and Practical Midwifery daily. Surgical Instrument Maker, A. Young, Forrest Road. Janitor, John Cunningham.

WESTERN DISPENSARY, Chalmers Institute, Ponton Street, Fountainbridge. Chairman, James Simson. Acting Committee, Miss Chalmers, Miss M. Douglas, Mrs Owen, S. Henderson, Stair Kerr, Dr Bruce Bremner, A. Dickison, Dr Pringle, A. M'Lellan. Consulting Physician, Dr A. Peddie. Consulting Surgeon, Dr Joseph Bell. Consulting Physician for Diseases of Women, Dr Croom. Physicians, Drs Murdoch Brown, Bruce, and D. Noël Paton. Assistant Physicians, Drs Home Ross and Harvey Littlejohn. Surgeon, Dr Shaw Maclaren. Diseases of the Throat, Dr Hunter Mackenzie. Diseases of Women, Dr Milne Murray. Vaccination Officer, Dr J. B. Buist. Midwifery, Dr Barbour. Diseases of the Eye, Dr Sym. Dispenser, Mr A. Wood. Secretary to Medical Board, Dr J. Murdoch Brown. Treasurer, Mrs Macnider, 31 Stafford Street. Open daily from 3.45 to 4.45 P.M. Diseases of Women, Monday, 4.45 P.M. Diseases of Throat and Ear, Monday, 4 P.M. Vaccination, Thursday, 3 P.M. Special Surgical Cases, Wednesday, 4 P.M. Diseases of the Eye, Thursday, 4 P.M.

ROYAL ASYLUM FOR THE INSANE. About 850 patients, over 300 of whom are private patients. Physician, Dr Clouston.

EYE, EAR, AND THROAT INFIRMARY, 6 Cambridge Street, Lothian Road, Edinburgh. Consulting Surgeon, Joseph Bell, *P*.R.C.S.E. Surgeons, J. J. Kirk Duncanson, M.D., F.R.C.P.E., G. Hunter Mackenzie, M.D., J. Maxwell Ross, M.A., F.R.C.S., and William George Sym, M.D. Eye Clinique daily at 1 P.M. Ear Clinique on Mondays, Thursdays, and Saturdays, at 12 noon. Throat Clinique on Tuesdays and Fridays at 4 P.M.

EYE DISPENSARY OF EDINBURGH, 31 Chambers Street. About 1500 patients annually. Surgeons, Mr Berry and Mr Caverhill. Assistant Surgeon, Dr Mackay. Open Mondays, Wednesdays, and Fridays, at 1 P.M. Clinical Instruction is given in the Diagnosis and Treatment of Diseases of the Eye (including minor operations) by the Surgeons in charge.

DENTAL HOSPITAL AND SCHOOL, 5 Lauriston Lane. Consulting Physician, Alexander Peddie, M.D., F.R.C.P.E. Consulting Surgeon, Joseph Bell, *P*.R.C.S.E. Consulting Dental Surgeon, John Smith, M.D., LL.D., F.R.C.S.E. Lecturers, Messrs Andrew Wilson, W. Bowman Macleod, and G. W. Watson. Dental Surgeons, W. Bowman Macleod, A. Wilson, M. Macgregor, G. W. Watson, J. S. Durward, James Mackintosh, and James Lindsay. Assistant Dental Surgeons, W. Forrester, John S. Amoore, J. G. Munro, W. Wilson, John Turner, and David Monroe. Tutorial Dental Surgeon, H. B. Ezard. Extra Assistants, F. Page and T. Gregory. Chloroformists, Robert Stewart, M.B., C.M., and William Keiller, L.R.C.P. & S.E. Dean and Hon. Treasurer, W. Bowman Macleod. Hon. Secretary, Lindsay Mackersy, W.S., 3A North St David Street. Daily, 9 to 10 A.M. Average number of patients, 8000 per annum.

GLASGOW.

ROYAL INFIRMARY. 582 Beds. Visit daily at 9 A.M. Dispensary Hour, 2 P.M. Physicians, Dr Wood Smith, Prof. Alex. Robertson, and Drs J. W. Anderson and Samson Gemmell. Extra Physician, Prof. M'Vail. Surgeons, Profs. Macewen, Clark, and Knox, and Drs Dunlop, Lothian, and Wm. J. Fleming. Gynæcologist, Dr Kelly. Aural Surgeon, Dr Macfie. Dental Surgeon, Dr J. C. Woodburn. Assistant Physicians, Prof. Dougall, and Drs Middleton, Henderson, Black, Macphee, and J. L. Steven. Assistant Surgeons, Prof. Barlow, and Drs Adams, Muir, Shaw, Whitson, and Macintyre. Diseases of the Skin, Dr Provan. Diseases of the Throat, Dr Newman. Diseases of the Eye, Dr Freeland Fergus and Prof. Wolfe. Pathologist, *vacant*. Vaccinator, Hugh Thomson, M.D. Electrician, Dr Macintyre. M. Thomas, M.D., Superintendent.

WESTERN INFIRMARY. This Hospital contains about 400 Beds for Medical and Surgical patients. There are Wards for Skin Diseases and Diseases of Women, and an out-door Midwifery department. Physicians, W. T. Gairdner, M.D., T. M'Call Anderson, M.D., James Finlayson, M.D., Gavin P. Tennent, M.D. Diseases of Women, W. Leishman, M.D. Surgeons, Sir George H. B. Macleod, M.D., George Buchanan, M.D., A. Patterson, M.D., Hector C. Cameron, M.D. Assistant-Physicians, Joseph Coats, M.D., Jas. Christie, M.D., and David C. M'Vail, M.D. Dispensary Physicians, John Alexander, M.D., Wm. G. Dun, M.D., R. Stevenson Thomson, M.B., George Dickson, M.D., C. O. Hawthorne, M.D. Dispensary Physicians for Diseases of Women, W. L. Reid, M.D., Robert Kirk, M.D., Murdoch Cameron, M.D. Dispensary Surgeons, J. C. Renton, M.B., Geo. T. Beatson, M.D., David Newman, M.D. Extra-Dispensary Surgeons, A. E. Maylard, M.S., Jas. Parker, M.D. Pathologist, Joseph Coats, M.D. Dispensary Surgeon for Diseases of the Ear, Thomas Barr, M.D. Dispensary Physician for Diseases of the Throat, J. Walker Downie, M.D. Hon. Consulting Ophthalmic Surgeon, Thomas Reid, M.D. Dental Surgeon, Wm. de Brassey Woodburn, L.D.S.F.P.S. Medical Superintendent, A. W. Russell, M.A., M.B., C.M. Lady Superintendent, Miss E. Clyde. Secretary, Henry Johnston, 125 Buchanan Street. The hour of visit is 9 A.M. Operating days, Wednesday and Saturday. The Dispensary for out-patients is open daily at 2 P.M.

LOCK HOSPITAL, 41 Rottenrow Street. 80 Beds. Medical Officers, Drs James Dunlop and A. Patterson. Superintendent, P. Condra. Treasurer, T. D. Findlay. Secretary, Robert Young.

MATERNITY HOSPITAL. 36 Beds; in-patients, 358; out-patients, 1711. Consulting Surgeon, Dr George Buchanan. Senior Consulting Physician, Professor Leishman. Consulting Physician, Dr Samuel Sloan. Obstetric Physicians, Drs W. L. Reid and Murdoch Cameron. Assistant Obstetric Physicians, Drs E. H. Lawrence Oliphant and John Ritchie. Pathologists, Drs David Newman and T. K. Dalziel. Out-door Physicians, Drs J. Dunlop, T. F. Gilmour, Malcolm Black, Alexander Jamieson, W. Wallace Anderson, William Cullen, and H. St Clair Gray.

GLASGOW ROYAL ASYLUM, GARTNAVEL. About 500 patients of all classes. Physician-Superintendent, Dr David Yellowlees. Lectures on Mental Diseases during Summer, at the University and the Asylum.

EYE INFIRMARY. 95 Beds (1154 house patients), 14,730 patients in all annually. Senior Surgeon, Thomas Reid, M.D. Surgeons, Thomas S. Meighan, M.D., Henry E. Clark, M.R.C.S., and J. Crawford Renton, M.D. Assistant Surgeons, D. N. Knox, M.A., M.B., A. Freeland Fergus, M.B., and A. Maitland Ramsay, M.B. House Surgeon, Andrew Wilson, M.B., C.M. Consulting Surgeon, George Buchanan, M.D. Secretary, William George Black.

DISPENSARY FOR SKIN DISEASES, 8 Elmbank Street. Physician, Professor M'Call Anderson. Open Mondays and Thursdays at 4 P.M., for out-door patients. This Institution is in connexion with the Wards for Skin Diseases in the Western Infirmary, to which the more important cases are sent.

OPHTHALMIC INSTITUTION, 126 West Regent Street. Consulting Physician, Dr S. J. Moore. Acting Surgeon, Dr J. R. Wolfe. Acting Physician, Dr J. S. Cumming. Assistant Surgeons, Dr J. M'Gregor Robertson and Dr A. T. Thomson. Secretary, Henry Johnston, 125 Buchanan Street. Open daily from 1 to 3 P.M. A Course of Lectures and Clinical Instruction during the Winter and Summer Sessions.

GLASGOW HOSPITAL AND DISPENSARY FOR DISEASES OF THE EAR, 28 Elmbank Crescent. 12 Beds. Hour of visit to the Hospital, 2 P.M. Out-patients seen at 2 P.M. on Mondays, Tuesdays, Wednesdays, Thursdays, Fridays, and Saturdays. Number of cases treated annually, 3600. During the Winter and Summer Sessions a Course of Practical Instruction in the Treatment of Ear Disease is given to the Students attending the Clinique. Senior Consulting Physician, Prof. W. T. Gairdner. Senior Consulting Surgeon, Dr James Morton. Pathologist, Dr Joseph Coats. Consulting Ophthalmic Surgeon, Dr Thomas Reid. Consulting Throat Surgeon, Dr Arthur Mechan. Consulting Dental Surgeon, Dr J. C. Woodburn. Physicians, Drs A. K. Irvine, A. L. Kelly, and J. Gardiner. Dental Surgeon, Mr Rees Price. Surgeon and Lecturer, Dr Thomas Barr. Secretary, Adam Sutherland, 75 West Nile Street.

ABERDEEN.

ROYAL INFIRMARY. Upwards of 200 Beds. Visits daily, Physicians at 11.30, and Surgeons at 12 o'clock. Physicians, Drs Smith-Shand, Fraser, P. Blaikie Smith, and Rodger. Surgeons, Drs A. Ogston, Will, Garden, and Hall. Ophthalmic Surgeon, Dr Davidson. Pathologist, Professor Hamilton.

Chloroformist, Dr Mackenzie Booth. Dental Surgeon, Dr Williamson. Honorary Superintendent, Miss Rachel Frances Lumsden. Treasurer, William Carnie.

General Dispensary, Lying-in and Vaccine Institution. 11,240 patients annually. Open daily at 10 o'clock A.M. except Sundays. Medical Attendants and Vaccinators, Drs Booth, Watt, Ruxton, Gordon, Edmond, and Gibson. Surgeon Dentist, W. P. Robertson. Secretary, John P. Cumine.

Children's Hospital. Physicians, Drs Stephenson and Garden. Junior Physicians, Drs Macgregor and Gordon. Clinical Instruction daily at 11 o'clock.

Lunatic Asylum. About 600 patients. Medical Superintendent, Dr Reid. Consulting Physician, Dr Jamieson. Treasurer, William Carnie.

Eye Institution, General Dispensary Buildings, Guestrow. Oculist, Dr M'Kenzie Davidson. Open three days in the week at 2.30 P.M. Clinical instruction on Diseases of the Eye and the use of the Ophthalmoscope. Average, 600 patients annually.

ARMY MEDICAL DEPARTMENT.

WAR OFFICE, LONDON.

SCHEDULE OF QUALIFICATIONS NECESSARY FOR CANDIDATES DESIROUS OF OBTAINING COMMISSIONS IN THE ARMY MEDICAL STAFF, WITH THE CONDITIONS OF SERVICE.

1. Every candidate for a commission in the Army Medical Staff must be 21 years and not over 28 years of age at the date of commencement of the competitive examination. He must produce an extract from the register of his birth, or, in default, a declaration, made before a magistrate by one of his parents or guardians, giving his exact age. He must also produce a recommendation from some person of standing in society—not a member of his own family—to the effect that he is of regular and steady habits, and likely in *every* respect to prove creditable to the Department if a commission be granted; and a certificate of moral character from the parochial clergyman, if possible.

2. The candidate must sign a declaration upon honour that both his parents are of unmixed European blood, and that he labours under no mental or constitutional disease, or hereditary tendency thereto, nor any imperfection or disability that can interfere with the efficient discharge of the duties of a medical officer in any climate: also that he does not hold, and has never held, any commission or appointment in the public services. His physical fitness will be determined by a Board of Medical Officers, who are required to certify that his vision is sufficiently good to enable him to perform any surgical operation without the aid of glasses. A moderate degree of myopia will not be considered a disqualification, provided it does not necessitate the use of glasses during the performance of operations, and that no organic disease of the eyes exists. The Board must also certify that he is free from organic or other disease, and from constitutional weakness, or other disability of any kind likely to unfit him for military service in any climate.

3. Certificates of age, registration of diplomas, etc., and of character, must accompany the declaration when signed and returned.

4. Candidates will be examined by the Examining Board in the following compulsory subjects, and the highest number of marks attainable will be distributed as follows:—*a*. Anatomy and Physiology, 1000; *b*. Surgery, 1000; *c*. Medicine, including Therapeutics and the Diseases of Women and Children, 1000; *d*. Chemistry and Pharmacy, and a practical knowledge of drugs, 1000. [*N.B.*—The examination in Medicine and Surgery will be in part practical, and will include operations on the dead body, the application of surgical apparatus,

and the examination of Medical and Surgical patients at the bedside. The examination in Chemistry will be limited to the elements of the science, and to its application to medicine, pharmacy, and practical hygiene.] No candidate shall be considered eligible for the Army Medical Staff who shall not have obtained at least *one-third* of the marks obtainable in each of the above *compulsory subjects.*

5. Voluntary subjects for examination. Candidates may be examined in the following voluntary subjects, for which the maximum number of marks obtainable will be—French and German (150 each), 300; Natural Sciences, 300. A number less than one-third of the marks obtainable in each of these voluntary subjects will not be allowed to count in favour of the candidate who has qualified in the compulsory subjects. The knowledge of Modern Languages being considered of great importance, all intending competitors are urged to qualify in French and German. The Natural Sciences will include Comparative Anatomy, Zoology, Natural Philosophy, Physical Geography, and Botany, with special reference to Materia Medica.

6. The appointments announced for competition will be filled up from the list of qualified candidates arranged in the order of merit, as finally determined by the total number of marks each has obtained in both the compulsory and voluntary subjects.

7. After passing this examination, every successful candidate will be required to attend one course of practical instruction at the Army Medical School as a Surgeon on probation on—(1) Hygiene; (2) Clinical and Military Medicine; (3) Clinical and Military Surgery; (4) Pathology of Diseases and Injuries incident to Military Service.

8. All Surgeons on probation will be required to conform to such rules of discipline as the Senate may from time to time enact, and provide themselves with uniform, viz., the regulation undress and mess uniform of a surgeon, but without sword.

9. They will be required to attend the Medical Staff Mess at Netley, and to conform to the Rules and Regulations thereof.

APPOINTMENT.

Every candidate for appointment to the Medical ranks of the Army Medical Staff must possess two diplomas or licenses, recognised by the General Medical Council, one to practise Medicine, and the other Surgery, and must be registered under the Medical Act in force in the United Kingdom at the time of his appointment.

A public and open competition will be held twice in the year for the admission of qualified candidates as probationers. The number of appointments so competed for will be not less than half of the number of vacancies which shall have arisen in the last completed half-year ending on the 30th June or 31st December. It will be competent for the Secretary of State for War to fill up the remaining number from such qualified candidates as may be proposed by the governing bodies of Public Schools of Medicine in the United Kingdom or in the Colonies as he may think proper. Every candidate so proposed must be certified by the governing body proposing him to be duly qualified according to a standard laid down by the Secretary of State, and shall be approved by the Director-General.

The Secretary of State for War will from time to time fix the order of precedence and the proportion in which the several Schools of Medicine shall be offered the nomination of candidates.

A Surgeon on probation, on being so nominated, will be sent to some large station for instruction in Ambulance and Medical Staff Corps duties, until the commencement of the next course of study at the Army Medical School, when he will go through such course as the Secretary of State shall decide. After passing a qualifying examination in the military medical subjects taught there, and satisfying the Director-General that he is a person of proper skill, knowledge, and character, for permanent appointment in the Army Medical Staff, the Surgeon on probation will be commissioned as Surgeon.

The Surgeons on probation who pass out of the Army Medical School at one qualifying examination will take precedence among each other as Surgeons, as follows:—(*a*.) Those appointed on nomination according to their date of joining on probation. (*b*.) Those appointed by competition according to the last day of the competitive examination, and in the order of merit, as determined by the combined results of the competitive and qualifying examinations, with priority over any joining under paragraph (*a*) on the last day of the competitive examination.

A Surgeon's commission will bear the date of the day of his passing out of the Army Medical School.

Rank and Pay.

The ranks and daily rates of pay of the officers of the Army Medical Staff are as follows:—Surgeon-General £2, 15s.; Deputy Surgeon-General, £2; Brigade Surgeon, £1, 10s., after five years in the rank, £1, 13s.; Surgeon-Major, £1, after fifteen years' service £1, 2s. 6d., after twenty years' service £1, 5s., after twenty-five years' service £1, 7s. 6d.; Surgeon, £200 a year, after five years' service £250, after ten years' service 15s. daily. Surgeon on probation 8s. Charge pay: the principal medical officer of an army in the field, consisting of 10,000 men and upwards, £1 daily; of 5000 men and upwards, 15s. daily; of less than 5000, 10s. daily. The principal medical officer of a command abroad where the number of commissioned officers and enlisted men is 1500 and upwards, 5s. daily.

The pay of medical officers is issued monthly in arrear.

The relative rank of officers of the Army Medical Staff is as follows:—I. A Director-General and Surgeon-General rank as Major-General. II. A Deputy Surgeon-General ranks as Colonel. III. A Brigade Surgeon ranks as Lieutenant-Colonel. IV. A Surgeon-Major ranks as Major; after twenty years' service as Surgeon-Major he ranks as Lieutenant-Colonel. V. A Surgeon ranks as Captain. VI. A Surgeon on probation ranks as Lieutenant.

ARMY MEDICAL SCHOOL.

ROYAL VICTORIA HOSPITAL, NETLEY.

President of the Senate.—W. A. Mackinnon, C.B., Director-General of the Medical Staff.

Members of the Senate.—Sir J. Fayrer, M.D., K.C.S.I., F.R.S., Physician to the Council of India; Surgeon-General Sir T. Longmore, C.B., Professor of Military Surgery; Dep. Surgeon-General Cayley, F.R.C.S., Indian Medical Service, Professor of Military Medicine; Sir William Aitken, M.D., F.R.S., Professor of Pathology; J. Lane Notter, M.D., Surgeon-Major, Professor of Military Hygiene; The Principal Medical Officer, Royal Victoria Hospital, Netley.

Assistant Professors.—Brigade-Surgeons C. H. Y. Godwin, J. F. Beattie, M.D.; Surgeons A. M. Davies and D. Bruce.

Candidates for Commissions in the Army, and in the Queen's Indian Service, proceed to Netley after passing the Examination at London. At Netley they attend the Medical and Surgical Practice of the Royal Victoria Hospital, and learn the system and arrangements of Military Hospitals. During four months they attend the lectures given by the Professors and Assistant-Professors, and go through a course of practical instruction in the Hygienic and Pathological Laboratories. The lectures and practical instruction are intended to explain the specialities of Military Medical Practice, attention being directed to gunshot and other wounds, surgical arrangements in the field during action and sieges, means of transport, field hospitals, tropical diseases and their means of investigation, service in India and in the various colonies, the sanitary arrangements in peace and war, and the means of carrying out the sanitary regulations. Every opportunity is taken of practising operations on the dead body, and practical points of a like kind.

INDIAN MEDICAL SERVICE.

REGULATIONS FOR THE EXAMINATION OF CANDIDATES FOR THE APPOINTMENT OF SURGEON IN HER MAJESTY'S INDIAN MEDICAL SERVICE.

(*Subject to any alteration that may be determined on*).

1. *Limits of Age.*—All natural-born subjects of Her Majesty, between 21 and 28 years of age at the date of the examination, and of sound bodily health, may be candidates. They may be married or unmarried. They must possess a Diploma in Surgery, or a license to practise it, as well as a Degree in Medicine, or a license to practise it in Great Britain or Ireland.

2. *Declaration to be submitted.*—They must subscribe and send in to the Military Secretary, India Office, Westminster, *so as to reach that address at least a fortnight before the date fixed for the Examination*, a declaration according to a prescribed form.

3. This declaration must be accompanied by the following documents :—

a. Proof of age, either by Registrar General's certificate, or, where such certificate is unattainable, by the candidate's own statutory declaration, form of which can be obtained at the India Office, supported, if required by the Secretary of State, by such evidence as he may consider satisfactory. A certificate of baptism which does not afford proof of age will be useless.

b. A recommendation from some person of standing in society—not a member of his own family—to the effect that he is of regular and steady habits, and likely in every respect to prove creditable to the service if admitted ; and a certificate of moral character from a magistrate, or a minister of the religious denomination to which the candidate belongs.

c. A certificate of registration, in accordance with the Medical Act of 1858, of the degrees, diplomas, and licenses possessed by the candidate.

4. *Physical Examination.*—The physical fitness of each candidate will be determined previous to examination by a Board of Medical Officers, who are required to certify that his vision is sufficiently good to enable him to perform any surgical operation without the aid of glasses. A moderate degree of myopia will not be considered a disqualification, provided it does not necessitate the use of glasses during the performance of operations, and that no organic disease of the eyes exists.

Every candidate must also be free from all other organic disease and from constitutional weakness, or other disability likely to unfit him for military service in India.

5. *Subjects for Examination.*—On producing the foregoing qualifications, the candidate will be examined by the Examining Board in the following compulsory subjects, and the highest number of marks attainable will be distributed as follows :—

	Marks.
a. Anatomy and Physiology, ...	1000
b. Surgery, ...	1000
c. Medicine, including Therapeutics, the Diseases of Women and Children, ...	1000
d. Chemistry and Pharmacy, and a practical knowledge of Drugs,	1000

(The examination in Medicine and Surgery will be in part practical, and will include operations on the dead body, the application of surgical apparatus, and the examination of medical and surgical patients at the bedside.)

The examination in Chemistry will be limited to the elements of the science, and to its application to medicine, pharmacy, and practical hygiene.

No candidate shall be considered eligible who shall not have obtained at least *one-third* of the marks obtainable in each of the above *compulsory subjects.*

6. *Voluntary Subjects.*—Candidates may be examined in the following voluntary subjects, for which the maximum number of marks obtainable will be :—

	Marks.
French, German, and Hindustani (150 each), ...	450
Natural Sciences, ...	300

The Natural Sciences will include Comparative Anatomy, Zoology, Natural Philosophy, Physical Geography, and Botany, with special reference to Materia Medica.

A number less than one-third of the marks obtainable in any of these *voluntary subjects* will not be allowed to count in favour of a candidate who has qualified in the *compulsory subjects.*

The knowledge of modern languages being considered of great importance, all intending competitors are urged to qualify in French and German.

7. The appointments announced for competition will be filled up from the list of qualified candidates arranged in order of merit, as finally determined by the total number of marks each has obtained in both the compulsory and voluntary subjects.

The Examiners in London will prepare a list in order of merit, with the marks affixed in the different subjects, to be transmitted to the Director-General and communicated to the Professors of the Army Medical School. If any candidate is found to be deficient in any particular subject, this shall be stated, in order that he may receive special instruction on the point at Netley.

8. *Course at Netley.*—After passing this examination the successful candidates will be required to attend one entire course of practical instruction at the Army Medical School, as Surgeons on probation, in—

(1.) Hygiene.
(2.) Clinical and Military Medicine.
(3.) Clinical and Military Surgery.
(4.) Pathology of Diseases and Injuries incident to Military Service.

These courses are to be of not less than four months' duration; but candidates who have already gone through a course at Netley as candidates for the Army or Navy Medical Service may, if thought desirable, be exempted from attending the School a second time.

9. *Pay and Uniform while at Netley.*—During the period of residence at the Army Medical School, each candidate will receive an allowance of 8s. per diem with quarters, or, when quarters are not provided, with the usual lodging and fuel and light allowances of a subaltern, to cover all costs of maintenance; and he will be required to provide himself with uniform (viz., the regulation undress uniform of a Surgeon of the British Service, but without the sword).

10. All candidates will be required to conform to such rules of discipline as the Senate may from time to time enact.

11. *Examination at Netley.*—At the conclusion of the course, candidates will be required to pass an examination on the subjects taught in the School. The examination will be conducted by the Professors of the School.

The Director-General, or any Medical Officer deputed by him, may be present and take part in the examination. If the candidate give satisfactory evidence of being qualified for the practical duties of an Army Medical Officer, he will be eligible for a Commission as Surgeon.

12. *Position on List of Surgeons, how determined.*—The position of the candidates on the list of Surgeons will be determined by the combined results of the preliminary and of the final examinations, and, so far as the requirements of the service will permit, they will have the choice of Presidency in India according to their position in that list.

MEMORANDUM REGARDING THE PAY, ETC., OF MEDICAL OFFICERS APPOINTED TO HER MAJESTY'S INDIAN FORCES.

Passage to India.—Surgeons on appointment will, if possible, be provided with passage to India by troop-ship; but when troop-ship accommodation is not available, passage at the public expense will be provided by steamer, or a passage allowance will be granted, if preferred. A charge for messing during the voyage will be made at the rate of 2s. a day. This payment will not include the cost of liquors, which will be charged for as extras.

Pay previous to Embarkation.—The rate of pay drawn by Surgeons from the date of passing the final examination at the Army Medical School until arrival in India will be 10s. a day.

Pay at the above rate will be issued in this country up to the date of embarkation, and an advance of two months' pay at the same rate will also be made prior to embarkation.

Pay and Allowances when in India.—Officers appointed to the Indian Medical Service will receive pay in India according to the following scale when not holding an appointment carrying higher pay:—

Rank.	Years' Service.	Pay and Allowances per Mensem.		
		R.	A.	P.
Brigade Surgeon and Surgeon-Major .	25	888	12	0
,, . .	20	852	3	7
,, . .	15	677	6	11
,, . .	12	640	14	6
Surgeon . . .	10	451	14	5*
,, . . .	6	433	10	2*
,, . . .	5	335	12	2*
,, . . .	under 5	317	8	0*

On first appointment they will only come into receipt of these allowances from the date of their arrival in India.

The monthly salaries of officers holding the principal administrative appointments and substantive military charges of the Indian Medical Service receive, at present, the following consolidated salaries:—Surgeon-General, Bengal, Rs. 2700; Madras, Rs. 2500; Bombay, Rs. 2500; Deputy Surgeon-General, from Rs. 2250 to Rs. 1800; Brigade Surgeon or Surgeon-Major, of twenty years' service and upwards, in substantive charge of a Native regiment, Rs. 1000, with Rs. 90 horse allowance in Cavalry regiments; Brigade Surgeon or Surgeon-Major in substantive charge of a Native regiment, Rs. 800, with Rs. 90 horse allowance in Cavalry regiments; Surgeon, above five years' full-pay service, in substantive charge of a Native regiment, Rs. 600, with Rs. 60 horse allowance in Cavalry regiments; Surgeon, under five years' full-pay service, in substantive charge of a Native regiment, Rs. 450, with Rs. 60 horse allowance in Cavalry regiments. The salaries of other substantive medical appointments in the Civil and Military Departments are consolidated, and vary from Rs. 1800 to Rs. 400 per mensem. Qualified officers of the Medical Service are also eligible for appointments in the Assay Department. The salaries of these appointments are from Rs. 600 to Rs. 2250 per mensem. Medical officers are required to perform two years' regimental duty in India before they can be considered eligible for civil employment.

Retiring Pensions and Half-Pay.—Officers of the Indian Medical Service will be allowed to retire on the following scale of pension, on completion of the required periods of service:—After thirty years' service for pension, £700; after twenty-five years' service for pension, £500; after twenty years' service for pension, £365; after seventeen years' service for pension, £292. A Surgeon-General, after five years' active employment in India in that grade, will be entitled to retire upon a pension of £350 per annum, in addition to that to which he may be entitled under the above scale. A Deputy Surgeon-General will, after five years' active employment in India in that grade, be entitled to retire upon a pension of £250 per annum, in addition to the pension to which he may be entitled under the above scale.

---

* When not receiving any staff allowance.

With a view to maintain the efficiency of the service, all medical officers of the rank of Brigade Surgeon and Surgeon-Major shall be placed on the Retired List when they shall have attained the age of fifty-five years, and all Surgeons-General and Deputy Surgeons-General when they shall have attained the age of sixty years. In any special case, where it would appear to be for the good of the service that the officer should continue in employment, he may be so continued, subject in each case to the sanction of the Secretary of State for India in Council.

The widows and families of medical officers are also entitled to pensions from the Indian Service Family Pension Fund, to which all Surgeons must, as a condition of their appointment, subscribe from the date of their arrival in India.

NAVY MEDICAL DEPARTMENT.

Admiralty, Northumberland Avenue, London, W.C.

REGULATIONS FOR ENTRY OF CANDIDATES FOR COMMISSIONS IN THE MEDICAL DEPARTMENT OF THE ROYAL NAVY.

1. Every candidate for admission into the Medical Department of the Royal Navy must be not under 21 nor over 28 years of age on the day of the commencement of the competitive examination. He must produce an extract from the register of the date of his birth; or, in default, a declaration made before a magistrate, from one of his parents or other near relative, stating the date of birth. He must also produce a certificate of moral character, and a recommendation signed by a clergyman or magistrate to whom he has been for some years personally known, or by the president or senior professor of the college at which he was educated.

2. He must be registered, under the Medical Act in force, as possessing two diplomas or licenses recognised by the General Council, one to practise medicine and the other surgery in Great Britain and Ireland.

3. He must sign a declaration that he is a British subject, the son of parents of unmixed European blood; that he labours under no mental or constitutional disease or weakness, or any other imperfection or disability which may interfere with the most efficient discharge of the duties of a medical officer in any climate; and that he does not hold, and has never held any commission or appointment in the public services.

He must also declare his readiness to engage for general service at home or abroad as required.

He must be free from organic or other disease, and his physical fitness will be determined by a board of medical officers, who are to certify that his vision comes up to the required standard, which will be ascertained by the use of Snellen's test types.

The certificates of registration, character, and birth must accompany the declaration, which is to be filled up and returned as soon as possible, addressed as above.

4. Candidates will be examined by the Examining Board in the following compulsory subjects, and the highest number of marks attainable will be distributed as follows:—Anatomy and Physiology (1000 marks); Surgery (1000 marks); Medicine, including Therapeutics and the Diseases of Women and Children (1000 marks); Chemistry and Pharmacy, and a practical knowledge of Drugs (1000 marks). (The examination in Medicine and Surgery will be in part practical, and will include operations on the dead body, the

application of Surgical Apparatus, and the examination of Medical and Surgical patients at the bedside. The examination in Chemistry will be limited to the elements of the Science, and to its application to Medicine, Pharmacy, and practical Hygiene).

No candidate shall be considered eligible who shall not have obtained at least *one-third* of the marks obtainable in each of the above *compulsory subjects.*

5. Candidates may be examined in the following voluntary subjects, for which the maximum number of marks obtainable will be :—French and German (150 each), 300 marks; Natural Sciences, 300 marks. A number less than one-third of the marks obtainable in each of these *voluntary subjects* will not be allowed to count in favour of the candidate who has qualified in the *compulsory subjects.*

The knowledge of modern languages being considered of great importance, all intending competitors are urged to qualify in French and German.

The Natural Sciences will include Comparative Anatomy, Zoology, Natural Philosophy, Physical Geography, and Botany, with special reference to Materia Medica.

6. The appointments announced for competition will be filled up from the list of qualified candidates arranged in the order of merit as finally determined by the total number of marks each has obtained in both the compulsory and voluntary subjects.

7. Successful candidates, immediately after passing the examination, will receive Commissions as Surgeons in the Royal Navy, and will undergo a course of practical instruction in Naval Hygiene, etc., at Haslar Hospital.

The full and half-pay per day of Naval Medical Officers is in accordance with the following scale :—FULL-PAY: Surgeon—On entry, 11s. 6d.; after 4 years' full-pay service, 13s. 6d.; after 8 ditto, 15s. 6d. Staff Surgeon—On promotion, £1, 1s.; after 4 years' full-pay service in rank, £1, 4s. Fleet Surgeon—On promotion, £1, 7s.; after 4 years' full-pay service in rank, £1, 10s.; after 8 ditto, £1, 13s. Deputy Inspector General, £2, 2s. Inspector General, £2, 15s. On first appointment to the service, medical officers will receive full pay, from date of appointment, but subject to the provisions of Art. 1255 clause 2, of the Queen's Regulations, 1879. HALF-PAY: Surgeon—Under 2 years' full-pay service, 6s.; after 2 years' full-pay service, 7s.; after 4 ditto, 8s.; after 6 ditto, 9s.; after 8 ditto, 10s.; after 10 ditto, 11s. Staff Surgeon—On promotion, 12s.; after two years' full-pay service in rank, 13s.; after 4 ditto, 14s.; after 6 ditto, 15s. Fleet Surgeon—On promotion, 17s.; after two years' full-pay service in rank, 18s.; after 4 ditto, 19s.; after 6 ditto, £1. Deputy Inspector General—On promotion, £1, 5s.; after 2 years' full-pay service in rank, £1, 7s.; after 4 ditto, £1, 9s. Inspector General, £1, 18s.

The following is a copy of the Papers set for the examination of Candidates for Her Majesty's Army, Indian, and Naval Medical Services, August 1889:—

ANATOMY AND PHYSIOLOGY.

1. Describe the fibula, the origin and insertion of muscles connected with it; and the ligaments which enter into the formation of the ankle joint.

2. Describe the origin, course, distribution, and anatomical relations of the great sciatic nerve.

3. Describe the small intestine from the pylorus to the ileo-cæcal valve, giving an account of its structure, anatomical relations, vascular supply, and innervation; as also of its functions and the processes carried on in different portions of the length of the tube.

4. Describe the parotid and submaxillary glands, giving an account of their anatomical relations, structural innervation and blood supply, the functions they perform, and the chemical and physiological character of the fluid they secrete.

5. Describe the dissection by which you would expose the subclavian artery, mentioning the parts of importance which would be also exposed, and their anatomical relations.

SURGERY.

1. Describe the symptoms by which syphilitic eruptions may be distinguished from those of a non-syphilitic origin, and the treatment to be adopted in the different varieties of the former.

2. Describe the dislocations common to the elbow joint, the complications which frequently attend them, the modes of reduction, and subsequent treatment of the limb.

3. A patient is the subject of femoral hernia. Describe the symptoms indicative of its becoming strangulated, the operation for its relief, and the general and local after-treatment.

4. What are the usual causes of retention of urine in old age, and the best measures for its relief? What subsequent conditions are likely to arise which require the attention of the Surgeon?

5. Describe the symptoms which would indicate fracture of the base of the skull, and the treatment to be pursued under the varying circumstances of such an injury.

6. A patient is the subject of a punctured wound in the groin, attended by arterial hæmorrhage, what treatment should be adopted in such a case?

NATURAL SCIENCE.

ZOOLOGY.

1. Enumerate in their order from without inwards, the fundamental germinal layers of the vertebrate embryo. Which of these gives origin to the brain and spinal cord, and which to the vascular system?

2. What is meant by a water vascular system? Name one or more examples of its occurrence in the animal kingdom.

3. Name the animal defined by the following characters:—

Nothocord persistent; spinal cord not presenting at its anterior extremity a distinct brain; respiratory organ in the form of a dilated pharyngeal sac; the place of the heart taken by pulsatile vascular trunks.

BOTANY.

4. By what anatomical characters may such flowerless plants as present a distinct stem and foliage be divided into two main groups? Give one or more examples of each of these groups.

5. What phenomenon in the life of plants admits of the closest comparison with the respiration of animals?

6. Refer to its Natural Order a plant with the following characters, and give one or more examples:—

Corolla gamopetalous, with the lobes imbricate in æstivation, bilabiate; calyx with five, more or less, united sepals, persistent; stamens inserted on the tube of the corolla, didynamous; ovary, superior two-celled, with the placentæ in the axis; fruit, a two-valved capsule; seeds numerous, albuminous.

PHYSICS.

7. A stone dropped into a well strikes the water at the end of three seconds, at what depth in feet is the surface of the water from the mouth of the well?

8. At what angle must a ray of light be incident on a plane reflecting surface, in order that the reflected ray be at right angles with the incident ray?

9. To what degrees of Fahrenheit's thermometer do the following degrees of the Centigrade respectively belong:—

100°, 96°, 29°.

MEDICINE.

1. A case for commentary—

In January, 1884, a lad, aged 20, began to have spasms (described as "cramps") in the left thumb and forefinger. These spasms or cramps consisted of "clonic" opposition of these named digits, occurring about twice a day for three successive months. The first severe fit happened in March, 1884. Spasm spread up the arm, and the patient fell. He had a second fit in January 1885; and another severe fit commenced a series of them, occurring once or twice a week, until admission to Hospital on December 4th, 1885. The character of the fits was almost always the same. They began by "clonic" spasmodic opposition of the thumb and forefinger of left hand; the wrist next, and then the elbow and shoulder were flexed clonically, then the face twitched and the patient lost consciousness. The hands and eyes then turned to the left, and the left lower limb was drawn up. The right lower limb was next attacked, and finally the right upper limbs. Paralysis of the left upper limbs frequently followed a fit. At frequent intervals every day the patient's thumb would commence twitching, but progress of the convulsion could often be arrested by stretching the thumb or applying a ligature on it. In February and March 1886 the twitching frequently commenced in the face, but again in April the thumb was the most frequent seat of origin of the fit.

On admission his condition was as follows:—(1.) As to *Power of Motion*—The grasp of left hand recorded 45, that of his right 85. He could perform all movements with the left upper limb; but those of the left hand were enfeebled. The left thumb was frequently in a state of rigidity, alternating with clonic spasm, a condition which could easily be induced by manipulating the thumb. (2.) As to *Sensation*—There was no affection save loss of muscular sense in the left thumb. (3.) As to *Deep Reflexes*—They were exaggerated in the left upper limb. The patient frequently had severe headache, beginning at the occiput and shooting forward, especially towards the right parietal region. (4.) As to *Optic Disks*—They appeared to be very pink, a condition of physiological hyperæmia.

Under these circumstances, on June 22nd, 1886, a tumour of a tubercular character was removed from the brain by a surgical operation, and by July the patient had recovered, having had no fits since the operation.

In commenting on the nature of the above case, explain what is meant by—(1) "Spasm;" (2) "Tonic spasm;" (3) "Clonic spasm;" (4) "Co-ordinated spasm;" (5) "Jacksonian epilepsy;" (6) Explain the method of analysing the convulsive symptoms in this case which point to lesion in a definite region of the brain; (7) Define by an ink line on the accompanying brain charts the motor region of the cerebral cortex, and mark with a × on the same chart the probable site of the tumour. (*Be so good as to attach the chart so marked, by its gummed surface, to your written answer to this question.*)

2. Explain the meaning of Lithæmia, and nature of the ailment it designates; describe the symptoms and treatment of the condition.

3. Discuss the Pathology of Typhlitis and Perityphlitis as regards:—(1) The origin; (2) the course of these affections. Describe how these conditions are diagnosed from other affections; and, lastly, give details as to treatment.

4. Acquired diseases of the heart in children (as distinguished from congenital affections) present certain peculiarities of practical importance. Write what you know regarding these peculiarities as to (1) the parts of the heart involved, and character of the pulse in children; (2) the causes of the lesions and the principles which ought to guide the treatment.

5. Discuss the principles by which you would be guided as to the use of Acids or Alkalies in the treatment of chronic dyspeptic conditions; and as regards the time for the administration of such remedies.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

ANTIFEBRIN IN THE TREATMENT OF EPILEPSY.—In the *Therapeutic Gazette* for June Dr Diller relates his experiences of antifebrin in the treatment of nine cases of epilepsy for the period of four months. It was administered in doses of 4 grains three times a day, except in two of the cases, where the dose was doubled. Dr Diller thus sums up:—1. That in all the cases in which the drug was given continuously there was noted a reduction in the number of fits ranging from about 25 to 75 per cent., as compared with other months, during which patients were on bromide and tonic treatment alternately. 2. The remedy was in all cases well borne, producing no apparent mental or physical depression. This in marked contrast with depressant effects noted after a course of bromide treatment. 3. No skin eruption was produced. 4. In any given case in which a great number of fits are occurring, and where it is desirable to control them as soon as possible, the bromides would be of far more value than antifebrin.

ANTIPYRIN IN THE TREATMENT OF URTICARIA.—Professor Hardy recognises a form of urticaria of nervous origin, in which nervous remedies are indicated, and in which Dr Nitot (*Journal de Médecine de Paris* for June 2, 1889) would attribute almost specific curative powers to antipyrin. Dr Nitot reports in full four cases, all occurring in young women who had since early childhood been subject to attacks of urticaria, in which alkaline treatment had utterly failed to produce any relief. In one of these the eruption appeared periodically each day, and Dr Nitot recommended the use of antipyrin in 7½ grain doses two or three hours before the expected period of eruption. After four days of this treatment the urticaria disappeared completely and did not return. His other cases are somewhat similar to the above, with the exception that the periodic element was not so well marked. In very chronic cases the improvement was less marked than in recent cases, though even then the cure was progressive and continuous.—*Therapeutic Gazette*, June 1889.

ANTIPYRIN IN CHOREA AND TETANUS.—In the *British Medical Journal* for 6th July Dr A. Garland, New Zealand, writes:—"F. C., aged 12, had an attack of acute rheumatism last year, without complications, from which he completely recovered under large doses of salicin. When seized with present illness, had been suffering two days; spasms almost continuous day and night;

semi-delirious. After twenty-one days' treatment by various methods without avail, chloride and bromide of potassium failing to produce sleep, antipyrin in 10-grain doses every four hours was given in the afternoon. That night he slept well. Next day the choreic spasms were less frequent, and in three days he was practically well. I may add that, at a consultation with two other medical men the day before the antipyrin was given, the prognosis was considered by all to be most unfavourable. Last week I gave antipyrin in tetanus, and although the case terminated fatally, the spasms were much relieved by the drug, more so than by doses of chloral ʒss. and pot. bromidi gr. xv. every four hours."

Nitrite of Amyl in the Treatment of "Status Epilepticus."—In the *British Medical Journal* for 10th August Dr Hugh Woods relates a case of this disease successfully treated by amyl nitrite. He administered by inhalation two capsules containing 4 grains each, and adds:—"There was a slight but distinct appearance of improvement at once, and the fits did not afterwards recur. After a time he recovered consciousness."

Menthol in Asthma.—In the *Therapeutische Monatshefte* for April 1889 Dr Theodore Jores writes, that since October of 1888 he has had a lady of 50 years of age under his treatment, in whom, in the summer of 1888, a number of polypous growths were removed from the nose. When the patient first came under his care she suffered from congestion of the head associated with great difficulty of breathing, the attacks coming on usually every two days. Footbaths, cathartics, and leechings over the mastoid processes of both sides, brought some relief. The attacks of congestion gradually disappeared, while the asthma became more and more prominent. A number of remedies were employed in the treatment of this symptom, which were, however, generally without avail. Dr Jores then made use of menthol in a 20 per cent. solution in olive oil, which was followed by the almost complete disappearance of all pulmonary symptoms. The drug was used as an inhalation, and from its very first application, and in every instance, served to relieve the asthmatic attacks.—*The Therapeutic Gazette*, June 1889.

Nitrate of Silver in Purpura Hæmorrhagica.—To the list of remedies Dr V. Poulet (*Bulletin Général de Thérapeutique,* May 30, 1889) would add nitrate of silver, since he believes that the admirable results which this remedy produces in the treatment of purpura would almost warrant attributing to this remedy the possession of some specific properties. In support of this view Dr Poulet reports two cases of purpura, which he stated were taken from his record of numerous others. In the first of these the hæmorrhage resisted the most varied and energetic forms of treatment, such as hypodermic injections of ergotine, tamponing the nasal passages, etc., and finally yielded to the internal administration of the nitrate of silver. In the second case the remedies employed were less numerous but equally fruitless; and here also nitrate of silver was especially efficacious, for, in a short time, all tendency to the formation of purpuric effusion disappeared, and the tendency to hæmorrhages was entirely overcome. Dr Poulet believes that nitrate of silver produces this effect in these affections by its action on the vaso-motor supply of the capillaries. He administers one-sixth of a grain of nitrate of silver twice daily in pill form, taking care to give it at least half-an-hour after eating.—*Therapeutic Gazette*, July 1889.

LOBELINE IN THE TREATMENT OF ASTHMA.—Lobeline, according to Dr Nunes, may be administered subcutaneously without producing any local reaction, and he advises its use in this manner where an immediate effect is desired. The author publishes tolerably full notes of nine cases of asthma in which lobeline, in doses varying from $\frac{3}{4}$ of a grain to 6 grains, seemed to produce decided relief from the asthmatic symptoms, and he terminates his memoir with the following conclusions:—1. Lobeline does not possess the toxic effects generally attributed to it in the doses which he recommends. 2. It possesses no emetic or nauseating properties, as is the case in lobelia, and its employment is, therefore, preferable where lobelia is indicated. 3. He has employed it in doses of from $\frac{3}{4}$ of a grain to 6 grains daily for adults; for children $\frac{1}{6}$ to $\frac{3}{4}$ of a grain. 4. It has no irritating action on the cellular tissue, and therefore may be administered in hypodermic injection, a fact which renders it more preferable than the tincture of lobelia. 5. The evident action of lobeline on the nervous system would seem to indicate its employment in other convulsive affections, such as tetanus. 6. The cases in which he has employed lobeline have remained permanently cured.—*Therapeutic Gazette,* June 1889.

PERISCOPE OF SYPHILOLOGY AND GENITO-URINARY DISEASES.

By FRANCIS CADELL, F.R.C.S. Ed.

THE EFFECTS PRODUCED ON MAN BY SUBCUTANEOUS INJECTIONS OF A LIQUID OBTAINED FROM THE TESTICLES OF ANIMALS (Dr Brown-Séquard).—He notices the general debility and lack of intellectual and physical activity found in eunuchs. The same results arise from seminal losses in proportion to their frequency. Spermatic plethora, on the other hand, especially in well-organized men from 20 to 35 years of age, who remain absolutely free from sexual intercourse or other cause of seminal loss, leads to a state of excitement, and a great, although abnormal, physical and mental activity. For long he believed that the weakness of old men depended on organic change and the diminishing action of the spermatic glands. He injected subcutaneously on himself a small quantity of water mixed with the three following parts:—First, blood of the testicular veins; secondly, semen; and, thirdly, juice extracted from a testicle crushed immediately after it had been taken from a dog or a guinea-pig. He is 72 years of age, and after the first few injections he found his strength manifestly increased. The power in urination and defecation was much improved. The facility of intellectual labour, which had diminished within the last few years, returned. On 4th June the last of these injections was made, and by the 3rd July he witnessed almost a complete return of the state of weakness which existed before the first injection. He holds this loss of strength an excellent counter proof of the influence exerted on him by the injection of a spermatic fluid. Dr Variot, without his knowledge, experimented on three old men with much the same results. They had no idea of what was being done, only being told that they were receiving *fortifying* injections. To show that it was not a mental effect, similar words of encouragement were used in two other cases whilst making injections of pure water, and no strengthening effect was obtained.—*Lancet*, July 20, 1889.

VACCINAL SYPHILIS (taken from *Journal of Cutaneous and Genito-Urinary Diseases*, July 1889).—Professor Fournier has recently collected his lectures upon this subject and published them in a little volume. The number of instances cited to demonstrate the danger of vaccination from humanized virus is so great that it is a matter of wonder that so many physicians still practise other than animal vaccination. The epidemics of Cremona, in which forty infants out of forty-six vaccinated were infected with syphilis, and, secondarily, a certain number of their mothers and nurses; of Morbihan, in 1866, where forty-five children were infected; and other smaller outbreaks of a similar nature, are mentioned. When syphilis has been inoculated along with vaccine virus, three alternatives may present themselves. Either syphilis is not transmitted, and, fortunately, this is the most frequent result; or the vaccination does not take, but syphilis is produced, and the symptoms and course are identical with those where the syphilitic virus alone is introduced into the body; or, finally, both forms of virus give a positive result, some points of insertion being followed by pure vaccine lesions, while the chancre appears only at the points where the vaccine has failed. Vaccinal syphilis always has an initial lesion, and this point must be remembered, so as not to mistake hereditary syphilis appearing shortly after vaccination for syphilis produced by vaccination. It does not by any means follow that the child from whom the virus is taken must be in an active stage of syphilis in order to transmit the disease in this way. The syphilis may be latent, or the child may even be in course of inoculation with syphilis, as proved by cases seen in epidemics. A healthy child is inoculated, and on the seventh or eighth day serves as vaccinifer, and the virus he furnishes may inoculate syphilis, and this before the chancre has appeared in his own person. A common error is still made to believe that the danger of vaccinal syphilis resides in the blood alone. Liquid without perceptible colour has in well-authenticated cases transmitted syphilis, but, without the aid of the microscope, it is impossible to say whether this colourless liquid contains blood elements or not. All coloured vaccine virus should be rejected. In the matter of diagnosis, ulcerating vaccinia must not be confounded with chancre, which it often simulates; but chancre never produces so extensive ulceration, and is accompanied by indolent adenopathies. Again, ulcerating vaccinia appears from twelve to fifteen days after vaccination, while the chancre seldom appears before the end of three weeks, and never before fifteen days. Vaccinal eruptions are at times mistaken for syphilides. They appear from the ninth to the fifteenth day after vaccination, while syphilides cannot appear before nine or ten weeks after this time. In vaccinating a number of children at once, the instrument should always be most carefully washed after each single vaccination. The only sure prophylactic measure is to employ only animal vaccine.—*Journal de Médecine*, April 1889.

OCCASIONAL PERISCOPE OF SURGERY.

By Edwin G. Bull, M.B.

Madelung on Fixation of Wound Dressings by means of Cutaneous Sutures.—Professor Madelung remarks that of late years the antiseptic plugging of wounds, especially with iodoform gauze, has been largely employed, but that in many cases it is difficult to keep the dressings in position. This is especially the case where the wound cavity is flat shaped or has partially osseous walls. In operating *(à deux séances)* for intraperitoneal hydatids, or pancreatic cysts, or morbid conditions of the stomach, pads placed in the wound in the abdominal wall are often found displaced or even entirely forced out of position by coughing or vomiting, though fixed by a tight circular bandage. Madelung raises as large a fold of skin as possible at each side of the wound, holds the two folds together above the tampon, and unites this and the cutaneous folds by means of several sutures of iodoform silk or silver wire. In the same manner after operations on the scrotum, after herniotomy, iodoform gauze wound tampons may be conveniently applied. Wounds in the neighbourhood of the anus or genitals may have dressings securely applied to them in the same way.—Prof. Madelung in *Centralb. für Chir.*, 1888, No. 46.

Chauvel, Minier, Breton, and Pesme on Lebel-Rifle Bullet Wounds.—The bullets are now constructed of lead with a facing of German silver. The experiments were made in winter with well-preserved cadavers at distances varying from 5 up to 2000 mètres. In all cases the bullet passed completely through the body struck, and Chauvel points out that the use of modern arms relieves the surgeon from the necessity of search for the projectile. The new bullet also never becomes altered in shape in its passage through the tissues. At close quarters there is extensive explosive action, at 800 m. and upwards the injuries are less severe, but only when the range is increased to 2000 do the bullets simply perforate the tissues. The aperture of entrance is generally round; it is oval when the bullet strikes obliquely, and is larger at the longer than at the shorter ranges. The shape of the aperture of exit varies, and may be either round, triangular, stellate, very irregular, or a simple fissure. Tendons, as a rule, escape laceration, but large and small arteries have been found wounded in six instances. In one case a portion of the wall of the epigastric was cut clean away. (Chauvel, etc., in *Arch. générales*, 1888, Oct).—Max Jungeugel in *Centralb. f. Chir.*, 1888, No. 46.

VON SWIECICKI ON COCCYGODYNIA.—The case was one in which most severe coccygeal pain followed the use of forceps for flat ricketty pelvis. Various methods of treatment, including faradisation, proved of no avail, and Swiecicki divided the coccygeal attachments of the *gluteus maximus, coccygeus, ischeo-coccygeus, sphincter*, and *levator ani.* For six months there was no pain, but then the unbearable agony returned. Von Sweicicki then proceeded to excise the entire coccyx. There was but little hæmorrhage, and the cure was complete. The coccyx was entirely necrosed. (V. Swiecicki, *Wien. Med. Presse*, 1888, No. 31).—P. Wagner in *Centralb. f. Chir.* 1888, No. 46.

VON WAHL ON THE DIAGNOSIS OF INTESTINAL OBSTRUCTION.—Prof. Von Wahl is of opinion that insufficient importance has been attached to localized tympanites in the diagnosis of these cases, and sums up his statement as follows:—That in certain forms of intestinal obstruction, strangulation, or volvulus, a portion of gut within the abdomen becomes abnormally fixed and distended, and that this fixed and distended gut produces visible asymmetry of the belly and increased resistance on palpation. When the ordinary classical symptoms leave the surgeon in the lurch the local tympanites may afford a correct clue to the seat of mischief. No exploratory operations will be undertaken, for the incision shall, as in herniotomy, lead the surgeon at once to the *fons et origo mali.* Prof. Von Wahl will publish shortly a series of clinical cases upholding his views in vol. 38 of Langenbeck's *Archiv.*—Prof. Von Wahl in *Centralb. für Chir.*, 1889, No. 9.

DU CASTEL ON THE ANTISEPTIC TREATMENT OF ACUTE GONORRHŒA.—Du Castel is an adherent of the gonococcus theory, and recommends antisepsis in the disease in question. After three years' probation he is well satisfied with the results obtained from the local use of resorcin. The greatest effect was produced when the remedy was applied at the beginning of the case or in its later stages, the least when used in the second week. From this Du Castel concludes that resorcin has no power over the gonococcus at the height of its development. The following are Du Castel's conclusions as to the treatment of acute gonorrhœa:—(1.) In some exceptional cases abortive treatment (with argent. nitras.) may be attempted. (2.) During the acute stage antiseptic treatment of the urethra is indicated, by preference with resorcin. (3.) After subsidence of acute symptoms the balsams, with or without simultaneous resorcin injections, may be used. (4.) In obstinate cases, however first treated, any single drug must not be used too long; but, as occasion may require, recourse may be had anew to antiphlogistic remedies. Du Castel does not say what strength of resorcin solution he employs, but Jaffé recommends one of from a half to three per cent. (Du Castel, *Gaz. des Hôpit.*, 1888, No. 3).—Jaffé in *Centralb. f. Chir.*, 1889, No. 9.

Hinterstoisser on Cavernous Angioma.—In the first of the two cases described, that of a boy aged two years, the growth involved the whole of the left side of the abdomen and the left loin. Bilroth, in whose clinic it occurred, removed the growth (which was congenital) in the following manner. From within outwards and from point to point skin and tumour were grasped by the hand, compressed and vigorously elevated whilst its base was secured by a number of Péan's forceps. Above and below the base of the now almost bloodless mass a strong perforated zinc plate (12 cm. long) was placed, and the two united by silver wire sutures. The mass was then cut off above the level of the plates, and the edges of the divided skin united by continuous suture. Cure. (Hinterstoisser, *Wien. Klin. Wochenschrift*, 1888, No. 18).—A. Fraenkel in *Cent. für Chir.*, 1889, No. 3.

Ricochon on Tetanus.—Ricochon regards tetanus as a true infectious disease communicable by the air as well as through direct contact. The solidungulae, of animals, are most often affected by it and frequently spread the infection. In this respect man and the ruminants come next in order. Tetanus germs long retain their vitality in dung soil and water, and it thus happens that agricultural implements and tools which are in constant contact with the soil form a storage place for such germs and a common source of infection. Tetanus frequently attacks those wounded with such tools. Should the germs be present in the soil in sufficient numbers a locality may become the seat of a veritable epidemic. Under certain cosmical influences also, not at present to be more nearly defined, the germs may be set free from the soil and give rise to an epidemic. Finally, the suspicion arises that there are certain regions where the tetanus germ is-autochthonous, and receives from the soil itself its tetanic virulence. (Ricochon, *Gaz. hebdom de Méd. et de Chir.*, 1888, Nos. 35 and 36).—Alex. Fraenkel in *Centralb. für Chir.*, 1889, No. 8.

Kocher on a further series of 250 Cases of Goitre.—Of the 250 cases operated on, only six ended fatally, a mortality of 2·4 per cent. Of these six, three were malignant (of 20 such operated on), and one an exophthalmic goitre (of 5 such cases). Excluding them, the death-rate would be but 0·8 per cent. Wound-treatment was aseptic rather than antiseptic. Catgut has been definitely given up. In the present state of our knowledge, the term *cachexia strumipriva* must be altered to *cachexia thyreopriva*, and it remains an essential condition that at all thyroidectomies a functionally active part of the thyroid gland be left behind. Kocher would only ligature *all* the thyroid arteries in cases of exophthalmic goitre. He reserves the term enucleation for operations involving merely the removal of goitrous nodules and cysts from the surrounding capsule of ordinary gland tissue. He has long used a method which he terms *Evidement*. In this the goitrous nodules are cut into and divided at once, and the segments extracted with the fingers and sharp spoon. Enucleation is easier and safer than extirpation, but it is doubtful whether it protects against recurrence. In general, extirpation is the operation for diffuse, enucleation for nodular disease. (Kocher, *Korrespond. für Schweitzer Aerzte*, 1889, No. 1).—Fraenkel in *Centralb. für Chirurgie*, 1889, No. 12.

Schuchardt on the Formation of Rice-bodies in the Sheaths, Tendons, and in Joints.—Schuchardt, after examining a number of cases in Volkmann's Clinic, concludes that the rice-like bodies are no fibrinous product of coagulation from synovia, but are indubitably altered masses of tissue from the synovial membrane. They resemble most nearly in their structurelessness the inner layer of the synovial membrane when in a state of coagulation-necrosis, and in this process (coagulation-necrosis) the chief factor in the formation of rice bodies is to be found. (Schuchardt in Virchow's *Archiv*, Bd. cxxiv.)—Rinne in *Centralb. für Chirurgie*, 1889, No. 17.

Part Fifth.

MEDICAL NEWS.

OBITUARY.

THE LATE DR ALEXANDER THOMSON, EDINBURGH.

It is with great regret we have to record the death of Alexander Thomson, M.D., F.R.C.S.E., one of the oldest, if not the oldest, practitioner in Edinburgh. He was born in the early part of this century at Eddleston, Peeblesshire, where his father carried on the business of farmer, and at the date of his death, on the 16th of September last, he had reached the age of 86 years. Originally intended by his parents for the Church, he completed the usual course of theological study; but from weakness of the chest, which it was feared might interfere with his clerical work, he decided somewhat later than usual to begin the study of Medicine; and in 1838 he became a Licentiate of the Royal College of Surgeons. In 1845 he took the degree of M.D. St Andrews, and in 1859 he was chosen to be a Fellow of the Royal College of Surgeons. He was also one of the oldest, if not one of the original members of the Obstetrical Society. During his professional life he devoted himself, from choice, chiefly to the artisan classes, among whom he laboured as a general practitioner and obstetrician, and by whom he was much esteemed as a generous friend and kind and able adviser. Such duties entailed upon him great toil and fatigue from night-work; but with his unusually methodical habits, he was enabled to overtake a large amount of reading, and thus to keep abreast of the times. Books, of which he has left a valuable collection in every department of literature, were always his companions, and his reading, which was extensive, was also most accurate; so that, with his retentive memory, he had his information always at his command. He was an honourable and Christian gentleman, most simple in his habits, and a kind and sympathizing friend. Liberal in politics, he was somewhat conservative in Medicine, and slow in adopting new remedies or modes of treatment. Now that he is gone from us, we can recall many pleasant meetings with him, while we sadly miss the deep-toned sonorous voice and Johnsonian language in which he was wont to deliver his opinion on current events.

PUBLICATIONS RECEIVED.

A. G. Bernard, M.R.C.S.,—On the Causes, Treatment, and Cure of Stammering. J. & A. Churchill, Lond., 1889.

Alex. Bowie, M.D., C.M.,—Harvey on the Circulation of the Blood. George Bell & Sons, Lond., 1889.

W. B. Cheadle, M.D., etc.,—Artificial Feeding of Infants, etc. Smith, Elder, & Co, Lond., 1889.

W. B. Cheadle, M.D.,—The Various Manifestations of the Rheumatic State as exemplified in Childhood and Early Life. Smith, Elder, & Co., Lond., 1889.

Joseph Coats, M.D.,—A Manual of Pathology. Longmans, Green, & Co., Lond., 1889.

Prosser James, M.D.,—A Guide to the Alterations in the British Pharmacopœia. J. & A. Churchill, Lond., 1889.

Dr E. Klebs,—Die Krankhaften Störungen des baues und der Zusammensetzung des Menschlichen Körpers. Gustav Fischer, Jena, 1889.

Joseph Leidy, M.D., LL.D.,—An Elementary Treatise on Human Anatomy. Smith, Elder, & Co., Lond., 1889.

Wm. Murrell, M.D., etc.,—What to do in Cases of Poisoning. H. K. Lewis, Lond., 1889.

C. G. R. Naylor, M.R.C.S. Lond., etc.,—Report on the Carbonic Acid Gas Treatment of Malarious Fevers. Maclachlan & Stewart, Edin., 1889.

George Oliver, M.D.,—On Bedside Urine-testing. H. K. Lewis, Lond., 1889.

Louis C. Parkes, M.D.,—Hygiene and Public Health. H. K. Lewis, Lond., 1889.

Prof. Lawson Tait, F.R.C.S.,—Diseases of Women and Abdominal Surgery. Vol. I. Richardson & Co., Leicester, 1889.

Part First.

ORIGINAL COMMUNICATIONS.

I.—THE SURGERY OF THE LIVER.

By Lawson Tait, F.R.C.S., LL.D., etc., Professor of Gynæcology in Queen's College, Birmingham.

(*Continued from page* 317.)

I have already said that there are other, and in some instances unknown diseases, which seem to yield to surgical treatment applied to them by accident, and I trust that these unexpected successes may some day lead us on the road to other victories.

I have had more than one occasion to draw attention to the astonishing disappearance of tumours, often of large size, after a mere exploratory incision. The absolute silence with which these statements have been received is very remarkable, and is capable of explanation, either by the fact that my papers have not been much read, or that the statements I have made on this subject have been received with a silent incredulity. They are true enough, though, for all that, and by-and-by the experience of others will substantiate them. The cases in which I have seen tumours disappear in this way have mainly been cases of diseases of the liver, spleen, and head of the pancreas. I have seen others, where the exact site of origin of the growth could not be accurately ascertained, disappear equally, and I have also seen at least one case of uterine myoma go away altogether after a simple exploratory incision. The cases are far too numerous, and the results indicate sequence far too clearly for us to dismiss the phenomena as a mere coincidence; nor can we accept the explanation of subsequent medical treatment as having brought about this much desired ending. I am satisfied that the mere opening of the peritoneal cavity has a direct influence in setting up the process of absorption of the tumour, and my conviction in this direction has increased my confidence in the principle of exploration. That some emphatic physiological change is at once set up by opening the peritoneal cavity is clearly indicated by the uniform onset of a most distressing thirst, which lasts for days, and is not seen so markedly after any other operation known to me. Let the incision in the abdominal wall be made down to the peritoneum, but let the serous cavity remain unopened, and this thirst is not marked. But let the peritoneum be opened but a finger's breadth, and the result is remarked.

That a therapeutic change is effected in the peritoneum itself by the mere opening of the cavity is now universally recognised in the treatment of what we call tubercular peritonitis by abdominal section. I have now had a large experience on this point, and can say positively, that we can cure permanently and speedily cases that have gone even as far as suppuration, by opening and cleansing. But in the bad cases in all probability the cleansing is never complete, no matter how much time and care is spent on it. And, in the non-purulent cases, I very often do no cleansing at all, but merely empty out the serum and put in a drainage pipe. Yet the great majority of these cases are cured by these simple means.

Let me now give a few cases of tumours and diseases of the liver, spleen, and pancreas cured similarly.

One of the most remarkable instances of this kind which has ever come under my notice was a patient, S. A. M., aged 30, placed under my care by Dr Van Vestrant of this city.

The abdomen was uniformly enlarged, and a distinct sense of fluctuation was to be felt in every diameter, somewhat obscure, but quite enough to make us perfectly certain that we were dealing with encysted fluid.

I therefore began the operation fully persuaded that I was about to remove a parovarian or ovarian tumour. The tumour when exposed, however, at once declared by its colour that a mistake had been made. Carefully examined, it was found to dip into the pelvis as a glutinous mass, and to be perfectly free from any connexion there. Traced upwards, it was found to be continuous with the substance of the liver, there being, however, a distinct pedicle for 6 or 7 inches across, attaching it to the under surface and anterior edge of that organ. I passed an aspirator needle into the mass in several directions without obtaining the exit of anything but a drop or two of bloody fluid. The wound was closed, and she made an easy recovery from the operation, which was performed on 12th January 1884, and went home on the 30th of the same month.

She has remained under observation ever since as an out-patient, coming up every three or four months for investigation, and no treatment of any special kind beyond a simple tonic of muriatic acid and gentian has ever been given to her; but since the exploratory incision she has steadily improved in health, and the tumour has slowly subsided in size, and at the same time being drawn up towards its hepatic attachments. It is now to be felt as a nodule no bigger than a man's fist. The nature of the tumour, of course, is absolutely unknown; it resembles nothing that I have ever seen before or since. It was not covered by liver tissue, and it certainly was not hydatid; and, in spite of the pseudo fluctuation which it presented before the abdomen was opened, it did not possess any fluid contents.

In June 1883, Dr Bradshaw Smith of Burbage sent me this patient, with the following history:—"The bearer, Mrs H., is suffering from some obscure disease situated in the region of the liver. I think there must be a deposit of matter. The beginning of last year she was confined, and had puerperal mania and several abscesses, being ill for some months. To all appearance she made a good recovery, and again became pregnant. Some three or four months after she was taken with very violent pain, like biliary colic. She became jaundiced after it, and had other symptoms resembling those of gall-stones. These symptoms continued more or less severe until she was confined nine months ago, recurring at intervals of a day or two. Since then the pain has continued, and she only gets relief from large doses of morphine. If you can throw any light upon the case I shall be much obliged."

I admitted her to my private hospital, and on 2nd July 1883 I opened the abdomen, and found the liver covered with small seed-like bodies, which I regarded as miliary abscesses. I closed it without doing anything. She made an easy recovery, and went home. Her symptoms were at once mitigated, and in a few weeks she sent me a letter to say how extremely grateful she felt for the wonderful cure she had received, and to tell me that she enjoyed the best of health. I had a letter from Dr Bradshaw Smith on 15th April 1886, in which he says,—"The woman H., whose liver you explored and found covered with small seed-like bodies about three years ago, has been in good health ever since, and she has just engaged me to attend her next May. Surely 'truth is stranger than fiction.'"

This patient was sent to me by Dr Coghill of Ventnor, suffering from a series of acute symptoms, the chief of which was intense hepatic pain. I made an exploratory incision, but could not find anything except adhesions between the edge of the liver and the wall of the peritoneum. These I separated, and closed the abdomen. The patient was relieved for a time, but all his old symptoms have returned.

As I have already said, we must carefully consider the results of an operation in the two shapes—primary and secondary. In these simple exploratory incisions the primary results of the seventeen cases which I have placed in my table have been perfectly satisfactory save in that of No. 7. There the exploration was made at the suggestion of Dr Clifford Allbut, and under the suspicion that there was a hepatic abscess. After I had opened and explored with my finger, I passed an exploring needle several times into the substance of the enlarged liver. I do not think that this had anything to do with the fatal result, as the poor fellow was so ill at the time that his death was only a matter of a few hours unless we could relieve him, and it occurred in the progress of the disease.

It is doubtful, however, if this ought to be reckoned as a simple exploratory incision. The risks of the proceeding, however, I reregard as very slight. All the primary results in the way of recovery were almost uniformly satisfactory; but in these mysterious secondary results, where the simple exploratory incision cures, we have abundant cause of satisfaction.

Of the sixteen cases half have died of the progress of the disease, but there are eight in which a cure has been effected.

One of the most remarkable was a case to which I was called by Professor W. Smith and Dr J. Denham of Dublin, in the case of a lady, aged 54, who was the subject of symptoms strongly pointing to the possibility of gall-stones. My own impression on seeing her, however, was that she had cancer of the liver, and on making an exploratory incision I found the organ scattered with large hard nodules, one of which closely imitated the lump which had led to the diagnosis of distended gall-bladder. We not only felt the lumps, but we inspected one or two of them by eye-sight, and I, for one, had no doubt whatever that my diagnosis was correct. Whether it be so or not, there can be no question that the patient has been cured, for she is now alive and perfectly well, although her appearance at the time of the operation was such as would justify prognosis for her of not more than a few months of life.

Four times in my life I have opened the abdomen for the purpose of removing enlarged spleens, and in every one of the instances I have been deterred from proceeding with the operation by reason of the hopelessness of the out-look for the patient. Strange to say, in three of the four patients the tumour has disappeared, and they are now to my knowledge—or were at least quite a short time since—in perfect health; the fourth succumbed to the exploratory incision.

In December 1885 I was called to see a lady, about 49 years of age, who had been suffering for some months from symptoms judged by her medical attendants to be indicative of gall-stones, and there was a tumour which they regarded as a distended gall-bladder. In my opinion, however, the tumour was too low down for the gall-bladder, and was completely tethered to the posterior wall of the abdomen. I was of the opinion that it was a case of tumour of the head of the pancreas.

The lady had been for many years an intimate friend of my own, and her appearance was so altered and so characteristic that I had not a shadow of doubt that the disease was cancer. At the earnest solicitations of her family and her attendants, but almost against my own conviction, I made an exploratory incision to determine the actual condition. I found a large hard mass in the position of the head of the pancreas, perfectly immovable, and of such a character that I did not even think it worth while to pass an exploratory needle into it, but closed the wound. In a few days the patient began to improve, and in six or seven weeks her former health was restored, and not a trace of the tumour could be felt. She has remained from that day to this in absolutely robust health. Of course, to say this was a case of cancer of the head of the pancreas would be to forsake the definition of cancer obtained from the late Mr Syme, and to which I adhere with absolute pertinacity. It was not an incurable disease, in so far that it was cured by my exploratory incision. That it was an abscess or anything but a positive growth of new tissue I cannot for a moment believe, still less can I hazard any explanation of the extraordinary facts—a few of which I have now given in detail.

Passing from this subject to that upon which we can speak with more decision, I think modern surgery is to be congratulated upon the distinct advance it has made in the treatment of abscesses of the liver and hydatid tumours of that organ. After what has been published upon the subject, it is strange beyond belief to see placed upon record cases in which abscesses of the liver are allowed to run their own way unguided. A few weeks ago I saw in a prominent medical journal a case narrated with every sign of satisfaction, in which an abscess of the liver had been watched after a clear diagnosis had been made, month after month, till it ruptured through the diaphragm into a bronchus, and suffocated the patient by the sudden discharge of pus. Such cases are neither more nor less than a disgrace to the art of true practice, nor is it anything better to see still going on discussions on the propriety of tapping hydatid tumours of the liver. The method for dealing with both of these diseases completely and satisfactorily is by abdominal section.

As will have been seen in the previous part of this paper, I have now on seventeen different occasions deliberately attacked these two diseases in this method, and I have been completely successful in fifteen of the instances. The first case in which I deliberately opened the tissue of the liver to remove a hydatid tumour has already been published in detail, but the case is so striking in its features, and of so much interest as having been the first of its kind on surgical record, that I venture here to reproduce it.

On August 15, 1880, I was asked by Dr Thelwell Pike, of Malvern, to see a lady, Miss E. G., aged 37, whose illness had the following history:—

Between 1870 and 1872 she broke down in health, suffered from obscure symptoms of which she can now give no very clear account, but which were referred, by three practitioners whom she consulted, to the spine. In 1872 she consulted the late Mr Cardew, of Worcester, who diagnosed some hepatic mischief, but gave no decided opinion.

In 1873 she had a severe inflammatory attack, the symptoms of which were regarded by her medical attendants as being due to diaphragmatic pleuritis. That illness continued for three weeks. Since then she has never been well, suffering from bilious attacks, swollen legs and feet, dyspepsia, inability to walk, and great mental depression. She asserts that the right leg has always been more swollen than the left.

In 1876 she and her friends noticed an alteration in her size; she had to have her dress let out, her breathing became interfered with, and an enlargement on the right side became apparent. This increased slowly till 1879, when it was evident that the whole of the right chest and abdomen were enormously increased in size, but it was not till February of this year that any attempt at diagnosis seems to have been made, and the opinion seems to have been that the enlargement was due to malignant tumour. In July she came under Dr Pike's care, and he diagnosed it as a case of hydatids of the liver, and this diagnosis was confirmed early in August by Sir William Jenner, who advised the use of the aspirator. Dr Pike and Mr Dawson, of Malvern, aspirated the tumour on 11th August, and withdrew a few teaspoonfuls of clear serum, enough to establish the correctness of the diagnosis of multiple hydatids, even though they could find no scolices in it.

When I saw her on the 15th, I found her in such a condition that it was evident death from suffocation and exhaustion was imminent if relief could not be given. She was propped up in bed to relieve her breathing, and was vomiting incessantly. She was extremely emaciated, had a hay-like odour of breath, pinched features, and yellow skin, and all the symptoms of extreme exhaustion. The hepatic dulness extended from the third rib down to the umbilicus, crossing the middle line to the left all the way for about two inches, and much more at the lower margin. The whole of the right side was occupied by the tumour, no air was entering the right lung, the left was greatly interfered with, and the heart was pushed much over toward the left. Below the right ribs distinct fluctuation could be obtained over the tumour.

Acting upon the principle, which I have already advocated in previous communications upon this subject, of opening the abdomen in all cases of tumours where life was threatened, and of the malignity of which there was no certainty, I had no hesitation in proposing abdominal section in this case.

Dr Pike at once concurred in my proposal, and it was readily accepted by the patient and her friends.

I therefore returned to Malvern the next day (16th August), and performed the following operation:—Dr Pike gave ether, and I was assisted by Mr Dawson and Mr Raffles Harmar. I made an incision four inches long, and about two inches to the right of the middle line, beginning at the edge of the ribs, and inclining slightly inward toward the umbilicus. Having carefully secured all the bleeding points, I opened the peritoneum, and found that there was no adhesion of the liver to the wall, and that I had exposed healthy liver-tissue. Into this I passed a large-sized aspirator needle, and evacuated a few teaspoonfuls of clear serum, as had been done before. Removing the needle I passed my knife into its track, and made an opening large enough for my forefinger. I then found that the layer of liver-tissue was from half an inch to three-fourths thick. I then fixed a pair of Koeberle's catch-forceps on each of the margins of the wound in the liver, and asked my assistant (Mr Raffles Harmar) gently to draw them up as I enlarged the incision. This I did to the extent of about three inches, and the moment I freed my finger, myriads of transparent globes of all sizes, from a pea to an orange, shot out, covering the table and floor, and were afterwards picked off the floor all over the room. When the tension was relieved, I dug them out with a large silver gravy-spoon, an instrument suggested to me by Dr Pike, and this process took much more time than the whole of the rest of the operation; and during its performance Mr Harmar most skilfully prevented any cysts entering the peritoneal cavity, by keeping the flaps of the liver close against the abdominal wound. Finally, I perceived that my gravy-spoon was causing some hæmorrhage from the inside of the cavity, which had no kind of lining membrane, and I had to leave a considerable quantity of cysts in the cavity. In the cut surface of the liver, two bleeding points gave me some anxiety, but I closed them temporarily with Koeberle's forceps, and finally secured them in the stitches. These I applied by a common short needle and piece of silk in the continuous method, fastening the wound in the liver through the whole thickness of the tissue to the wound in the abdominal wall, so as effectually to close the peritoneal cavity; I then fastened in a wide glass drainage-tube eight inches long. The quantity of hydatid cysts evacuated is estimated by Dr Pike to be about two gallons, and I think the amount is not exaggerated.

The patient rallied well from the operation, and seemed to suffer nothing from shock. Her sickness ceased immediately after the operation and did not return, and her breathing became at once relieved, so that she could lie flat on her back, or on either side.

I saw her again with Dr Pike on the 19th, when I found her without a bad symptom, eating well, entirely free from pain, and with the hepatic dulness contracted to almost normal limits. A large number of cysts had come through the tube daily with the discharge, which was faintly tinged with bile. Dr Pike washed out the cavity twice a day with weakly carbolized water.

I saw her again on 2nd September, and found the wound healthy, that Dr Pike had removed the stitches, and that the cavity held only about half a pint. Only one very small cyst had come away since my previous visit. I found also that she was gaining flesh rapidly and eating well, her diet that day having included bacon, cheese, and porter.

The daily details of the case possess but little interest, beyond the fact that at the end of the first week a short (three-inch) wide glass tube replaced the long one, and in a fortnight more, a $\frac{3}{8}$ rubber tube was inserted instead of the glass. Fragments of cysts continued to come away for about a month, and upon 17th October there had been hardly any discharge at all for a fortnight, and nothing remained but a sinus.

Dr Pike notes that one day during the syringing out of the cavity she had a sharp, sudden pain passing round from right to left. This lasted some three or four hours, and after that about half a pint of bile was passed from the wound, and the pain gradually ceased.

The patient herself wrote to me that she felt quite well, and was able to walk about alone, not quite eight weeks after the operation. She is now in perfect health, and has married.

I have now twelve times operated in this way, and only one of the cases has succumbed; and she succumbed, not to the primary operation, but to the secondary result of it.

I was called to the patient by Mr Brookhouse of Nottingham, and found a solid hydatid cyst, which I opened and emptied of its contents, stitching the liver, as usual, to the edge of the abdominal incision. She made a long, steady effort to recover, but she died some three months after the operation from exhaustion, due to the perpetual flow of bile from the wound. It was clear that I had either opened a large duct in my incision in the liver, or, what I think more likely, that the progress of the disease had involved and opened one of the major ducts. In any case, from the wound there flowed a continuous stream of bile, which ultimately, in conjunction with the suppuration, wore the patient down.

If I had to deal with that or a similar one again, I should have no hesitation in reopening the cavity and searching for the apertures of the duct, and contrive, if I possibly could, to divert the bile into its normal channel.

Of hydatids of the liver I am perfectly certain there are two kinds: the more common is the large single cyst formed of gelatinous layers easily stripped from one another, and in which, under very high powers of the microscope, a dark fibrinous structure is to be discovered.

The fluid in this cyst is always limpid and free; in it float innumerable quantities of scolices of a tape-worm. It is always embedded in the substance of the liver, and to reach the cyst considerable thickness of liver tissue has generally to be gone through. The tissue of the cyst is in immediate contact with liver tissue, which it excavates into a form and shape to suit itself. These are cysts which are sometimes cured by tapping; but it is far more frequently simply a supposed cure than a real one, for five or six of the cases upon which I have operated had been supposed to have been cured by that process. I doubt very much if they really ever are cured by tapping. Within the last few weeks I have seen a lady, with Dr M'Munn of Wolverhampton, who was tapped, and who was supposed to have been cured of hydatid tumour in her liver nearly twenty years ago, and there it is still, and is now increasing.

The other variety of hydatid is the multiple variety, where the cysts are numerous, and vary in size from a pin's point to that of a cocoa-nut. They lie packed together in a cavity of the liver which is not lined by any sac, and in the wall of each of these cysts there are fastened to the base enormous numbers of scolices of another tape-worm. This variety corresponds very nearly in form and appearance with the sacs seen in the brains of sheep suffering from the disease known as sturdy. This second class constitutes the cases in which the tissue of the liver ruptures, and the hydatids are poured out loose into the cavity of the peritoneum, and then they penetrate the tissues in all directions, forming the advanced embedded hydatid disease of the peritoneum, which is one of the most hopeless conditions the surgeon can ever be called upon to deal with by operation.

When first I attacked the liver by surgical operation I certainly was in terror of hæmorrhage, for I thought that if an incision opened a large sinus the arrest of hæmorrhage would be a matter of considerable difficulty, but I was encouraged by an accident which befell me in performing an ovariotomy, for there, on undoing an adhesion to the liver, I tore a cleft in the free edge of the organ, certainly an inch and a half deep, which bled freely, and I was greatly alarmed; but I took a small piece of solid perchloride of iron about the size of a pea and rubbed it over the edge of the tear. The hæmorrhage stopped immediately, and my patient made an easy recovery, so that my respect for the liver greatly diminished.

On one occasion I did open by my incision a sinus of considerable size, but I passed a thread by means of a fine needle down one side of it and up the other, and tied the sinus between the two limbs of the ligature. In this way the hæmorrhage was arrested, and I heard nothing of the ligature. I was also in fear of another condition which, *à priori*, one might have expected—that it would be difficult to stitch the edge of the wound of a friable organ like the liver to the abdominal wall, and that the stitches would be very likely to give way. As a matter of fact, I have not found it so, and in not a single instance has this given trouble.

In my operations upon abscesses of the liver all the cases have recovered but one, and that was a case in which the adhesions had taken place already, and I had no need to make any effort to produce adhesion. The first case in which I operated was, like my first case of operation for hydatids, so remarkable that I reproduce it at length.

J. D., aged 56, was seen by me for the first time on February 5, 1881, in consultation with Dr G. P. Hadley of Lozells, under whose care, in conjunction with Dr Heslop and Dr B. Foster, he had been for some months. Dr Hadley has favoured me with the following notes:—He saw J. D. for the first time in August 1879, when he had an attack of severe illness which was regarded as due to the passage of a gall-stone. In January 1880 a large tumour was discovered occupying the whole of the epigastrium, right hypochondrium, and extending downward into the right iliac region. The tumour had an indistinct fluctuation. During 1880 the patient became greatly emaciated, passed generally clay-coloured stools, and frequently had his urine deeply tinged with bile. In December 1880 the cyst seemed to find an opening into the intestine, for the tumour became greatly diminished in size, and the patient passed large quantities of brick-red fluid from the rectum. After this discharge the cavity seemed to refill in a few days, and the process was repeated at intervals. In January 1881 the process of emptying seemed to cease, and it was proposed to tap the cyst, but on account of the presence of intestines all over the front of the tumour it was deemed more prudent to have an exploratory incision made, and I was asked by his attendants to undertake this. I had no hesitation in doing so, for I found the patient extremely emaciated, with a distinct icteric hue, and evidently sinking. There was a very large cystic tumour, apparently belonging to the liver, and on February 6th I performed the following operation, assisted by Mr Wright Wilson and Dr Williams, of Dyffrin, the ether being administered by Mr Bennett, Dr Hadley's assistant.

I made an incision about three inches in length over the tumour, in the axis of the right rectus muscle, and about three inches to the right of the middle line, beginning about two inches above the level of the umbilicus. The peritoneum was easily reached, but there I found intestines and omentum glued everywhere over the surface of the tumour, and I had to exercise much care in dissecting them off, so as to clear a part of the cyst about two square inches in area. There was, however, no adhesion between the parietal layer of peritoneum and the subjacent intestines. I then passed my small-sized trocar into the tumour, and evacuated seven and a half pints of dark, bilious-coloured fluid. When the cavity was emptied completely, I enlarged the opening made by the trocar so as to admit two fingers, and came at once upon a loose mass, which I removed, and which proved to be a slough of liver-tissue weighing about one ounce. I then stitched the edges of the wound in the liver to those of the wound of the abdominal wall, and fixed in a glass drainage-tube. The cyst was clearly the liver itself, which had been distended into a shell, with apparently a pretty uniform thickness of about half an inch. The fluid removed was carefully examined by Dr Saundby, the pathologist to the Women's Hospital, and found to consist of nearly pure bile, mixed with pus.

The progress of the case must be given briefly, for there is very little to tell. No effort was made to conduct the treatment upon Mr Lister's principles. The glass drainage-tube was left in for about a fortnight, and then a piece of rubber tube replaced it. The temperature and pulse curves were almost normal, the patient's appetite rapidly improved, and upon my last visit to him (March 30th) there was very little discharge from the drainage-tube, and he had gained fourteen pounds in weight in the seven weeks which had elapsed since the operation. (*P.S.*—He has gained forty-two pounds since the operation, September 16th.)

I think it would be perfectly evident from the narration of such cases as the above, that, upon that patient an attempt to procure adhesion between the peritoneal surface of the abdominal wall and the wall surface of the liver would be perfectly needless, and that operations may be done and ended at one sitting, with as great readiness upon the liver as upon any other organ in the abdomen.

II.—DIPHTHERITIC PARALYSIS OF ACCOMMODATION.

By William George Sym, M.D.

The question of the true position and nature of the lesion in post-diphtheritic paralysis is still unsettled. Although much has been written on the subject, one reason for this is that only rarely does an opportunity arise to examine microscopically the parts affected,—so rarely, indeed, that some have doubted whether such cases are of the same nature as the more usual. Another reason is that in very few cases or none does the condition ever remain permanent; almost all patients recover eventually, recover completely, and recover irrespective of treatment. In this paper I intend to look at the question from a clinical point of view, as shown by some cases of accommodation paralysis with which I have recently met. The ocular paralysis is a very common variety, and affords us a good ground on which to work at the pathology.

Case I.—J. M., a girl, æt. 8. Ill of "ulcerated sore throat" when there was diphtheria in the neighbourhood. The child was confined to bed "a week or two," and the vision was only observed to be defective when she was sent back to school, six weeks after the attack began. On examination the pupils were found to be of moderate size, equal, and mobile to light. Convergence was good, but pupils did not contract during that movement: $V = \frac{20}{50}$ R. and L.; $+ 1\cdot5\ D = \frac{20}{20}$. Near vision—only very large print;

+ 5·5 D = No. 1 J fluently. A fortnight later paralysis of throat began. The patient recovered completely.

Case II.—M. S., a girl, æt. 14. A fortnight after recovery from a very severe attack, the eyes were examined. Pupils equal, moderate, contracting on convergence, which is free, and also, though somewhat sluggishly, to light: $V = \frac{20}{40}$; $+ 1 D = \frac{20}{20}$. Near vision—large type; + 5 D, No. 1 J at 4 inches. Patient recovered completely, but the pupils long remained sluggish.

Case III.—A. M., a girl, æt. 8. After a mild attack there were very serious nervous symptoms,—vomiting, fainting, etc. Pupils widely dilated, the right being more so than the left, and both acting very sluggishly to light. Convergence is poor, the pupils acting along with it very slightly: $V = \frac{20}{200}$; $+ 3 D = \frac{20}{20}$. Near vision—no print; + 7 D = No. 1 J fluently as near as 6 inches. Within this distance convergence failed, and there was diplopia. Recovery was very slow, but was eventually complete.

Case IV.—B. C., a girl, æt. 10. Presented herself a fortnight after recovery from an attack which was not severe. Pupils moderate, left being rather the larger, mobile to light, but not at all so to convergence. Convergence fails when object is brought nearer than 18 inches: $V = \frac{20}{70}$; $+ 1·5 D = \frac{20}{20}$. + 5·5 D = No. 1 J. Recovery complete, though at one time there was a return of dysphagia, etc.

Case V.—G. M., a man, æt. 30. Presented himself three months after being affected with an "ulcerated sore throat," which he believes he acquired through "catching cold." He knew of no neighbours or others from whom he could have become affected with diphtheria. He never had any other paralysis than that of the ciliary muscle. Pupils are of moderate size, the left being rather the larger; they act freely along with convergence, which is good, but are sluggish to light: $V = \frac{20}{30}$; $+ 1 D = \frac{20}{20}$. Near vision = No. 5 J; + 3·5 D = No. 1 J at 6 inches. Hypermetropia estimated ophthalmoscopically at 1·5 D. A hypermetrope of that degree at 30 years of age, and possessing normally 7 D of range of accommodation (Donders, Landolt), has his near point at 6 inches. Since the patient required a lens of + 3·5 D to accomplish this, he can only have at command half his normal amount of accommodative power. Neither eserine nor pilocarpine gave any lasting good result, though they temporarily caused contraction of the ciliary muscle. He improved slowly, but in three months the cure was by no means complete, and the pupils were still sluggish to light.

Case VI.—M. A. W., a girl, æt. 10. Six weeks before she presented herself she had suffered from what had been supposed to be mumps, her throat being sore at the same time. Sight noticed to be defective on her return to school: $V = \frac{20}{70}$; $+ 1{\cdot}5D = \frac{20}{30}$; $+ 5{\cdot}5\,D =$ No. 1 J. Pupils are freely mobile to light, and along with convergence, which is good. After this she began to suffer from regurgitation of fluids through the nose, nasal speech, etc. Other cases having since occurred in the neighbourhood, where the sore throat was better marked and the swelling of the glands less so, this case was believed to have been one really of diphtheria. Recovery complete.

Case VII.—A. M., a boy, æt. 11. Was ill of severe sore throat six weeks ago. No physician was called in, but as the disease attacked several of the family, and diphtheria was prevalent at the time, the cases were probably of that nature: $V = \frac{20}{70}$, $+ 1\,D = \frac{20}{20}$; $+ 5\,D =$ No. 1 J. Convergence was good, and the pupil movements free.

According to Maingault, Chomel was the first to describe paralysis as the result of a form of sore throat. In the same year (1747) Ghizi of Cremona gave an account of some cases seen by him. Guimier (de Vouvray) has, however, the honour of first mentioning "amaurosis" as a sequela. A case—apparently the first diagnosed in this country—was observed by Mr Dixon at Moorfields Hospital, and was published in the *Medical Times and Gazette* for 1859. "Does the affection," he asks, "depend on an impaired tonicity of the muscular apparatus of the globe, inducing difficulty of regulating the foci?" Trousseau says that Follin suggested the explanation of a muscular paralysis before any one else. (Trousseau himself, however, did not accept that theory.)

Before going any further, it is necessary to glance for a moment at the nervous relations of the three associated functions of accommodation, contraction of pupil, and convergence. The three are to a certain extent closely connected. Thus convergence cannot occur to any considerable degree without accommodation and contraction of the pupil, and accommodation only to a limited extent can take place without convergence and myosis. There is no power of voluntary contraction of the pupil. But lateral movements of the eyes, in which one or other internal rectus muscle is used, are not associated with accommodation or myosis; and the action of light on the pupil has no effect on the ciliary muscle. In these circumstances, then, one would expect the pupil and the internal recti to be represented in the central nervous system by two centres each; in the case of the pupil, a centre for contraction to light and one for contraction along with accommodation; in the case of the internal recti, one centre for lateral and one for convergence movements. The muscle of accommodation, the constrictor pupillæ, and the internal rectus, are all supplied by the third nerve. By a series of experiments Hensen and Voelckers have shown that in the dog the most anterior of the centres of the nucleus of that nerve is that for accommodation, next follows that for contraction of the iris, and then that for the internal rectus. This area may thus be regarded as a centre for the three combined. Adamük found a spot in the corpora quadrigemina, irritation of which produced movement of both eyes to the right, another for movement to the left, and a third for downward movement and convergence.

Leaving the above necessary digression, we come to the important question, "In what relation does the subsequent paralysis stand to the antecedent diphtheria?" To this question there are, I think, three possible answers :—

1. That the paralysis is simply a coincidence. I think none hold this opinion nowadays, although it was at one time considered correct, so I may dismiss it.

2. That the paralysis is not more nearly related to diphtheria than to many other diseases.

3. That the paralysis is caused by diphtheria, and is peculiar to it.

Ebert showed to the Medical Society of Berlin, in 1861, a case of diphtheria, in which there was paralysis of palate and eye. In the course of the discussion on the subject, Von Graefe went the length of denying all special connexion between paralysis and diphtheria. He considered that paralyses were frequent after every grave disease of the nervous system—typhoid, erysipelas, etc. Every disease which was accompanied by fever was accompanied also by paralysis of accommodation, and a semiparalytic state of most of the muscles of the eye. Sir John Rose Cormack and others have more recently expressed somewhat similar views. If this second answer to the question above be true, one must look for a cause of paralysis common to all acute diseases, and this can only be—is by the upholders of this theory only stated to be—asthenia, anæmia, debility produced by the violence of the original malady. They thus make the accommodation paralysis of diphtheria analogous to the weakness of the ciliary muscle which sometimes shows itself during lactation, but which is in reality merely a sign of diminished nervous and physical strength. It was at one time suggested that diphtheritic paralysis was due to the patient "catching cold," and thus acquiring a rheumatic paralysis; but as paralysis of accommodation is probably invariably bilateral, as simultaneous bilateral rheumatic palsies are very rare indeed, as double rheumatic cycloplegia is never found apart from diphtheria, and as there are seldom any grounds for supposing that the patient has caught cold, I think we may reject this theory. That the third answer is the correct one I am quite certain, and it is partly upon the truth of it that I base the theory of causation of the subsequent paralysis which I am inclined to favour.

I place briefly under *nine* headings the principal points in this discussion.

1. *The remarkable frequency with which attacks of diphtheria are followed by paralysis of some organ.*—Out of 37 consecutive cases of diphtheria occurring at the City Fever Hospital of Edinburgh, 14 *at least* suffered from paralysis of some part.[1] I should have liked to have given even an approximate estimate of the percentage of cases of diphtheria followed by paralysis, but do not feel myself justified in doing so; one so often hears of cases of diphtheria diagnosed by the occurrence of paralytic symptoms when there had been previously no suspicion of the specific nature of the sore throat, that one cannot help wondering how many mild, undiagnosed cases of diphtheria occur which never show any such symptoms, or in which they are ascribed to weakness. The proportion of paralytic cases has been variously estimated at from 8 to 66 per cent. of the patients who recover.

2. *The comparative rarity of paralysis after other diseases.*—Cormack says, "During the course of, and in the convalescence from no other acute disease, are asthenia and anæmia so constant and so profound as in diphtheria." I do not think that this statement is correct. Asthenia and anæmia are seldom so profound as they are after enteric fever, yet the percentage of paralytic sequelæ of this fever does not approach the lowest estimate of diphtheria. I do not suppose that paralysis follows acute pneumonia once in five hundred times, although there is no question about the severity of the prostration after that disease. As Hermann Weber says, though it is quite true that in the majority of cases of diphtheritic palsy there is some anæmia, in others there is little or none; in others, again, it does not appear till after the paralysis is quite established, so that the anæmia can hardly be the cause of the paralysis. It is an interesting criticism on this matter, too, that the majority of writers on pneumonia, fevers, etc., never mention the occurrence of paralysis, while scarcely a pamphlet even has within the last twenty or thirty years been published on diphtheria which does not contain a reference to subsequent nervous symptoms.

3. *The curious selection of the seat of paralysis.*—Maingault's statistics show that by far the commonest situations are the palate and ciliary muscle. Not only so, but one sees only few cases of diphtheritic palsy affecting other parts in which these two are not also implicated. When paralysis does follow other diseases there seems to be no such predilection for any special muscles, and certainly not for these particular ones.

4. *The fact that while one part is paralysed another is at the same time in a state of healthy activity.*—Thus a patient just convalescent from diphtheria can run about, take a walk of a few miles, or play a short game at lawn tennis, while his palate is so affected that

---

[1] This statement is made on the authority of Dr Wood, the resident physician, who very kindly took the trouble to look up the numbers for a certain period.

fluids, on an attempt to swallow, regurgitate through the nose. Trousseau in reference to the throat, and Donders in reference to the eye, particularly mention this fact. It is noteworthy, too, that the last symptom of nervous implication to disappear, viz., the interference with the "tendon-reflex," often persists for months, even after the whole frame seems to have returned to its usual vigour.

5. *The paralysis does not occur at once.*—This, I am aware, is not universal, but in the majority of cases the paralysis does not begin to show itself until after improvement in health and strength has been proceeding for some time.[1]

6. *The paralytic symptoms are frequent after mild or comparatively mild primary attacks.*—Wagner, Oertel, Jacobi, and Keyworth are inclined to the opinion that such attacks are more likely than severe ones to be followed by paralysis. Trousseau, Greenhow, and others do not think this to be the true state of affairs; but whichever be the most common, it is undeniable that a considerable number of cases of mild diphtheria are followed by severe paralysis. Such occurrences are so frequent as hardly to require illustration. I shall merely mention a case of Weber's in which paralysis affecting palate, eyes, arms, and legs, and being sensory as well as motor, and associated with somewhat prolonged impotence, followed an attack of faucial diphtheria so mild as to be hardly recognisable.

7. *Pathological changes have been in some fatal cases observed in the nerve centres, nerves, and muscles.*—It used to be supposed, before such changes had been found, that the absence of them indicated with certainty a simply asthenic cause. That confidence is now destroyed, but I postpone the discussion of the exact nature and position of the lesion.

8. *The change in the electrical reactions of the affected muscles.*—On this subject there is a great deal of difference of opinion even among the best authorities. The reaction to galvanism has been found normal, and has been found diminished; the reaction to faradism has been found diminished, increased, and absent. Under these circumstances one cannot with safety draw much inference from the electrical state of the affected part.

9. *The symmetry of the paralysis.*—This would seem rather to point to an asthenic than to a peripheral organic paralysis. So far as I have myself seen, or have been able to ascertain, no case of unilateral cycloplegia from diphtheria is known, and certainly the other paralyses are far most frequently symmetrical, but paralysis of the sixth nerve on one or other side is not unknown, and hemi-

---

[1] C'est presque toujours à une époque assez éloignée du moment où la fausse membrane a disparu, deux ou trois semaines après la cessation de tout phénomène morbide du côté de la gorge, qu'on voit survenir les premières signes de la paralysie . . . . . Les malades reprennent leurs forces, on les croit en pleine convalescence, lorsque apparaissent de nouveaux phénomènes morbides (Maingault).

plegia and monoplegia have been seen. I think this may, however, be otherwise explained than on the anæmia theory.

What, then, is the essential nature of post-diphtheritic paralysis? Diphtheria in nearly every case arising in the throat, and paralysis of the palate being, according to Maingault, Jacobi, Gowers, Eade, Hutchinson, Manz, and others, the most frequent paralysis, it was natural at first to refer the palsy to a local condition. And there is no doubt that in some cases in which this affection occurs very early it is due, at least in part, to the infiltration of inflammatory and other products into the muscles. But whether or not it be the case, as Buhl and Eustace Smith consider, that accommodation is at least as frequently affected as the palate, the theory which makes diphtheritic paralysis analogous to that of the bladder and intestine after peritonitis (Maingault) will not suffice. Besides, two important facts must be taken into account—(*a*) the palate is a favourite seat of paralysis in wound-diphtheria, and (*b*) cases are not infrequent in which there is cycloplegia, but in which the palate is not affected till after other parts (*e.g.*, Cases I. and VI.), and some in which it is never affected at all (Case V.) There are five situations in each of which the lesion has been supposed to be situated.

1. *The muscles.*—In favour of this theory there is the fact that degenerative changes have been found in the muscles of various parts of the body, including the heart (Hiller), but of course chiefly in the palate. There are, however, certain objections:—(*a.*) The muscle changes are rather of the type of alterations secondary to nerve lesions. (*b.*) The fact that the muscles react to other influences, such as electrical or therapeutic stimulation; thus the ciliary muscle responds readily to eserine, although eserine acts directly upon the muscle elements (Lauder Brunton). (*c.*) The frequent occurrence (more frequent, probably, than is usually supposed) of sensory paralysis. (*d.*) The uncertain, wandering, widely distributed character of the paralysis.

2. *The nerve terminations.*—I am not aware of any facts distinctly in favour of this theory, but it has been suggested. Against it may be mentioned the fact that pilocarpine, which acts on the nerve terminations (Brunton, Jessop), is capable of readily exciting contraction of the ciliary muscle. For obvious reasons, however, this style of argument must be received with caution.

3. *The nerve trunks.*—Degenerative changes have been seen in the nerve trunks, from anterior root to finest divisions, proportionate to the degree as well as to the extent of the paralysis (Gowers). Buzzard is strongly in favour of the neuritis theory, which Greenhow was among the first to suggest, and considers the affection to be closely analogous to alcoholic paralysis. Benson, in a valuable paper read before the Ophthalmological Society, combats this opinion, pointing out that frequently only one muscle is paralysed out of several supplied by the same nerve, which could hardly be

the case were the cause a neuritis. An even more serious objection is the occurrence, which I have seen, of paralysis of convergence and of pupil movement along with it, while the internal recti retained full power and the pupil acted readily to light (Case IV.) No peripheral lesion can account for this.

4. *The sympathetic system.*—The theory that the lesion lies here arose from a remark made by Hughlings Jackson, that most of the parts affected were connected with the sympathetic system. I am not aware that anything else can be said in its favour, and as Jackson himself pointed out, it will not account for the sensory paralysis.

5. *The brain and spinal cord.*—I think most observers consider the central nervous system to be the seat of the lesion, but opinions are considerably divided as to its nature. Oertel, Déjérine, Abercrombie, Kidd, and others, have found the large nerve cells of the anterior cornua in various states of degeneration, swollen, vacuolated, shrivelled, and granular; some have also found small hæmorrhages in the neighbourhood. Mendel has seen similar changes affecting the ganglion cells of the deep origin of the third nerve. Pierret, Vulpian, and Weber have, however, found no changes whatever.

When he became convinced that the general paralytic condition which he had seen was really due to diphtheria, Trousseau's first idea was that the symptoms were produced by albuminuria. This cannot be, for (*a*) albuminuria is only present in a small proportion of such cases, and (*b*) the nervous symptoms produced by albuminuria or in connexion with that condition are of an entirely different type. Recognising that theory to be untenable, Trousseau then suggested what I take to be the true explanation of the paralysis, viz., that it is produced by a poison, that it is a toxic paralysis. In this connexion we must note two points:—

1. *Spontaneous recovery is the almost invariable rule, a fact which is strongly suggestive of the gradual weakening and final disappearance of a toxic effect.*—Although it is quite true that in some, perhaps in most cases in which a *post-mortem* examination has taken place, degenerative changes have been found, and although it is also quite true that we do not know from what degree of degeneration it is possible for tissues to recover, one may be able to draw a distinction here between a primary alteration of structure and a degeneration produced by the temporary action of a poison. Is it not more probable that the latter may be recovered from *entirely* than the former? And it must be remembered that the fatal cases are—at least such is my impression—in no way different from those which recover; a paralysis which might as well have fixed upon the lower limbs has "happened" to attack the heart with fatal result; the essential nature is the same: the area affected, and therefore the result, different. Kidd's observations led him to the opinion that the lesion was indistinguishable

from that of acute poliomyelitis, but having regard to the vastly different prognosis in that disease, I think there must be some such difference between the two as I have suggested.

2. *Analogies exist.*—Lead poisoning is a not infrequent condition, and is one which presents some interesting analogies, for it shows a peculiar antipathy to a certain set of muscles, and a form of neuritis is the usual lesion found *post-mortem*. It has even been suggested that the neuritis may be preceded by a lesion of the nerve centres which may be entirely "functional." Cases sometimes occur in this country, though they are more frequent abroad, where persons have been poisoned by putrid meat, stale fish, or some poisonous variety of fish. In a good many of these, paralysis of accommodation has been seen, and in some paralysis has been observed to remain for months. Scheby Buch saw five cases of paralysis of accommodation as the sole nervous result of "Wurstvergiftung," in which the condition remained for from 17 days to 7 weeks. Paralysis of accommodation has also been seen in other parasitic diseases, particularly trichinosis.

There being good reason, then, for rejecting all intermediate situations of the lesion, and some ground for not accepting the necessary presence of such central lesions as have been observed, we are obliged to conclude that the condition is produced by a toxic effect of diphtheria or of the cause of diphtheria. Jacobi considers that where the temperature has been high and the original reaction severe, there is less likelihood of paralysis, owing to the more thorough removal of the poison from the system, and *vice versâ*. This is a contention which it would require large statistics to prove, but he may be right. A series of cases observed by Boissarie seems even to suggest that one may have diphtheritic paralysis without diphtheria, for during an epidemic of that disease he saw several examples of paralysis of accommodation, of throat, of limbs, etc., in the persons of those who had not been affected by sore throat. Handfield Jones, in his Lumleian Lectures for 1865, referred to some similar cases.

I am aware that there are objections to the theory of a poison acting in such a way as to produce paralysis in various parts of the body at a distance of some months from the time of its absorption; but there are two diseases which may act in a somewhat analogous fashion, viz., malaria and syphilis. I suppose there is practically no limit to the time at which these two diseases may exert their effects, during the intervals of which, however, the patient seems in perfect health. The other theories, at all events, all break down when closely examined, and this may be the correct one.

In conclusion, I desire very briefly to draw attention to the various alterations seen in the seven cases above related in the functions of convergence, convergence-myosis, and light-myosis. In two cases the three movements were unaffected; in one all were seriously interfered with; in two the pupils acted decidedly sluggishly to light, though the other two movements were unaffected; in one convergence and convergence-myosis were lost, the light reflex remaining normal; and in one convergence-myosis was abolished, though light-myosis and convergence were active. With one exception, then, that of convergence present with the two pupil movements lost, these are examples of all the variations possible, since unless convergence is present one cannot tell whether convergence-myosis is active or not. It would be interesting to know whether these alterations occur in cases in which the accommodation is unaffected; very likely they may be to be found—there is no reason why they should not—but I have not seen such, my investigations being so far confined to patients suffering from cycloplegia.

III.—THE CŒLIAC AFFECTION IN CHILDREN.

By R. A. Gibbons, M.D., M.R.C.P., Physician to the Grosvenor Hospital for Women and Children, Physician for the Diseases of Women and Children at the St George's and St James's Dispensary.

(*Continued from page* 330.)

Treatment.—The only treatment of any avail in this affection is diet. Drugs are of no use without the most scrupulous attention to every detail in the feeding of the children. It has been found by experience that they cannot digest the curd of ordinary cows' milk; in fact, that the milk of ruminants is injurious. The milk of the mare or the ass is excellent on account of the small amount of curd it contains; therefore their milk should be selected. As in this country the ass is the more convenient, it is well to order asses' milk. Of course, human milk in the case of a very young child is desirable. Cream may be given, either with a little water, or, better still, mixed with scalded whey, the latter being made from cows' milk. To give as a drink when the child is thirsty, whey made from cows' milk may be used. For the principal meal the best that can be given is pounded raw meat; beef steak, freed from all skin and fat, cut up finely, pounded in a mortar, and rubbed through a hair sieve, is the way in which it should be given. Of this, from one tablespoonful to four, or even more, depending upon the age of the child, may be given in the middle of the day, or towards evening, and it is most palatable when mixed with a little sifted sugar, or a little plain fruit jelly. A pinch of salt should be added. In addition to the above, plain biscuits or rusks, with plenty of fresh butter, may be allowed. It has been proved by experience that all starchy foods are injurious, and therefore must be forbidden, although it is prudent to order a little floury potato with the meat, as there is an absence of vegetable from the diet. Malted foods are useful, but peptonized foods are really of little or no use, although one would have thought that they would be of great value. In any case in which I have given them a fair trial I have been disappointed, and of late I have never prescribed them. In some cases where the raw meat has not been relished I have used the fluid extracted from the meat by hydrochloric acid, after the ordinary manner, and with excellent results. I consider that mutton is quite as useful as beef, and as with the latter there is a slight risk of tapeworm, some might think it more advisable to order pounded mutton.

Beyond diet, it is important that the child should be thoroughly well clothed, that the whole of the body should be covered with silk or wool, and that he should be kept free from draughts; that he should sleep in a well-aired bedroom, and that he should be placed generally in the best hygienic surroundings. Above all, there should not be the least break in the diet strictly laid down for the child, for, as I have already pointed out, such occurrences are apt to lead to great danger. As to drugs, what I have used in nearly all the cases is bismuth, for attacks of looseness of the bowel; and if the diarrhœa has been watery, the compound kino powder, or compound tincture of camphor. As regular medicine when it could be borne, the tincture of perchloride of iron and cod-liver oil; in some cases, a small dose of the compound decoction of aloes when it has been necessary. Several cases would not stand the latter. In those cases where there have been attacks of watery diarrhœa with persistent vomiting, which occasionally happens, minute doses of mercury with chalk have been useful, and on recovering from these attacks the liquid extract of bark has appeared to be of service.

In a lecture delivered at the Hospital for Sick Children, October 5th, 1887, by Dr Gee, he mentioned a case which did remarkably well by being fed upon the best Dutch mussels. I have had no opportunity of trying this, but the success in the case he mentioned was striking. Changes to the seaside are of no great consequence, as the disease is really influenced by diet only.

The literature upon this question is scanty. A description of it will be found to be given by Cælius Aurelianus,[1] who calls it the "Stomach, or belly passion," and says that it comes from continuous indigestion. I have not seen the variety of stools given in his account of the disease. Aretæus, describing it, says that the cœliac disease "is of a chronic nature from atony of the heat which digests, and refrigeration of the stomach when the food is dissolved in the heat, but the heat does not digest it, nor convert it into its proper chyme, but leaves its work half finished." In neither of these authors is the treatment commendable.

A disease akin to this is mentioned by Sir Joseph Fayrer in his work on *Tropical Dysentery and Chronic Diarrhœa*, 1881, under the name of "Chronic Diarrhœa, Diarrhœa Alba, or White Flux,"

[1] *Cælii Aureliani, de Morbis Acutis et Chronicis*, libris viii. p. 499.

which he considers is one of the most important and most dangerous forms of diarrhœa. He states that it is insidious and slow in progress, and when protracted beyond a certain stage, fatal, owing to irreparable degeneration of the intestinal mucous surface and glands, which causes starvation on account of their inability to absorb nourishment. This form of diarrhœa is known in India as "hill diarrhœa," because it is apt to appear in some parts where malaria, atmospheric changes, and probably water, help to produce it in people who are debilitated by residence in the plains. Sir Joseph Fayrer considers it must be regarded as a climatic disease, in which probably malaria is concerned,[1] and that it appears to be the result more of general degeneration of all than of any special organs. In this affection, however, there are distinct pathological changes. If death occur early, the intestines are found contracted, and the mucous membrane thickened, congested, or ulcerated. If death occur later, the mucous membrane is atrophied, and the seat of fatty or lardaceous degeneration, with frequently ulceration in both ileum and colon, with atrophy also of the glandular structures, mesenteric glands, and mesentery. Dr Manson, quoted by Fayrer, has described a similar disease to the above, which is known in China and Java by the name of "sprue." He considers that it is the result of inflammation of the intestinal mucous membrane, followed by anæmia and general atrophy, and to be probably due to the influence of a tropical climate on Europeans, especially after continued residence in the East.

The foregoing, which I have given at some length, although in many respects similar to the affection described in my paper, is not the same disease. For it is impossible to imagine that should any pathological changes be connected with the disease I have been describing, they would not long ago have been published. Attention was drawn to the cœliac affection by Dr Gee in the lecture above mentioned, and more recently he has published an excellent description of it in the St Bartholomew's Hospital Reports for 1888.

Etiology.—The etiology I have left to the last for consideration. Up to the present no cause has been discovered. It has variously been attributed to teething, to worms, to injudicious feeding, etc., but no good reason has been offered for the occurrence of this obscure disease. It is distinctly not the disease known as chronic diarrhœa of children. It is well known that one of the commonest causes of such diarrhœa is injudicious feeding, where the children suffering from it have been allowed to take "just what the parents have" regardless of age. Moreover, such children are usually living in poor circumstances, possibly crowded into a small, ill-ventilated room, and living in the worst hygienic surroundings, whereas the children suffering from the cœliac affection are met with amongst the rich as well as the poor. The children whose cases I have here brought forward belonged to parents who could afford to bestow luxury upon them if necessary, and who took care that they had every possible attention as regards their rearing long before the disease developed. Moreover, it will be noted that hereditary influence is absent, and in each family such a disease was unheard of before. The only hereditary tendency that I have been able to discern in the majority of the parents of these children, as I have already pointed out, has been gout. It will be seen, therefore, that, with every opportunity for a thorough investigation, I have been unable to definitely name anything as a cause of the onset of this trouble. I have a strong opinion that the origin must be sought for in the liver, or rather the secretions of the liver and the intestines. This is not offered merely for the sake of suggesting a name as a cause, but because, after careful consideration of all the symptoms and signs, it can be readily understood that altered secretions can have the greatest part to play in the signs present. It is well known from experiments that are not recent, that the presence of bile in the intestine is not proved by the colour being bright yellow, for it has been distinctly shown that bile may be present in the fæces, and yet no coloration whatever appear. If it be true—and it is unquestionable—that the bile contains antiseptic properties, then the absence of the constituents of the bile which produce antisepsis can readily account for the decomposition which the fæces undergo in this affection. At any rate, this explanation is a reasonable one, for there can be no doubt whatever that the food is hurried along the intestinal tract, and undergoes decomposition. This explanation is perfectly consistent with all the signs present, for the large motions which have been referred to, and which are as a rule out of all proportion to the amount of food taken, can be accounted for by the fermentation undergone in the alimentary canal. It is possible that some subtle changes take place in the liver, and that, although bile may be secreted as usual, some chemical change occurs which prevents the antiseptic property from coming into play. Although the foregoing theory is reasonable, it is quite possible that the pancreas may have something to do with this affection, and might in part be the cause of the light coloration of the fæces.

---

[1] *Op. cit.*, p. 136.

In a paper read before the Royal Medical and Chirurgical Society in March 1889, Dr Walker related two cases in which during life the motions were colourless, and where after death it was found that the pancreas was diseased, but the liver was, as far as could be made out, healthy, with the bile ducts perfectly free from obstruction. He stated his belief that the ordinary colour of the motions was due to the proper admixture of pancreatic juice and bile under the conditions met with in the intestines. Now, in the case here mentioned, the mere absence of hydro-bilirubin, which is said to be the colouring matter of the fæces, is in itself of no importance, for colourless stools are not uncommon in mere temporary affections, and the absence of colour, as before stated, is now known not to indicate the absence of bile constituents; as far as I am aware, it affords no reason for stating that the fæces are more prone to undergo decomposition. But the chemistry of the liver and its secretion is by no means settled, and, as far as we know, it is possible for certain constituents of bile to be absent; and if this be so, then the chemical combination which produces antisepsis might, in the cœliac affection, be absent. It is well known, of course, that the gastric juice has a considerable antiseptic power, and it is possible that this secretion also is at fault. As to the liver, Tiedemann and Gmelin have shown that the intestinal contents are much more fœtid than usual after the common bile duct has been tied. The experiments of Bidder and Schmidt on animals with artificial biliary fistulæ confirm this observation. It has also been found that the mixture of bile with a fermenting fluid stops or spoils the process of fermentation.[1] But in the disease now before us the absence of bile from the intestine will not explain all. Indeed, from the condition of the patients who suffer from this affection, it would be naturally supposed, remembering certain experiments which have been performed upon the bile ducts, that bile must be present in the fæces in spite of the absence of colouring matter. The importance of bile in the preparation and absorption of fats in the complicated process of digestion is well known. In animals upon whom biliary fistulæ have been established, so that no bile has been allowed to enter the intestines, it is found that they lose flesh and become greatly emaciated. Moreover, it is noticed that they have transparent chyle, and that fat is discharged with the fæces.[2]

In some experiments performed by Dr Legg,[3] he found that after ligature of the bile ducts jaundice appeared on the tenth day, and that the animals wasted, in spite of the appetite remaining good in most of them. He attributes this waste to the interruption of the glycogenic function, for the liver, when tested with iodine, contained little or no glycogen, and no sugar could be found in the watery extract of the liver. When it is remembered how important a factor glycogen is in the nourishment of the body, the waste can be readily understood; but it will be borne in mind that, in the patients under consideration, there was no evidence whatever of loss of flesh in the sense of real emaciation; on the contrary, although the flesh was flabby, there was a fair amount of adipose tissue covering the muscles. In none of them was jaundice present. As clinical experience has proved the fact that clay-coloured motions may occur when there is nothing whatever in the liver or its ducts to account for them, but where there is obstruction to the duct of the pancreas, it is possible that in this cœliac affection there may be, not obstruction to the duct, but some profound inhibition of the secreting apparatus of the gland itself. The secretion of the pancreas commences as soon as food is in the stomach, and is therefore reflex in nature.

---

[1] Carpenter's *Principles of Human Physiology*.

[2] *Op. cit.*, p. 60.

[3] *St Bartholomew's Hospital Reports*, vol. ix., 1873.

The afferent nerve is probably pneumo-gastric, and the centre of reflexion is in the medulla oblongata, the motor nerves running first in the pneumo-gastric, and afterwards in the hepatic, splenic, and superior mesenteric plexuses.[1] The nerves supplying the gland terminate, according to Pflüger, as in the salivary gland, by penetrating the secreting cells. The general relations of this gland to the nervous system are similar to those of the salivary glands, and if the pancreatic secretion be inhibited, it is most probable that the secretion of the small intestine,—succus entericus,—secreted by Brunner's glands and the follicles of Lieberkühn, is also inhibited. In the part of the intestine where the secretion is poured out the process of digestion is completed, and starchy material already unacted on is now converted into saccharine matter, the oleaginous compounds are emulsified and in part saponified, and a solvent action exercised on albuminous material.

It is known by experience in treating those patients suffering from this disease that starchy food is not borne well, and that it invariably does harm, just as in young infants starchy food is not tolerated, because, as we know, the salivary glands are at first functionally inactive. It is therefore reasonable to presume that some mechanism regulating the intestinal juices is at fault. The amylolitic power of the pancreatic juice, resembling as it does that of saliva, is like that of the feebler action of the succus entericus, in all probability, more or less absent, and, together with the change in the chemical constitution of the bile already referred to, would account for the condition of the motions.

It is quite possible that some profound nervous disturbance is at work inhibiting the secretions, more or less, of the liver, pancreas, glands of Brunner, and follicles of Lieberkühn. It may be asserted that this suggestion is a purely hypothetical one. And this I do not for a moment deny, but I should assert in defence of it that it at any rate appears reasonable. For up to the present no post-mortem change, either macroscopically or microscopically, has been made out; whilst we know how common it is for the most profound changes from health to take place without any possible cause for it to be found post-mortem, as in various forms of nervous disease—insanity, for instance. There are strong grounds for believing that some nerves contain secreting fibres—the chorda tympani, for example—which directly influence the gland cells. This fact has, therefore, a most important bearing on the foregoing.

The theory of nervous origin I have suggested would appear to be supported by some examples which may be quoted. For instance, pale stools are known to occur after the use of opium or its alkaloids; in some cases of asthmatic patients, before an attack of asthma; or in connexion with nervous attacks, as epilepsy. In the latter case it is assumed that the inhibition of the formation of pigments by the protoplasm of the liver takes place;[1] and if, under these conditions, there is an inhibition of one constituent, it is quite conceivable that in the disease now under consideration there is the same amount of nervous influence inhibiting the formation of those constituents which form the antiseptic properties of the bile, as well as influencing the other secretions. It is true that the results of experiments as to the influence of the nervous system on the secretion of the liver are contradictory. For, whilst Pflüger[2] states that section of the pneumo-gastric, splanchnics, phrenics, or sympathetics, or by destruction of the cœliac plexus—as long as the blood supply is not interfered with—produces no effect upon the secretion of the liver, at least as to quantity, Röhrig[3] found that section of the spinal cord, or of the splanchnics,—either causing dilatation of the abdominal vessels,—is followed by increased flow of bile, whilst reflex irritation of the cord (as by stimulating a sensory nerve), by contracting the vessels, lessens it. This theory of inhibition might apply either to actually preventing the formation, from the components of the blood, of certain constituents which give antiseptic properties to bile, or, when formed, to keep them stored up in the cells of the liver, instead of allowing them to mingle with the ordinary secretion.

---

[1] Pflüger's *Archiv*, 1878.

Before terminating these remarks, I should like to say a few words on the possible secondary cause of the general condition of these patients, as to the languor, muscular weakness, and occasional attacks of severe diarrhœa from which they suffer. When the state of the intestines, with the amount of decomposing material which is locked up in them, is taken into account, it is hardly to be wondered at that these patients are seriously affected; indeed, it would be surprising, in view of recent discoveries, if they were not so. We do not yet know enough of the animal alkaloids, ptomaines and leucomaines,—the latter word being used by Gautier to indicate their albuminoid origin,—to speak at all positively about the effects of decomposing material;[4] but as it is, of course, evident that absorption must be always taking place from the intestines, it is quite possible that these patients may be in a state of chronic poisoning, and that it may be due to alkaloids, the result of putrid material, taken up from the mucous surface of the bowel. It is hardly conceivable that any child could for long contain so much obnoxious material without being more or less seriously affected by it. In my opinion, the attacks of severe watery diarrhœa to which these patients are liable,—often bringing them to a most prostrate condition, and not unfrequently proving fatal,—are due to the absorption of a larger quantity than usual of these alkaloids, so that in addition to the state of chronic poisoning they have superadded an acute attack. This view is borne out by the evidence that these sudden attacks frequently come on without any apparent cause, and when no possible discoverable alteration in diet, or anything else pertaining to the child, has taken place. Of course, in such attacks as these rapid wasting and emaciation take place, and strength is reduced to a minimum.

---

[1] Med. Chirurgical discussion on Dr Walker's paper, remarks by Dr Thudicum, March 1889.

[2] *Archiv* of 1870.

[3] Stricker's *Jahrbucher*, 1873.

[4] *The Animal Alkaloids: Ptomaines and Leucomaines.* A. M. Brown, M.D., 1887.

In a paper on the "Anæmia or Chlorosis of Girls," read at the Medical Society by Sir Andrew Clark in November 1887, he drew attention to his conclusions that this condition was produced for the most part by what he termed "fæcal poisoning," and he stated that he formed this opinion years ago whilst studying the effects produced upon the body by the undue retention of the excretions, and of the products of their decomposition. Whether one agrees with these conclusions or not in regard to the conditions seen in anæmia, the matter is of great importance, and opens up a wide field for study.

Since Selmi first drew attention to cadaveric alkaloids in 1872, when he communicated to the Academy of Science of Bologna a memoir with his conclusions, Armand Gautier,[1] Brouardel, and Boutmy[2] advanced our knowledge concerning them, and proved that animal tissues in process of putrefaction and decomposition always elaborate certain powerful toxic alkaloids, and they have had reason to suspect that poisonous substances are variably elaborated in animal tissues during life, and that by means of tissue-transformations, or independently and alone, their action is sufficient to determine specified maladies and death.

Bouchard points out in his *Lessons on Auto-Intoxication in Disease*[3] how poisonous the human fæces are. His conclusions are that there are formed in the adult human intestine in twenty-four hours a quantity of alkaloids which would be quite sufficient to destroy life supposing that all excretion were stopped, and that these alkalies were absorbed. If these alkaloids are so potent in health, how much more important must they be in such a disease as that now being considered, where we know that the motions are in a state of advanced decomposition! It is a point of grave import, and it is unquestionable that the absorption of such deleterious matter, which means circulation of those alkaloids in the blood and tissues, must have a serious effect upon the animal economy. It may be said that supposing this absorption takes place, oxygenation should be sufficient to get rid of these alkaloids, for by such processes—continuous combustion by the oxygenation of the blood—are they disposed of.

M. Gautier points out that we resist auto-infection by two modes or mechanisms: by the elimination of the toxic products, and by destruction of oxygenation, and that accumulation may take place where there is an excess of alkaloids with deficient elimination, or deficient elimination alone. In patients suffering from the cœliac affection there is, doubtless, such an excess of alkaloids that they cannot be oxygenated by the blood. Again as, according to my theory, the nervous mechanism of the liver is at fault, it is impossible to say to what extent this disturbance may proceed. But, in all probability, it is in some way unfitted to act as a normal barrier, in the course of the circulation from the intestines, to poisonous substances taken up from the mucous surface of the bowel. Indeed, Dr Lauder Brunton believes that the liver has another function in addition to those assigned to it, viz., that of preventing the digestive ferments from reaching the general circulation so as to act upon the tissues. We do not yet know the rôle played by animal alkaloids formed by bacterial and cellular processes, but it seems unquestionable that they must frequently be the starting point of many diseases.

---

[1] *Chimie appliquée à la physiologie à la pathologie et à l'hygiène*, Paris.

[2] *Ann. d'Hyg. et de Méd. lég.* tome. iv. 3^{me} série, p. 355.

[3] *Leçons sur les Auto-intoxication dans les maladies*, 1887.

The conclusions at which I have arrived regarding the cœliac affection, and which I venture to bring forward in this paper, are:—

1. That as pathological anatomy teaches us nothing regarding it, this disease, for the reasons already adduced, depends upon a functional disturbance of the nervous supply of the liver, pancreas, glands of Brunner, and follicles of Lieberkühn, possibly also on those of the stomach and salivary glands.

2. That as this causes a serious alteration of the digestive process, the food is too readily decomposed, and that the absorption into the blood of deleterious elements produces the profound ill health from which patients attacked by this disease suffer.

IV.—CLINICAL LECTURE ON FLAT-FOOT.[1]

By A. G. Miller, F.R.C.S.E., Lecturer on Clinical Surgery, and Surgeon to the Edinburgh Royal Infirmary.

Gentlemen,—Flat-foot is a very common affection, and there are many various forms in which one sees it manifested in young and in old.

Flat-foot may be variously divided. It may be spoken of as—1. Natural and acquired; 2. Slight and severe; 3. Simple and complicated; 4. Painful and painless; 5. Progressive and complete; 6. Traumatic and idiopathic; 7. True and false, etc. But whilst there are many varieties and degrees of flat-foot, I think there are two principal types to which most of the cases that present themselves for treatment may be referred.

Before describing these types I wish to refer to the terms used above in reference to this disease.

1. *Natural and Acquired.*—By "natural" flat-foot I mean that

---

[1] Delivered in the Royal Infirmary, Edinburgh, on 3rd October 1889, during the Post-Graduate Course. Illustrated by cases, casts, and drawings.

condition of the foot which might perhaps be called congenital, in which the arch of the foot is naturally very low, or entirely awanting, and in which, therefore, the whole of the plantar surface of the foot comes in contact with the ground in progression. This condition is called "splay-foot," and requires no treatment, because it causes no pain or discomfort beyond a certain amount of awkwardness in walking. The term "acquired" would include every other form of flat-foot.

2. *Slight and Severe.*—There might be a great advantage in considering flat-foot under the heads slight and severe. The treatment would be different, the symptoms are different, and the importance of early diagnosis would come under consideration. On this point permit me to observe that it is of very great importance to be able to diagnose and treat properly flat-foot when it is beginning. The slight limp, the turning out of one foot more than the other in walking, the slight groove in the boot or shoe just behind the ball of the great toe near the sole, the feeling of tiredness hardly amounting to pain. These symptoms, if early recognised, and treated with rest, gymnastics, properly made boots and tonics, may quickly disappear, and an unpleasant, painful, and unsightly deformity be prevented.

3. *Simple and Complicated.*—By these terms I would draw a distinction between those cases in which the loss of the plantar arch (or rather arches) is the only deformity and the only disease, and those in which other conditions are present—such as (*a.*) genu valgum; (*b.*) talipes valgus; (*c.*) hallux rigidus; (*d.*) gout in the tarsal joints; (*e.*) varicose veins; (*f.*) strumous disease; (*g.*) rheumatism; (*h.*) the effects of some injury, such as sprain or fracture, etc.

Such a distinction is of importance, especially as regards treatment.

(*a.*) For instance, genu valgum and flat-foot are said to be always associated. This I have not found. In a case of genu valgum, seeing that the leg is abducted from the knee downwards,—in other words, slopes obliquely outwards,—the inner side of the foot comes in contact with the ground principally in walking, or ought to, at any rate. As, however, this deformity (genu valgum) occurs gradually and not suddenly, the foot is educated into adopting a position of inversion that brings the whole sole, and not merely the inner side, in contact with the ground. The proof of this is easily seen after the limb has been straightened, say by Macewen's operation, when the sole of the foot is seen to turn inwards, and it is some time, in aggravated cases, before the patient learns to turn the foot out again, for at first he walks on the outside of the foot. The foot, indeed, is in a condition resembling somewhat talipes varus, and not valgus, as is sometimes erroneously stated. The primary condition in genu valgum is not flat-foot properly speaking, far less talipes valgus, but certainly resembles flat-foot.

I admit, however, that flat-foot and genu valgum may occur together. But it is a mere coincidence, as of any other two affections, which may occur on the same side or opposite sides of the body. If flat-foot is only simulated, the diagnosis is easy, for on careful examination of the foot the existence of a tarsal arch that cannot be opened out is made out at once.

(*b.*) Talipes valgus is sometimes confounded with flat-foot. Even eminent writers do not hesitate to say that the two are virtually or practically the same. The two may exist together, and thus prove their separate identity. Talipes valgus, I take it, consists essentially in a turning outwards of the whole foot, including the os calcis as well as every other bone of the tarsus, and is associated with paralysis. Pes-planus, even when very aggravated, does not affect the astragalus or os calcis materially in the way of eversion, and is not due to paralysis, but is essentially a giving way of the arch of the foot, and is associated with stretching of the ligaments.

(*c.*) Hallux rigidus is sometimes a complication of flat-foot. That it is a result of flat-foot I doubt very much. I cannot enter into an argument on that subject here. I would only say this from a clinical point of view. I have found the rigidity of the great toe disappear to a great extent under anæsthesia. It was due, therefore, to muscular spasm. Muscular spasm is generally associated with joint disease. Pes-planus is not a joint disease, but an affection of the ligaments, and associated with muscular relaxation and atrophy. Hallux rigidus is always associated, sooner or later, with changes in the cartilage and bone of the joint that is made rigid. I am inclined to think, therefore, that the rigidity ought to be the result of the joint disease, and not of the flat-foot directly.

(*d.*) Gout is not unfrequently associated with flat-foot in persons of the "good dinner" old gentleman type. The gout here is a cause of the flat-foot, as I shall afterwards explain.

(*e.*) Varicose veins are often present in cases of flat-foot—probably because they are both due to the same cause, namely, standing. I do not think I would look on either of the two as a serious complication of the other. The presence of varicose veins, however, is apt to mask the flat-foot in some cases and to simulate it in others, because, as is well known, the veins at the inner side of the foot become varicose. In such a case the inside of the foot is swollen, and the normal hollow disappears. In this way flat-foot is simulated, or masked if really present. As regards treatment, I don't think that the presence of varicose veins would have any injurious influence. The application of Martin's bandage, if properly done, would materially help the flat-foot, and the appropriate treatment of the latter would benefit the former.

(*f.*) The co-existence of strumous disease (in tarsus or ankle) with flat-foot would, of course, throw the latter into the shade entirely. And I need therefore say nothing about it beyond this, that I have seen the one mistaken for the other, and have also seen the slighter apparently merge into the graver affection.

(*g*) Rheumatism of the joints of the foot may be mistaken for flat-foot, and *vice versa*,—the latter more frequently. The two affections, of course, may also exist together.

(*h*.) A sprain of the tarsal ligaments may give rise to flat-foot. A sprain of the ankle (internal or external lateral ligaments) may give rise to a condition resembling flat-foot. And Pott's fracture of the fibula, if not properly treated, may leave an amount of eversion of the foot that will give an appearance resembling flat-foot. Such an injury may also leave behind it a weakness of the limb that permits a subsequent falling of the arch of the foot, more especially if the limb is used too early in progression.

4. *Painful and Painless.*—The use of these terms would divide flat-foot, first, into what I have called the natural and acquired forms. Then the latter would have again to be divided, for some forms are more painful than others, and sometimes flat-foot, after being very painful, becomes almost or quite painless when the arch has fairly descended, and there is no longer any strain on the ligaments.

5. *Progressive and Complete.*—Were one to divide flat-foot under these heads it would be for the purpose of treatment,—the former needing every care and attention, the latter admitting of almost no treatment. Indeed, in view of what I said under the former heads, it is a fair question whether a complete flat-foot should be treated at all, unless there is some marked symptom, such as pain, calling for interference.

6. *Traumatic and Idiopathic.*—Under this head I will only say, that sometimes flat-foot may result from some injury, perhaps slight, to the ligaments of the foot, and that these cases, if diagnosed early, ought to be very amenable to treatment.

7. *True and False.*—I have already mentioned several conditions which may be mistaken for flat-foot, and some conditions that are deliberately confounded with it. I will now attempt to describe the principal types of true flat-foot, to which, I think, most cases that present themselves for treatment may be referred.

I will not attempt a description of the pathology of this affection. This has been ably done by Symington and others. Besides, I wish to treat the subject from a clinical point of view, and also to dwell on the treatment specially. I will therefore content myself with defining true flat-foot as mainly a giving way, or straightening out of the arch of the foot, the result of which is, that the patient when walking and leaning his weight on the foot brings the inner as well as the outer side of the foot (more or less) in contact with the ground. The common proof of this, as is well known, is the mark that is left on the floor by the wet foot. In the normal condition one sees the heel, outer side of the foot, and toes only, while in flat-foot the inner side of the foot leaves its mark also.

A. *First Type.*—The *mechanical*, due to carrying heavy weights, etc.

B. *Second Type.*—The *atonic*, due to debility of patient and part.

A. *Mechanical Flat-foot.*—This form of flat-foot is met with in porters and those who carry heavy loads, in cooks and persons who stand much, and in those who are unusually stout and heavy. The flattening of the arch comes on very gradually, as the tarsus is fairly borne down with the superincumbent weight. There is no diseased condition, only deformity. Those affected do not often present themselves for treatment. When they do, it is on account of an aching pain across the instep, or just below the external malleolus. What the former is due to I don't know. But I suppose it is caused by jambing together of the upper surfaces of the tarsal bones. Were the pain due to tension, I would expect to find it on the plantar surface. The pain below the external malleolus is due, I think, to pressure of the malleolus' tip on the os calcis. This occurs when the foot is rotated outwards as well as flattened, and is, as we have seen, not common.

The diagnosis is easily made, the deformity being well advanced before the patient applies for advice.

The treatment is, if the cause cannot be removed, to support the part with some mechanical appliance, such as a pad, or steel spring in the boot, or to throw the weight of the body, in standing or walking, on to the outer side of the foot by a simple arrangement that I heard recommended by Dr Heron Watson long ago, and have found to be very useful. The method is simply to have the sole of the boot made twice as thick on the inner as on the outer side of the foot. To effect this, the patient has only to get a wedge shaped piece of leather put on the inner side of the sole. He should be directed, at the same time, to walk with the foot straight, and to try to lean on the outer side. If it were possible to see a case early, I believe that such a system of walking and shoeing might prevent or even cure the flat-foot.

I have never performed any operation for flat-foot. But I could imagine a case of this type in which, on account of severe pain unrelieved by simpler means, it would be necessary to do something to enable the sufferer to go about and to work. Under such circumstances I think that the removal of the scaphoid, as recommended by Mr Davy (*Lancet*, 6th April 1889), or some other operation which would permit the arch of the foot to fall at once or produce anchylosis of the tarsal joint, and thus relieve tension and pressure, might be appropriate.

Flat-foot may also be due to *habit.* This form of flat-foot occurs in those who stand or walk awkwardly, *i.e.*, on their heels. I have noticed it mostly in adults, and especially in butlers, and those who go about in slippers. Various causes may give rise to the "heel walking." It may be due to mere awkwardness, or to gouty tenderness of the ball of the great toe, or to corns. In any case the result is twofold. From want of exercise the muscles of the leg and especially of the calf atrophy, and consequently the plantar ligaments are not properly reinforced and assisted by the tibials. In consequence these ligaments in time give way. And they do this all the more certainly that they have an undue strain put upon them. In standing or walking, mainly on the heels, the whole weight of the body is imparted to the os calcis, only the posterior tip of which touches the ground. This tends to tilt the bone forwards, which throws a considerable strain on the ligaments uniting it to the other bones of the tarsus. In normal walking and standing, where the weight rests on the metatarso-phalangeal joints as well as the heel, the strain is more equally distributed among all the constituent parts of the foot.

Such persons whose condition I have tried to describe seldom apply for advice unless they suffer from some other affection, such as varicose veins. These two conditions, indeed, are very frequently combined in this class of cases. Many persons have only one foot affected—or one much more than the other. It will generally be found that that one is the favourite one on which they stand. It will also be more turned out in walking.

The treatment should be, if the case is seen early (which unfortunately is not likely), to explain to the patient the cause of the deformity, and how it can be cured, or prevented getting worse, by proper walking. I would advise the person to have his corns cured, or to get rid of his gout, and when standing to lean forward so as to feel the toes on the ground. I would tell him to work his toes and the muscles acting on the foot, and to walk with the foot directed straight forward, and to feel his toes on the ground at each step. Of course such little things, requiring considerable attention till they become a habit, are very distasteful to most people, and they won't take the trouble to practise them. But if so, they must just do as they prefer—and take the consequences.

B. The *Atonic* type of *Flat-foot.*—This is by far the most important form of flat-foot. It is the most painful, the most frequently seen in our hospitals, and the most difficult to treat. Indeed, it is the only form of flat-foot that can truly be called a disease.

The patients are generally young, delicate, atonic, often strumous-looking boys and girls, who have to walk much, carry weights, and are poorly fed. If only one foot is affected it is turned out more than the other in standing and walking. It is carefully saved when the patient is standing, and he will limp on it when walking. There will be pain on the inner side of the foot, just behind and below the tubercle of the scaphoid. The pain is of a dull aching character, and is almost constant, though worse after walking or standing. The patient is usually much depressed and unable to carry on his employment as message boy. The pain is a well-marked symptom, is that for which the patient asks advice, and is due to an inflamed condition of the overtaxed calcaneo-scaphoid ligament. If the disease has gone on for some time and the deformity is well marked, the pain may have left the inner side of the foot and gone to the front or outer side, as in the other form.

The diagnosis is usually easy. The pain on pressing the calcaneo-scaphoid ligament is quite characteristic. The boy or girl looks ill-fed, over-worked, chronically tired, and unhappy. The leg is flabby and thin. The foot is soft and unnaturally plastic. When pressure upwards is made on the ball of the great toe, the foot does not rise as a whole, but becomes flattened—so much so, in a well-marked case, that a pencil or ruler can be laid flat along the inside of the foot touching the skin in its whole length.

In regard to treatment a great deal may be said, but, unfortunately, little can be done to cure this most distressing affection. Still there is some comfort for the patient in the prospect of relief from pain when the tarsus has completely descended, and the ligaments cease to be on the stretch.

I have called this type the "atonic." The treatment consists in the administration of tonics, both to the system and to the part. The best of all, when it can be got, is rest, combined with change of air and healthy exercise. Iron, quinine, and strychnine may be given internally. Gymnastics should be practised to increase the tone and power of the tibial muscles and other supporters of the tarsal ligaments.

We have seen that the calcaneo-scaphoid ligament is the one chiefly affected. I believe that its condition is partly the result of the undue strain put upon it on account of the tibialis posticus failing to support it. In some persons the ligament would slowly give way, and flat-foot would be the gradual result, without pain because without inflammation. But in the class of patients of whom I am speaking at present the strain is more sudden, more severe, or more irritating, and hence the inflammation and the pain. While in the former the mechanical force of weight is the main factor, in the latter the weak and sensitive condition of the plantar ligaments comes into play in the production of the flat-foot.

The first requirement, therefore, in treatment is rest; the next the toning up of the tibialis posticus along with the other muscles and the system generally; and, thirdly, the employment of some apparatus that will help to support the arch of the foot, and perhaps correct the deformity that is forming.

For a time, if possible, the boy or girl should walk as little as possible, and should not stand or carry weights at all. The tone and power of the muscles and system generally should be worked up by massage. Not the skin polishing process that is sometimes employed, but deep kneading of the muscles combined with free movement of all the joints. The patient should be taught to flex, extend, and circumduct the foot, and to practise this exercise frequently every day. The battery also may be employed with benefit. The patient should also be taught to walk on the outside of the foot, and to use all his muscles in walking. Standing on the toes is an excellent exercise, after a time, when it can be done.

For mechanial appliances I would recommend Dr Heron Watson's plan of making the inner side of the sole of the boot thicker than the outer side for slight cases. For more advanced cases a steel support fixed into the inside of the boot, or a pad of cork, may help to make progression more comfortable and natural. Mr Archibald Young, surgical instrument maker, has devised a boot which I have found useful in some cases. It has a strap which passes under the instep, and lifts it up at each step which the wearer takes. In advanced cases I have found no good whatever from mechanical supports while the patient continues his or her previous employment. I am thoroughly convinced that without relief to the over-strained ligament, and toning up of the system and limb, no cure can possibly be effected.

V.—A CASE OF TRAUMATIC EPILEPSY CURED BY TREPHINING.

By CH. FÉRÉ, Physician at Bicêtre. Translated by THOMAS IRELAND, L.R.C.P. & S.E.

(*Extrait des Bulletins et Mémoires de la Société Médicale des Hôpitaux de Paris.*)

THE patient, a man aged 36, was admitted into the Bicêtre on the 30th July 1887, to be treated for epilepsy. There was no hereditary tendency. The patient enjoyed good health until 1870, when he served as a volunteer with Garibaldi. On the 28th November he was struck by a fragment of a bomb, which caused a fracture of the skull. The wound appeared to heal in a few weeks. Six months after, he had his first convulsive attack, with loss of consciousness. No return of the fits appeared till eighteen months after, when he had several attacks of epileptic vertigo. Nevertheless he still continued to lead an adventurous life, serving with the Dutch Colonial troops and in the Bulgarian war. He had been under treatment the first time at the Bicêtre in 1883, but as the attacks were unfrequent he was discharged. He himself said that the only benefit which he received in the hospital was his being deprived of alcohol, the use of which evidently hastened the return of the epileptic attacks. But even while keeping free from any alcoholic excess, he had generally every month two attacks of vertigo, and as many of regular epilepsy. On many occasions after drinking absinthe he had as many as five or six epileptic attacks in the day. The patient showed no abnormality of physical structure. He was vigorous and intelligent, earning a good living as a sculptor in marble, a profession requiring special ability. Nothing could be found except the old wound to which the malady could be attributed. There was on the middle part of the frontal region of his head an irregular cicatrix, showing ramifications in different directions. This cicatrix was almost eight centimetres in length. It showed several points more depressed than the general surface, from which splinters of bone were exfoliated from time to time. A bony fragment had passed during the week before his admission, and the opening which it had made in its passage was not yet completely healed. It was impossible to explore the cicatrix, as on the slightest pressure a fit or attack of epileptic vertigo was induced. The simple contact of the hair in this region produced an exceedingly painful sensation. It was therefore necessary to make a special head-dress to prevent all rubbing. The treatment with bromide was continued for three months without having produced any change in the morbid symptoms. He had five attacks of epilepsy and vertigo during this period. It was then decided to consider what chances trephining gave him. It was remarked at first that the cicatrix was entirely situated in front of a line passing vertically across the skull between the two auditory meati. It was entirely in the frontal region, and at some distance from the psycho-motor centres. It nearly corresponded to the two middle parts of the first frontal convolutions of each side. As the patient stated that the projectile did not penetrate the skull, from the site of the lesions it might be presumed that the convulsions were due to irritation transmitted to the motor centres. Generally the attacks were preceded twelve, twenty-four, sometimes forty-eight hours, by a pain in the region of the cicatrix. When seized, he became pale and fell heavily backwards, screaming loudly, his arms stiffly stretched out along the body, and his legs extended and pressed firmly together. Next clonic movements came on, mostly on the right side. The head was bent to the right. He bit his tongue badly. The attacks lasted about a minute, and were followed by feelings of dejection, but not by stertor or stupor. A dynametric examination was made during the course of several attacks. Normally it gave 55 m. to the right and 47 to the left hand. Immediately after an attack, there was a loss of 19 to the right and 5 to the left, the loss being thus greatest on the side where the clonic convulsions were most severe. After the attack he generally spoke incoherently of matters relating to his wound. The vertigos provoked by irritation of the cicatrix were accompanied by muscular tremors. By applying two myographs to two corresponding muscles of either the arms or legs, or two muscles of limbs of the same side, it was shown that each time the tremors were induced, by pressure on the irritable region, they were exactly synchronous, whether timed in two limbs of the same side or in two corresponding limbs. The reaction presented different forms, but as the movements were simultaneous, it could not be a Jacksonian epilepsy. It was then concluded that a reflex epilepsy was provoked by a peripheral irritation, due to either a lesion of soft parts or of the skull. The case being considered suitable for operation, Mr

Reclus, surgeon at Bicêtre, trephined on the 29th October. When the patient was under chloroform, pressure on the epileptic area did not produce any tremors, and the part was shaved without provoking any reaction. It could be then seen that the seat of the greatest depression in the wound was to the left of the middle line. An incision was made 18 centimetres in length, with the convexity to the left. When the soft parts were detached several points in the skull were seen to be altered in appearance. The trephine was first applied above and within the left frontal eminence at a point where the bone appeared roughened. When the disc of bone was lifted out, nothing notable was seen. Another part was trephined, 3 centimetres behind and a little inside the first, on a depressed spot. The disc removed showed on its inner aspect a projection which appeared to be prolonged backwards. Two other discs of bone being taken from behind the second, allowed the removal of a rough projecting spicule of bone. The opening left in the skull reached to about 1½ centimetres in front of the parieto-occipital suture, and encroached on the middle line, situated very near the motor centres. The dura-mater did not show any alteration in structure. The wound was brought together with four stitches and healed without any trouble. On the fourteenth day after the operation, the patient had a slight attack of giddiness, not resembling at all his former attacks. On the sixteenth day various stimuli applied to the skin on the site of the operation produced no effect. No bad symptoms occurring, he was discharged, and in spite of his becoming intoxicated on getting out of the hospital, by drinking a large quantity of absinthe and two bottles of wine, he still kept well, and could now be considered quite cured. It was thought needless to protect the parts where bone had been removed, as pulsatory movement was almost imperceptible.

VI.—HYPNOTISM, OR PSYCHO-THERAPEUTICS.

By R. W. Felkin, M.D., etc., Lecturer on Diseases of the Tropics and Climatology, Edinburgh.

(*Continued from page* 339.)

It is of extreme importance to understand the action of the eye during hypnotism. As soon as the hypnotic influence commences to affect a subject, the eyes tend to close, and this closure of the eyes is important, because, whether brought about manually or by suggestion, it tends to increase the hypnosis. According to most observers, the pupils become more or less dilated and the eyeballs rotate upwards, but in most cases they probably do not converge. The eyes are not quite, but almost, completely closed. The importance of this remark will be seen presently. A slight tremor is seen in the eyelids, sometimes even a distinct spasm. It must be noted, however, that in some exceptional cases a person may be placed in the deepest hypnotic sleep with the eyes open. In such cases the eyeballs slightly protrude, but it is probable that this occurs only where hypnotism has been induced by fixing the eyes upon a bright object. Some observers, *e.g.*, Moll, found that when patients were hypnotised by suggestion alone the pupils were slightly contracted. Paralysis of accommodation may often be noticed, and the pupil reflex is either abolished, or at any rate considerably lessened. The conjunctivæ are insensible.

With regard to sight, hearing, taste, and smell, there is, in slight hypnosis, little alteration from the normal. In deeper hypnosis the senses may become extremely acute, and in very deep hypnosis they may be abolished altogether. At the same time, during the hypnotic sleep these senses are to a greater or less extent at the command of the operator, and they can be abolished or increased within reasonable limits at his will.

Illusions and hallucinations with regard to these senses may be induced. For instance, suggest to a hypnotised person that he cannot see, and he becomes blind; that he cannot hear, and he becomes deaf. Let him smell the strongest ammonia, and he will not be affected, if you have told him he cannot smell.

Again, the acuteness of the sense of sight is sometimes so great that its range may be doubled as well as the vision being sharpened; and hearing may be rendered so acute that a conversation in the next room may be overheard by the hypnotised subject.

It may here be noticed that common sensation is affected during hypnotism. Very slight variations in temperature can be distinguished in objects held at a distance of about an inch from the body. The sensation of weight may be heightened, so that very slight differences in the weight of various articles can be correctly estimated; and if we test common sensation by means of the points of the compasses, we find that a hypnotised person is able to distinguish the two points at a much slighter distance than under normal circumstances. For instance, according to Berger, a person in whom Weber's compasses applied to the skin in the waking state would require 18 degrees to produce a twofold sensation, when hypnotised would distinguish the points at 3 degrees distance.

With regard to the various sensations of hunger, thirst, joy, sorrow, sickness, etc., they can all be influenced by suggestion, and feeling may be so abolished during the hypnotic sleep that operations may be performed without causing the patient the slightest pain.

The circulation is stimulated if the person is hypnotised by fixing the eyes; if by suggestion, it is not affected; but during the hypnotic sleep the rate of the pulse may be either diminished or increased at the will of the operator. The same remarks apply to the breathing, and respiration may be brought to a complete standstill, but this inhibition is, of course, dangerous if continued longer than half a minute. It is possible, too, to induce an increased temperature in localized areas on the body; a difference of from 2 to 3 degrees Fahr. may be effected at will, and the application of a postage stamp will produce a blister on a hypnotised person should the suggestion be made to him that the blister has been applied. Copious perspiration may be also induced. Bleeding of the nose has been successfully suggested in many cases, as also the production of stigmata.

I must now pass on to consider the psychological alterations which are, or which may be, produced during the hypnotic sleep. First, with regard to memory. In the lighter stages of hypnotism it is not affected, and the person on coming out of the hypnotic sleep remembers everything that has happened. In deeper sleep memory is abolished, unless after awakening a hint be given to the person as to what has happened. For instance, Haidenhain hypnotised his brother. He said to him, "Alles schweige jeder neige ernsten Tönen nun sein Ohr." At first, after awakening, the brother made futile endeavours to recollect, but as soon as the word "schweige" was mentioned he had complete recollection, and reproduced the sentence. In the deepest stage of hypnotism memory is absolutely abolished; but it is most important to remember, that if the subject is hypnotised again, the memory of what has happened at a previous sitting revives. I will subsequently call attention more particularly to this fact; in the meantime, suffice it to say that some individuals who are frequently hypnotised may be said to lead two lives—an ordinary everyday life, and a hypnotic life filled with spectres and fantastic dreams. During the hypnotic sleep the most vivid recollection of long-forgotten scenes or experiences can be induced with comparative ease. Binet and Féré give an instance which displays the acute memory of somnambulists: "A girl in a state of somnambulism was in Charcot's room at the Salpetrière when Parrot, the physician to the Refuge for 'Enfants Assistés,' entered. The subject was asked what was the stranger's name, and she replied, to the surprise of all present and without hesitation, 'M. Parrot.' On awakening, she declared that she did not know him; but after looking at him for a long while, she finally said, 'I think he is a physician at the "Enfants Assistés."' When about two years old she had been for some time in this Refuge, and had long forgotten the physician, whom she now recognised with difficulty in her waking state, while she could during somnambulism give his name at once when ordered to do so."[1]

The writer once had occasion to hypnotise a person aged about 50. She very easily passed into a deep hypnotic sleep, and, to his great surprise, commenced to speak English, although no one present knew that she could understand a word of the language. During her waking state she did not know a single word of

---

[1] *Animal Magnetism*, Binet and Féré, 1887, p. 136.

English, and it could be proved that, at any rate for twenty-five years, she had never spoken it. The only explanation of this incident which I can give is that when very young she must have had some knowledge of the language. This case is exactly on a par with one which Benedikt relates: An English officer was hypnotised by Hansen, and during the hypnosis he suddenly spoke a new language. It subsequently turned out that it was the Wallachian, which the officer had learned as a child and had quite forgotten.[1]

Perhaps the most important point with regard to treatment by hypnotism is the memory of the occurrences during previous hypnotic sittings, for it is by a knowledge of this fact that patients can be trained, and unless it is remembered it may give rise to undoubted errors in experiment. As Moll says, on one occasion he told a hypnotised person to raise the left leg, and as he said this he accidentally took hold of the patient's right hand. On a subsequent occasion, when the patient was hypnotised, he took hold of the hand, and immediately the left leg was raised.

Perhaps one or two other points with regard to memory should be briefly mentioned. By means of suggestions, one can blot out completely the recollection of a certain number of years of the patient's life, or make them believe that they have performed certain actions. This may be termed retro-active suggestion. Or one can, by means of suggestion in hypnotic sleep, produce, say aphasia or agraphia, and it is possible to carry over this influence to the waking state by telling the person that after awakening he will be unable to perform this or that action, or to remember certain circumstances. The previous life of the patient can also be recalled in the most vivid manner either in whole or in part. For instance, if a soldier is hypnotised, some battle he has fought can be brought to his remembrance, and he will act, think, and speak as if on the battlefield. Or, give an old lady a doll, and tell her that she is a child once more, and if you try to take away the plaything, she will cry and call for her mamma. In the same way, it is possible to make persons believe that they are kings, beggars, actors, or anything else one may wish. One can also make them perform actions after awakening—actions which have been suggested to them during the hypnotic sleep. These are the so-called post-hypnotic suggestions. For instance, a person may be told that an hour or two hours after waking from the sleep he will see a certain person; that person will be seen at the appointed time; or that, when the clock strikes a certain hour, he is to perform a certain act, and the act will be performed. But, between the time at which he awakens and the time at which he is either to see or to do something, he will be quite oblivious to the order which has been given him. This certainly does not correspond to anything we experience during our ordinary state of existence. We cannot by any means be certain to remember to perform a certain act at a definite time, nor could we say to ourselves that at a given moment, an hour or two hence, we should see a spectral figure which would be to us as a real one. It must be distinctly understood that there are no means of ascertaining that the patient is in any way otherwise than absolutely himself between the time he awoke from the hypnotic sleep and the performance of the action. It should, however, be stated that cases are occasionally seen in which a person under this post-hypnotic suggestion may be susceptible to other suggestions even in a waking state, until the act he has been ordered to do has been fulfilled; but this will not be the case if the hypnotised person is told during the hypnosis that after waking he is to be perfectly himself.

---

[1] *Der Hypnotismus*, Moll, p. 88.

Moll summarizes these post-hypnotic states as follows, and, as far as I can ascertain, his summary is well borne out by facts:—1. There is a condition in which, during the fulfilment of a suggestion received during sleep, the person falls into a temporary hypnotic condition which is followed by forgetfulness of the act performed. 2. A condition in which there is not the least sign of hypnotism—that is to say, the person is perfectly awake. 3. A condition in which the suggestion had been fulfilled and the act followed by complete forgetfulness, and unaccompanied by new suggestibility. 4. A condition in which the patient is liable to suggestibility and memory. It is sometimes noticeable that, when a person fulfils the required act, the eyes are fixed and the face is expressionless.

I must now refer for a moment to *rapport*. This is, as a matter of fact, only the exaggeration of a normal condition. Just as a person in ordinary life may be attracted to, or readily influenced by, another, so one person may be more readily hypnotised by one operator than by another; and, again, the explanation of the sole influence of an operator upon a patient, to the exclusion of the influence of bystanders, is to be explained by the patient having had the attention fixed upon the operator during the induction of the hypnotic sleep. Now, although at first the patient only sees, hears, and obeys the operator, yet this condition, which we can call by no other term than *rapport*, can be conveyed to a bystander, should the operator wish it, and convey the suggestion to the patient. This may be illustrated by supposing that a cataleptic fixation of the arm is produced by the operator in a patient. No bystander will be able to put an end to such a contracture by a fresh excitement of the same nature, which seems to prove that such contractures cannot be brought about without psychical action on the part of the patient.

In all probability one is justified in stating that there is rarely any total loss of consciousness during hypnotism. At the same time, there can be no doubt that the will power of the person hypnotised is diminished. It is, however, quite possible for a hypnotised person to fight against the suggestions of the operator, and this takes places the more readily if anything is suggested which is out of harmony with the normal character of the subject. It has been found extremely difficult to influence persons to do anything which they may consider wrong during a hypnotic sleep, and I believe that I am justified in saying that in the vast majority of cases moral sense becomes more acute, more sensitive, during the hypnotic sleep than during the waking hours. Beaumis relates cases in which hysterical convulsions have occurred when actions have been suggested which the patient did not approve of, and Moll says that he has known persons demand to be awoke when he has made suggestions to them which they did not appreciate. Pitres mentions that once, having suggested to a girl that she would be unable to speak after he had awoke her, she refused to awake until he withdrew the command. It is also found in some cases, where the hypnotised individuals apparently consent to suggestions to be performed after awakening, that they subsequently successfully withstand the impulse to perform an act inconvenient to them; and it is in such cases that it is necessary to choose some distinct external suggestion in order to attain obedience in an unwilling subject.

Before leaving the symptoms of hypnotism, which it will be seen are of an extremely intricate character, it may be well to give a very brief summary of the symptoms which Charcot and his school attribute to the three stages or degrees into which hypnotism is divided by them. I quote from Von Schrenck-Notzing:—

1. *Catalepsy.*—Produced by a strong sensory stimulant, *e.g.*, a sudden intense noise, or the sudden exhibition of an intense light.

Symptoms.—Immobility, flexibilitas cerea. If the arm be stretched out horizontally it will remain in that position for a certain time without alteration in respiration. After a varying period it will fall to the side. Pressure upon muscles, tendons, or nerves relaxes the muscles affected. Paradoxical contraction is lessened, and appears slowly.[1] Tendon reflexes are weakened; respiration is slowed; the magnet is without influence; and apnœa is frequently seen. There is anæsthesia of the skin and ovaries; the senses are blunted. With regard to the circulation, the peripheral vessels are contracted. The eyes are open, and have an immovable staring gaze. Verbal suggestions have little effect; on the other hand, suggestions may be conveyed through the muscles.

2. *Lethargy.*—Produced by fixing the eyes upon a moderately bright object.

---

[1] A paradoxical contraction is, according to Westphal, "when a sudden and energetic movement of dorsal flexion is, for instance, given to the foot. The anterior tibial muscle contracts so as to produce adduction and a certain degree of dorsal flexure of the foot, which remains fixed in this position." Erlemeyer thinks that *contracture by antagonistic distension* should be substituted for the expression *paradoxical contraction.*

Symptoms.—The muscles are perfectly relaxed. A condition of neuro-muscular hyper-excitability obtains. Mechanical irritation of muscles, nerves, and tendons produces a rapid contraction of the corresponding muscles. The tendon reflexes are increased; the paradoxical contraction is more rapid. Susceptibility to æsthesiogenic irritation is increased, especially to magnets. There is hyperæsthesia of the ovaries and hearing, as well as analgesia. The respiration is deep and more rapid. With regard to the circulation, the peripheral vessels are dilated. The eyes are nearly closed, and the eyelids tremble. Mental dulness is, as a rule, complete; in exceptional cases, suggestions or suggestive hallucinations may be received.

3. *Somnambulism.*—Induced by regular, continuous, feeble sensory stimuli, or else by the suggestion of sleep.

Symptoms.—The muscular condition is almost normal. Gentle irritation of the skin, such as blowing on it and stroking it, causes the group of muscles under the skin to contract. The same irritation will cause the muscles to relax again. Mechanical irritation easily produces general muscular rigidity, which does not give way to the mechanical irritation of the antagonistic muscles. Tendon reflexes normal; analgesia sometimes present. The senses are mostly sharpened, and the receptibility of sensory impressions is heightened. The eyes are half open; the lids vibrate. Consciousness and mental activity are clouded. Suggestions easily produce results, but opposition to suggestion may be present.

These three stages may be made to pass the one into the other. If the operator closes the eyes of a person in the cataleptic or the somnambulistic state, the lethargic state will be produced. By the operator opening the eyes, the lethargic state may be changed into the cataleptic condition. Rubbing the vertex in cataleptic or lethargic stages will produce somnambulism.[1]

To avoid confusion, as we have referred to Charcot's three stages, and tabulated the symptoms which he attributes to them, it will probably be of advantage to tabulate the degrees of hypnotic sleep and their symptoms as specified by the two great exponents at the Nancy School. Liebault distinguishes six stages of hypnotism, as follows:—

Degree 1.—*Symptoms.*—Drowsiness; the eyelids feel heavy; it is usually impossible for the patient to open the eyes; a feeling of lassitude is experienced; consciousness is absolutely unaffected. This stage is almost invariably seen, especially in women.

Degree 2.—*Suggestive Catalepsy* (*Hypotaxis*). *Symptoms.*—Consciousness retained; if a limb be placed in a certain position, it is retained there for a few seconds and then tremulously sinks; the fingers do not retain a position which may be given them; the eyelids are closed; the limbs are flaccid; the patient's relation with his surroundings is uninfluenced; memory of what has happened is perfect on awakening.

---

[1] *Ein Beitrag zur therapeutischen Verwerthung des Hypnotismus*, Von. Schrenck-Notzing, Leipsic, 1888, pp. 5, 6.

Degree 3.—*Symptoms.*—Movements suggested to the patient are automatically continued, such as rotating the arm; contracture of muscles, if suggested, takes place; sensibility is diminished; consciousness and memory are retained. Most patients assert that they have never slept, and say that they have obeyed the operator in order to please him.

Degree 4.—*Symptoms.*—The hypnotised person is *en rapport* with the hypnotiser alone. He is absolutely shut off from the influence of others, unless the operator transfers the *rapport.* ·Other symptoms are present as in the third degree. Consciousness and memory are retained.

Degree 5.—*Light Somnambulism. Symptoms.*—Diminished or completely abolished sensibility; consciousness clouded; memory uncertain and indistinct; suggestive hallucinations are possible. Other symptoms as in the fourth degree.

Degree 6.—*Deep Somnambulism. Symptoms.* — Consciousness abolished; complete amnesia after awakening. The symptoms mentioned in the fifth degree are all more strongly marked.

Bernheim gives a slightly different arrangement of the degrees of hypnotism, making nine degrees distinguishable.

A. *Degrees* 1 *to* 6.—After awakening, the patient remembers everything that has happened.

1*st Degree.*—*Symptoms.*—The patient is susceptible to certain suggestions, namely, to the sensation of warmth in different parts of the body or to the abolition of pain. None of the symptoms mentioned above are present. The patients assert confidently that they have never slept.

2*nd Degree.*—*Symptoms.*—The patients are unable to open their eyes spontaneously. Otherwise negative symptoms.

3*rd Degree.*—*Symptoms.*—Suggestive catalepsy is present. The patient has, however, the power of consciously arresting the suggestions.

4*th Degree.*—*Symptoms.*—Suggestive catalepsy is present, the patient being unable to withstand suggestions made to him unless they are neutralized by the operator. Automatic movements suggested by the operator are usually obeyed.

5*th Degree.*—*Symptoms.*—Suggestive contracture may be induced.

6*th Degree.*—*Symptoms.*—Automatic obedience. In this stage the hypnotised person can only walk with difficulty. It is impossible to induce either hallucinations or illusions.

B. *Degrees* 7 *to* 9.—Somnambulism. Amnesia subsequent to awakening.

7*th Degree.*—*Symptoms.*—All those mentioned in the other degrees are present, but it is impossible to induce hallucinations.

8*th Degree.*—*Symptoms.*—Hallucinations may be produced.

9th Degree.—Symptoms.—The patient is subject to hallucinations and also to post-hypnotic hallucinations.

More or less analgesia is usually present in all the degrees, but it is most frequent in the last three.

The next point we have to examine is one of no little importance, namely, the investigation as to whether a person who is professedly in a hypnotic sleep is really hypnotised, or whether he is simulating. With reference to this question, it is to be hoped that our former remarks will be borne in mind, and that investigators will not jump at the conclusion that it is by any means easy always to arrive at a right conclusion. When we remember that some of the best intellects of Europe have sometimes been deceived, it is only to be expected that novices should be either too credulous, or, what is just as bad, too bigoted or sceptical. I will as briefly as possible call attention to the points chiefly worthy of notice.

In the first place, the behaviour of the eye should be carefully watched. It is not easy to describe on paper its behaviour. The eyelids gradually sink or close as the hypnotic influence commences to be felt. The action of the frontalis should also be observed. It acts as it does when the eyes of a sleeping person open. The convulsive rotation upwards of the eyes should be noted, but the tremor of the eyelids has no value. In cases where the eyes are open during the hypnotic sleep, attention should be paid to their expression. A peculiar cold and blank glance will be seen. The expression of the face is like that of a mask, and the *tout ensemble* of the person is characteristic. In most cases the movement of the limbs is remarkable for its sluggishness, as if they were heavy. Some few exceptions to this rule are found in persons who are only very slightly hypnotised. It is most important to notice the expression of countenance of the person when various suggestions are made to him. In a moment his face will change from the expression of anger to one of joy, or from a look of scorn to one of approval. Indeed, this almost instantaneous change of expression is usually thought by the ignorant to be a proof of fraud. Again, the expression of wonder and astonishment when the person awakes should receive attention, and then, of course, his action in regard to post-hypnotic suggestions must be carefully studied. It is by no means an easy matter for all these points to be simulated, either consciously or unconsciously. The person trying to cheat usually over-acts his part, and it will be noticed that those persons who are imposing usually obey each suggestion with extreme rapidity, whilst experience teaches that in real cases of hypnotism the speed with which a person obeys the operator increases gradually,—at first the commands being obeyed slowly, almost, as it were, with a certain amount of reluctance, but subsequently with great rapidity. Most impostors are very fond of pretending insensibility to slightly painful impressions, because they erroneously believe it to be a very common symptom of hypnotism.

(*To be continued.*)

VII.—MALIGNANT DISEASE OF THE LARYNX—A SEQUEL.

By G. HUNTER MACKENZIE, M.D., Laryngist to the Eye, Ear, and Throat Infirmary of Edinburgh; Corresponding Member of the Société de Médecine Pratique of Paris.

IN the number of this Journal for July 1888, I published a short paper entitled "Cases of Endo-Laryngeal Removal of Growths from the Vocal Cords." It is the second, and, as stated in that paper, the most important and interesting case of the series, whose farther history I now proceed to relate.

This, it may be remembered, was the case of a patient whose right vocal cord (not the left, as stated by error in one part of the paper) "had a nodular thickening on its anterior third, and from its thickened posterior third there sprang a sessile, fleshy-looking growth which projected into the glottis" (Fig. 1, reproduced from previous paper). This was removed in August 1887 by laryngeal forceps, and two smaller growths which recurred were similarly treated three months subsequently. The result of these operations was that the laryngeal symptoms almost entirely disappeared.

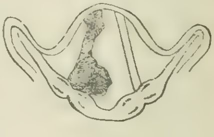


Fig 1

The growths removed were examined by Dr Wolfenden, of London, and were found to be typically epitheliomatous (see drawing of microscopical appearances in previous paper).

Subsequent History of Case.—From the date of the second operation (November 1887) all through that winter, and also during the following summer, the patient was apparently very well—so well, indeed, that he, and more particularly some of his relatives, declined to accept my diagnosis of malignancy, or agree to the operation of a partial laryngectomy, which, as stated in my previous paper, I proposed. In the autumn of 1888, however, throat symptoms commenced again to develop, and from a rough sketch of the laryngoscopic appearances at that time, kindly supplied to me by Dr Stewart of Uphall, his medical adviser (Fig. 2), it will be seen that a certain amount of recurrence had again taken place. The patient, now anxious about his condition, commenced to cast about for a favourable opinion, and he had neither to wait long nor go far to obtain this. He consulted another laryngologist in Edinburgh, who gave a favourable prognosis, and fortified it by the views of a surgeon in Glasgow, to whom he sent the patient for examination. All now appeared to be going on most satisfactorily, for in response to farther advice of mine communicated to Dr Stewart, that the opinion of a London specialist should be taken, I was informed that the patient had been so comforted by the aforesaid Edin-

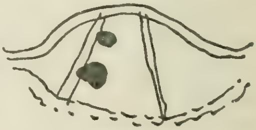


Fig. 2.

burgh and Glasgow favourable opinions, that his mind was now at ease, and it was resolved to pay no farther attention to the matter. This was not to be, however, for though his throat kept fairly well throughout the winter, early in the following spring the disease broke out afresh, spread rapidly over the larynx, and it became evident to the merest tyro in laryngoscopic work that cancer was thoroughly established, and that no operation short of complete extirpation of the larynx would have any chance of eradicating it. Professor Chiene was accordingly now called in, and by him this operation was successfully performed. The patient lives, voiceless.

Remarks.—This case tells its own story sufficiently clearly, and there are probably only two points on which remarks are called for. These are the diagnosis and the treatment.

The microscopical examination of the growths removed by me showed that they were undoubtedly epitheliomatous, and this affirmative sign was of the highest possible value in diagnosis. Whilst, as is now well known, portions of a malignant growth may present benign microscopical characters, malignancy under the microscope is *always* indicative of a corresponding character in the neoplasm from which the preparations were taken. A negative result in a case may be therefore comparatively worthless; an affirmative one, on the other hand, cannot be over-estimated.

But in addition to the microscopical test, there was one clinical feature which I now consider of diagnostic importance. I allude to the impaired mobility of the affected vocal cord (the precise words are, "the mobility of the vocal cord was noted as being slightly impaired"). I had previously directed attention to this sign as being of value in the differential diagnosis of benign and malignant growths in the larynx (see "Case of Thyrotomy for Recurrent Growths in the Larynx," *Edin. Med. Journal*, Decr. 1887), and I have since had occasion to observe its presence not only when the disease appears as a growth, but also when it assumes other initial forms, such as an ulcer. In its very early stage, and in its manner of development, this paresis presents such characters as distinguish it from all other forms of laryngeal paralysis, but the appearances are at first so very slight as to be capable of detection by experienced observers only. This point I propose to make the subject of another communication. Meanwhile I wish to emphasise its value in the diagnosis of malignant disease of the larynx in its early stage—the period when it is of the utmost importance to diagnose accurately.

Would a partial extirpation of the larynx, as proposed by me, have saved the patient? There is every reason to believe that it would not only have done so, *but would also have preserved his voice.* The case was pre-eminently suitable for this operation, for not only was the disease intra-laryngeal, but during my period of observation, and for 8 or 10 months thereafter, it was strictly limited to one side. The most successful case of complete or partial extirpation of the larynx in this country, *i.e.*, where the patient has lived longest after the operation, is that of the well-known London barrister, one-half of whose larynx was removed on account of unilateral malignant disease by Hahn of Berlin some three or four years ago. Not only is he still alive and well, but his voice is sufficiently good to enable him efficiently to discharge his duties as a metropolitan police magistrate. This case may be justly claimed as a triumph of laryngology, for it was owing to the early recognition of the disease by an eminent London laryngologist that the operation was timeously performed.

In the present case, when the eminent surgeon to whom the patient so far owes his life was called in, the disease had spread to such an extent as to leave no operative alternative but to sweep away the whole larynx.

Part Second.

REVIEWS.

Lectures on Ectopic Pregnancy and Pelvic Hæmatocele. By LAWSON TAIT, F.R.C.S., LL.D. Birmingham: The Journal Printing Works, New Street: 1888.

The Diagnosis and Treatment of Extra-uterine Pregnancy. By JOHN STRACHAN, M.D., etc., Belfast. Being the Jenks Prize Essay of the College of Physicians of Philadelphia. Philadelphia: P. Blakiston Sons & Co., 1012 Walnut Street: 1889.

WITHIN recent years the fact has come to be recognised that extra-uterine pregnancy occurs with much greater frequency than had been supposed, and that many, if not all cases of severe or fatal hæmatocele owe their origin to the accidents attending this condition. As bearing on this question we notice a statement made by the coroner's physician for Philadelphia, who says that during the past five years he has had occasion to observe nineteen cases of this condition, all cases of sudden death with histories unknown, and that he has reason to believe that more cases occurred but escaped notice. This represents a very large mortality when we consider that these were only the coroner's cases. Another indication of the frequency of the condition is afforded by the table of operations contained in the first of the books before us, where we see that the author has been operating about eight times a year for the past five years on cases where rupture has occurred with the formation of hæmatocele. To this table we will return later on, merely remarking in passing, that all, or nearly all those operation cases or ante-mortem findings would have become post-mortem ones had it not been for the skilful intervention of the surgeon.

While thus the profession has underestimated the frequency of this occurrence, there have also been entertained the vaguest and haziest notions of its varieties and of the clinical history, not only of each case, but of classes of cases. Faulty observations, and the failure to recognise that one kind of case might pass on to become another, have greatly obscured the whole subject. We venture to say, however, that Mr Lawson Tait's observations and deductions as laid down in his work bring the whole question out of the tangled and uncertain mazes of theory into the clear atmosphere of fact and practice. The keynote to Mr Tait's conclusions and to the course of practice he bases upon them is his assertion that all extra-uterine pregnancies are, so far as our present knowledge shows, in the first instance, tubal, and that the further history of each case depends upon the direction in which "primary" rupture occurs. In the tubo-uterine and in a large proportion of the tubal cases this rupture occurs direct into the abdominal cavity, and all such cases are (we are following Mr Tait's assertions) uniformly fatal unless removed by abdominal section. The table of forty-two cases to which we have referred contains such cases upon which he has operated up to date of publication. As a type of such a case we take one—a multipara whose last child is some years old—who is not conscious of being pregnant, and who suddenly presents all the symptoms of severe hæmorrhage and abdominal collapse. On abdominal section being performed it is found that the peritoneal cavity is filled with imperfectly coagulated blood, and that the blood is coming from a rent on the free surface of one of the Fallopian tubes. The tube is ligatured and removed, toilette of the peritoneum carried out, and a life saved. Of the forty-two cases only one died, while if left alone all would have succumbed. Surely this is a veritable triumphal advance in modern surgery. For be it remarked, there is no need to hesitate over the diagnosis further than to be assured of the existence of collapse dependent on internal pelvic hæmorrhage, by no means a difficult matter.

The importance of this matter for general practitioners is insisted upon by the writer of the second work before us, who very rightly points out that this is an operation for the general practitioner even more than is that for strangulated hernia, that it is probably not nearly so difficult, and that the results are fully better.

After putting aside, however, the cases which we have just considered where rupture occurs on the free surface of the tube, there remain many cases which do not end so abruptly, or which go on to term, and these Tait explains by showing that rupture may occur through that portion of the tube which is not covered by peritoneum, and which lies between the two layers of the broad ligament, and that here the tendency to unlimited hæmorrhage is checked by the assistance of the broad ligament layers, thus in many cases allowing of further growth of the fœtus and gradual formation of a new sac, which stretches but may never burst.

Space, however, forbids our following the author further, but we trust that we have said enough to indicate the enormous importance to the practitioner of his having a clear understanding of the ways of extra-uterine pregnancies, and we would urge upon all whose work may bring them in contact with such cases the imperative necessity of their doing so in the light of such results as those of Mr Tait. What would be said of the man who confined his treatment of strangulated hernia to poultices and opium ? and what will be said in the immediate future of the man who will confine his treatment of abdominal hæmorrhage from extra-uterine pregnancy to ergot and brandy ?

In conclusion, while we would most strongly urge the perusal of these two works on all general practitioners, we would say that while Mr Tait's is, as might be expected, strongly individual and somewhat dogmatic in its tenor, Dr Strachan's is fuller and more complete (including, as it does, the most of Mr Tait's), and the subject is treated from all sides, and in an eminently impartial manner. In fact, Dr Strachan has relieved all reviewers of the task of criticising Mr Tait's lectures, as he has so fully and completely done so in his essay.

Papers on Dermatology. By E. D. MAPOTHER, M.D., Ex-President Royal College of Surgeons in Ireland; Consulting Surgeon to St Vincent's and St Joseph's Children's Hospitals, Dublin, etc., etc. London: J. & A. Churchill: 1889.

THESE papers of Dr Mapother's show evidence of rather extensive reading, and are written in an easy and attractive style, but at the same time must be looked on more in the light of sketchy outlines than as solid contributions to medical science. Perhaps the one of greatest practical value is the first, on the vascularity of the skin, and the communications of subcutaneous with visceral veins in relation particularly to topical blood-letting. The practice of leeching has—possibly too completely—fallen out of use, yet there are cases where individual organs may be diseased, in which the hints given by Dr Mapother as to the spots on the surface from whence blood should be drawn in order to influence deeper lying parts might prove of advantage. In another article on the correlations of eczema and gout, an attempt is made to prove that all forms of eczema are due to excess or retention of uric acid, that the gouty poison is the cause of eczema. With this we are by no means prepared to agree. No single disturbance of the balance of nutrition can be in all cases the predisposing, as no one irritant is the exciting cause of eczema. Gout, especially when masked, irregular, or ill-defined, makes eczema more intractable, but the large majority of those affected with eczema are in no sense gouty, and anti-arthritic remedies fail signally, *per se*, to cure it. The successful treatment of eczema consists, not in any routine procedure, or on the employment of any specific, either external or internal, but in the management of each case on its merits. The concluding paper contains an interesting account of the Irish sulphur spas of Lucan and Lisdoonvarna, both situated in the midst of picturesque scenery, and the former within easy distance of Dublin; yet so far as we can gather from its perusal, the quantity available at present at either is much too small to admit of the water being used for baths, and the curative effects of sulphur springs in cutaneous affections when taken internally alone are, to say the least, questionable.

Applied Anatomy: Surgical, Medical, and Operative. By JOHN M'LACHLAN, M.D., B.Sc., M.R.C.S., etc. Illustrated with about 230 Engravings. Third edition. 2 vols. Edinburgh: E. & S. Livingstone: 1889.

IF the medical student of to-day comes to an examination unprepared, and retires without his diploma, he certainly cannot lay the blame of his failure on want of instruction.

With systematic courses of lectures constantly increasing in number and severity, with tutorial classes, with extra fees and extra hours, to assist him in assimilating the indigesta moles that is spooned into his unlucky brain, by the constant yearly deciphering of ancient manuscripts, with text-books to be mastered which contain not only all that is really known on any subject, but also all the facts which the respective authors have evolved from their own conceit, and therefore believed to be valuable,—no wonder the poor victim appears at the examination stuffed, not taught, with material crammed, not digested. To help him in his difficulties, to guide him in the waste wilderness of his poor lecture-room notes, and to give him a chance of learning something, the student has to find for himself men that can really teach him, and books that convey useful information,—information that will bear not only on the work of the examination, but also on his future professional life. Dr M'Lachlan is such a teacher, and his book is of the kind we mean. It is a successful attempt to show how the facts of Anatomy are necessary as a groundwork both of Surgery and Medicine. If mastered it will do the student more good than acres of MSS. dealing with theory and fad, for it makes his anatomy interesting, and the bedside or the operating theatre intelligible, and, except where vested interests in fees come in, it is pretty generally acknowledged now that the dissecting-room, the laboratory, and the hospital are the real places of study. One caution we may give the author. Let him try to keep down the size of his book; for mere fulness of detail may be overdone; brevity and accuracy should be his aim, but to write briefly is not so easy as it seems.

The Cerebral Palsies of Children. By WILLIAM OSLER, M.D. London: H. K. Lewis: 1889.

THE five lectures which are collected in this small book were noticed in a recent periscope of the diseases of children, and we are glad to see them in a collected form. A great number of cases, partly from the author's personal experience, partly from general medical literature, are collected together and made the basis of a valuable contribution to our knowledge of the brain diseases and injuries of childhood. The chapters deal with Infantile Hemiplegia, Bilateral Spastic Hemiplegia, Spastic Paraplegia, together with their pathology, diagnosis, and treatment. We can heartily recommend it to all those who have to do with the diseases of children.

The Canaries for Consumptives. By E. PAGET THURSTAN, M.D., B.A. London: W. H. Allen & Co: 1889.

THIS little volume is the result of a recent visit to Tenerife and Gran Canaria. Information regarding the islands themselves, the modes of access, the social life, attractions, conveniences, and drawbacks, is given in a pleasant, gossipy fashion. The author, wiser than some of his brethren who write such books, refrains from giving records of cases, but indicates the kind of patients who, in his belief, would be benefited by a residence in the Canaries. In addition to consumptives, sufferers from Bright's disease, chronic bronchitis, and rheumatic gout ought to do well. The excellence of the climate he attributes to the proximity of the African continent. The Canaries have thus the equability of an island climate combined with the dryness of a continental one. The volume is one which may safely be put in the hands of rich consumptives.

The Retrospect of Medicine. Edited by JAMES BRAITHWAITE, M.D. Vol. XCIX. Jan.–June 1889. London: Simpkin, Marshall, & Co.

THE present volume, like its predecessors, gives a most excellent *résumé* of the progress made in the Medical Sciences during the first half of the present year. It is a work which the inquiring practitioner will find of the utmost value.

Tooth Extraction. By JOHN GORHAM, M.R.C.S., etc., Third edition. London: H. K. Lewis: 1889.

THAT this little book has entered upon a third edition is not surprising. It is eminently practical, very concise, and contains about all the information required upon the subject. The works generally published upon Dentistry in the new aspect it has assumed as a special department of Surgery are in many cases far too lengthy, their bulk being frequently due to a repetition in their pages of what might be more advantageously studied in the ordinary manuals on general Medicine and Surgery. The volume under review sticks to its subject, and, with several other small works recently appearing on this branch, will probably be found more practically useful than the large "Systems" ever and again published upon Dental Surgery.

Annual Report of the Royal Edinburgh Asylum for the Insane for the Year 1888.

Annual Report of Crichton Royal Institution, Dumfries, Year 1888.

Report of the Derbyshire County Lunatic Asylum, 1889.

Cumberland and Westmoreland Lunatic Asylum Annual Report for the Year 1888.

Newcastle-upon-Tyne City Lunatic Asylum Annual Report, 1888.

NOT only does Dr Clouston present the statistics of the Morningside Asylum in a lucid and striking form, but he actually makes his report entertaining, by his pithy observations about mental disease, and his instructive comments upon events therewith associated, such as the recent suicide of the Crown Prince of Austria, the Whitechapel murders, and the Lunacy Amendment Bill.

In the beginning of the year the number of patients in the Morningside Asylum was 806, and during the year there were admitted 348, of whom 172 were men and 176 women. The total number of patients under treatment was 1154. There were discharged during the year 249, of whom 122 were men and 127 women.

The cost of pauper boarders during the year was about £31, and that of the intermediate class of boarders a little above £40 a year.

The recovery-rate was 32·1 per cent. on the admissions. The death-rate was 9·6 per cent. on the average numbers resident, and 6·8 on the total numbers under treatment. Dr Clouston explains the causes which have made his recovery-rate lower and his death-rate higher during 1888. Of course every superintendent has a right to appeal from the fluctuations of one year to the average of several years.

Dr Clouston remarks that "the English Commissioners in Lunacy have some very important observations on a table in their last Report (for 1887), showing the percentage of recoveries each quinquenniad since 1854 in England. They say, 'It would appear that the recovery-rate fell between 1854 and 1868, but that since the latter year there has been a gradual though slight improvement. Probably the improvement is somewhat greater than the figures show, as we think some allowance may fairly be made for the generally less favourable character of the cases of insanity which, as before mentioned, have been brought under curative treatment. We are unable, however, to regard the results obtained as altogether satisfactory, and think that a larger percentage of cures should be possible.' My experience is also strongly in favour of the idea that many more cases than formerly of mental disturbance from old age, from paralysis, and from general breakdown, that formerly would not have been reckoned insanity at all, now come within that category, and are sent to asylums from poorhouses and from their homes, not with the view of cure, for they are mostly incurable, but to save trouble and get rid of them. This must, of course, lessen the recovery-rate. And I also agree that we should not be satisfied with our present recovery-rate."

Dr Clouston claims to have anticipated the suggestions of the English Commissioners, that asylums should have more the equipment of an hospital for the sick in the special wards prepared at Morningside for newly-admitted patients.

The question, with all the money spent and all the new appliances, Are there more cures in insanity? should assuredly never be lost sight of. One might expect, from the tone sometimes assumed, that the superiority of British asylums over those of our neighbours on the Continent and in America could be justified by a higher rate of recovery and a lower death-rate; and unless this can be done, and we do not think it can, we regard mere appearances—decorative papering and painting and upholstery—as of comparatively trifling importance. The efficiency that cannot cure and cannot save life is not the kind one should sit down contented with either in an hospital or an asylum, and the increased efficiency with increased death-rates is failure which superficial observers may perhaps mistake for success. In the present time it is needful that we should go deeper than appearances, but this is a test which Dr Clouston can well stand. During all his asylum experiences he has paid a steady and intelligent attention to the medical aspects of his work.

We have now only room for Dr Clouston's telling criticism on the new English Lunacy Bill, which has already passed a second reading, and is now referred to the Standing Law Committee.[1] Its provisions, he observes, "are drawn up too much from the supposed liberty of the subject point of view, and too little from the treatment of the patient aspect. They are too much in the spirit of the English criminal law, encouraging and almost suggesting to the insane man, whose curtailment of liberty is necessary for the treatment of his disease, to put himself in a fighting attitude, and appeal to the magistrate, instead of trying to resign himself to what is good for him. One most objectionable provision I am quite sure will never work well in practice, and that is the one which gives the committing magistrate the option of a personal interview with the patient, and gives the former the power against, it may be, the strongest testimony of the nearest relations and of the two medical men, of refusing to grant the order on his own personal impressions of the case. Fancy a non-medical magistrate going to visit a case of child-bed insanity, and having the power to decide that the measures which those nearest the patient and the doctors in attendance thought the best were not necessary!"

---

[1] It appears from a copy of the "Clauses as Amended by the Standing Committee" that the proposal objected to by Dr Clouston is still maintained.

Into the Crichton Royal Institution at Dumfries there were admitted during 1888, 294 cases; 812 have been treated, and on the 1st of January 1888, 517 patients were resident. The discharges numbered 133, of whom 82 had recovered. There were 44 deaths, one being accidental or suicidal. The ratio of recoveries to admissions was 27·5 per cent., being a little higher for females than males. Dr Rutherford traces this low rate to the largely increased number of admissions during the year, doubling that of the preceding year; 11 patients entered the asylum voluntarily, not being certified as lunatics. 110 of the cases admitted were of the private class. Attached to the Crichton Institution there is a large mansion-house at Kirkmichael for first-class patients. Three patients enjoy shooting on the neighbouring grounds, which extend over 3000 acres, and more enjoy the trout fishing in the river which bounds the estate. The following table shows the number of private patients at different periods:—

	Admitted.	Discharged.	Died.	Remaining.	
				189	Dec. 31, 1868
1869 to 1873	280	204	46	219	,, ,, 1873
1874 to 1878	378	308	70	219	,, ,, 1878
1879 to 1883	315	252	62	220	,, ,, 1883
1884 to 1888	445	277	88	300	,, ,, 1888

The increase of late is largely due to the admission of patients paying intermediate rates of board from £42 to £52 a year.

Dr Rutherford records an interesting experiment which is now closed for the present. Some female patients who had been boarded out were returned as unmanageable. They were placed in two cottages at the outskirts of the grounds of the Crichton Institution, and have for the last four years "lived quietly and contentedly in the two cottages at Spitalfields, without any attendants, and managing entirely their household affairs. The death of the old woman, who took the principal charge of one of the cottages, suggested the idea of replacing her by an attendant who is advanced in years, no longer fit for full work, and who would rather remain in the service, with lighter duties, than accept a retiring allowance. The characteristic feature of this little community is therefore, for the present at least, destroyed."

Probably Dr Rutherford found some difficulty in getting a patient insane enough to be declared incapable of caring for herself and sufficiently sane to be able to be entrusted with the care of others. Dr Rutherford, it must be acknowledged, has conducted some difficult experiments with wonderful success.

The Report of the Derbyshire County Lunatic Asylum shows the successful manner in which the establishment is managed. Dr Lindsay is in search of a new water supply—the water now in use contained too large a quantity of lime and magnesium salts. It is proposed to meet the difficulty by boring the wells to such a depth as to pass to a rock stratum beyond the lime, where a purer water would probably be obtained. The reduction of the county rate for pauper lunatics from 9s. 6d. to 9s. each per week is recommended. The admissions during the year have been 159, the discharges 101, and the deaths 75, leaving 435 patients in the asylum, showing a decrease of 16 in the number since last year.

Dr Murray Lindsay, the medical superintendent, notes the admission of several idiots, and gives his adherence to the very general opinion that such cases should be dealt with outside a lunatic asylum. Of the 101 patients discharged, 65 were recovered, being at the rate of 43·9 per cent. to admissions (excluding transfers), which is a very satisfactory result. Dr Murray Lindsay remarks on the very unfavourable character of the admissions, to which he attributes the unusually high death-rate. Out of the 74 deaths, coroners' inquests were held on four, of which two were suicides, one accidental (caused by swallowing a large chestnut, necessitating œsophagotomy, death taking place from hæmorrhage), and one non-preventible. The average age at death was 46 years. An interesting table is given of the probable causes of insanity. The diet roll included in the Report would be difficult to improve on.

It is worth noting that in the Cumberland and Westmoreland Asylum only one death from suicide and one from accident has happened in the last twenty-seven years. During that period 41·2 per cent. of those undergoing treatment have been discharged as cured, and during the last year 53·1 per cent., showing a steady rise in the recovery-rate. Dr Campbell accounts for the smaller number of recoveries amongst private cases by the fact of their having, as a rule, been kept too long at home before being sent away for skilled treatment. The recommendation of his committee to erect separate cottages for private cases, would, if acted on, help to remove the objections of friends to send such cases for treatment along with pauper lunatics. We note with pleasure that the committee also recommend the erection of a rate-supported training school for imbeciles. No one acquainted with the matter looks on the confinement of such cases in an asylum for the insane otherwise than as a melancholy survival of the neglect and ignorance of past times. It is surprising that none of our politicians who watch with such zeal over the liberty of the subject, has, as yet, got a clause inserted in the Lunacy Law Amendment Acts, forbidding the imprisonment of idiotic and imbecile children of tender age in asylums for adult lunatics.

The charge to the Union for each pauper patient for the last year has been eight shillings per week. This contrasts favourably with the rate in many Scotch asylums. Amongst the 63 deaths which occurred during the year more than a quarter were patients of over 70 years of age. Although the medical superintendent remarks on the unusual number of patients suffering from acute pulmonary affections, the death-rate from phthisis is smaller than in most other asylums. The average time of residence with recovery seems to have been from three to six months. The diet roll, otherwise very good, does not contain fish. The statement of the farming and gardening accounts indicates the successful management of this most important part of asylum administration. The proportion of attendants to patients is about one to fourteen.

During 1888, 97 cases were admitted into the Newcastle-upon-Tyne Asylum, the number resident at the beginning of the year being 319. The discharges were 53, and there were 32 deaths; 41 cases were discharged recovered, or at the rate of 42·27 per. cent. of the admissions. The percentage of deaths of the average number daily resident was 9·8. One suicide took place. The committee note the satisfactory completion of the additions to the asylum and of the farm buildings. This asylum has in the past year lost the valuable services of its able superintendent Dr Mickham, who has had to retire through ill health, after eighteen years' services, on a pension of £400 a year.

We advise his successor Dr Callcott to consider carefully what form of classification of insanity he should adopt in future reports. In the present one he puts down his 97 admissions as 55 cases of mania, 34 of monomania (in which term is included melancholia), 5 of dementia, 3 of congenital imbecility, and 16 cases of general paralysis (2 of which were in females). This classification is not up to the level of modern psychiatry.

Thirty-second Annual Report of the Registrar-General. (Abstracts of 1886.)

To touch upon all the points of interest in this Report would occupy too much time and space; nevertheless, it is well worthy of careful study, and only by this means is one able to realize the enormous labour entailed and the great value accruing from such an accurate record of the births, deaths, and marriages.

The total number of births registered throughout Scotland during 1886 is 127,890, or 324 in every 10,000 of estimated population. This is a little higher than last year, but, with that exception, the lowest record since the returns are available. Illegitimate births are a little below the previous year, and equivalent to 8·21 per cent. of the whole,—that is to say, 1 in every 12 was born out of wedlock in 1886 against 1 in every 11 the year before. In Wigtown and Banff over 16 per cent. of the births are illegitimate, a very high proportion. There were 1331 twin births as against 1423 the year before, and 10 cases of triplets against 14 in 1885. The number of marriages registered in Scotland in 1886 was 24,515, less by 789 than the previous year, and giving the lowest marriage-rate on record, namely, 62·07 per 10,000; whereas in England during the same year it was 70 per 10,000. It is also the lowest marriage-rate recorded during 1886 amongst the principal European nations. Among the principal towns, the marriage-rate is highest in Edinburgh, being 86·9 in 10,000 of estimated population, Glasgow coming next with 85·8 per 10,000. The fertility of marriage is slightly less than the previous year, 479 children being born to every 100 marriages as against 491.

The deaths registered during the year amounted to 73,640, giving a mortality of 18·6 per 1000 of the estimated population, or 1 death in every 54 of the inhabitants of Scotland. This is lower than the death-rate of the preceding year, which was 19·1, and lower than the mean death-rate for the last 10 years, which is 20 per 1000. Of course the death-rate in the principal towns and large town districts exceeds that of the country as a whole. Glasgow has a total number of deaths numbering 13,104, giving a death-rate of 25 per 1000; Edinburgh, with 4889, has a death-rate of 19 per 1000; Dundee, 18; Aberdeen, 19; Leith, 18; Paisley, 23; Kilmarnock, 21; Perth, 18; and Greenock, 17 per 1000; being a slight decrease over the previous year for Glasgow, Dundee, Paisley, and Greenock, and an increase in the rates of the other towns mentioned. The death-rate of the eight principal towns, however, is in every case considerably below the mean of the preceding 10 years. This is very satisfactory, and shows that sanitary legislation is exercising a sure and certain effect on the health of the community.

In regard to the mortality as to age, it is satisfactory to find some decrease in the death-rate under 1 year, both in Scotland as a whole and in the principal towns. Also under 5 years there is some decrease. Still our large towns show an amount of infant mortality which calls for inquiry and some legislation which will cause a check to be put on the insurance of infants' lives, and enable the cause of death in many cases to be more accurately ascertained. In the meantime, registrars might refuse to register the deaths of infants in whose cases medical assistance has not been summoned, and thus force the parents to have the *bodies*, at least, medically examined, and this should be all the more stringently enforced where the children are illegitimate or their lives insured. In Glasgow, 17 in every 100 infants under 1 year die. Dundee shows the next highest rate, 15·5; followed closely by Kilmarnock with 15·1; the lowest being Perth with 10·9. During the second year of life Glasgow and Paisley also head the list, and at all ages under five years Glasgow indicates a mortality rate amongst children of 7·82, Paisley being 5·96, Kilmarnock, 5·67, Edinburgh, 5·56, Greenock, 4·2, and Perth, 3·57. Comparing infant mortality with the total deaths in Scotland in 1886, 37 per cent. of the male deaths and 30 per cent. of the female are registered as occurring during the first five years of life. This is slightly lower than the mean of the preceding ten years.

Coming to the cause of death, 73,640 deaths were registered during 1886, and in only 828, or 1·1 per cent. of the whole, was the cause not specified or ill-defined. This, as compared with other parts of the United Kingdom, is highly satisfactory, but still leaves much to be desired. Why should there be any uncertified deaths? In our opinion, it is the duty of the State to see that every death is certified by a qualified medical man, and to provide a means by which this can be accomplished.

8103 deaths were ascribed to zymotic diseases during the year, or 1143 fewer than the previous year, with a mortality of 11·12 per cent. of the specified deaths against 12·55 the previous year. Of the total zymotic deaths, it is worthy of remark that 5352 occurred during the first five years of life. It is further brought out that 21·23 per cent. of all deaths occurring during the first fifteen years of life are due to zymotics; while from fifteen years upwards the percentage is 4·09.

On looking at the mortality curves for zymotic diseases during the last twenty years, one is struck by the steady fall which is taking place in regard to some of them; for instance, diphtheria shows a gradual and continued fall during the last twelve years, till now it is lower than it has ever reached since 1865. Typhoid is also lower than it has been since 1865, and shows steady decrease. Scarlet fever also, with the exception of last year, is lower by far than it has ever been during the same period. Measles also are lower than they have ever been, as also typhus fever, which has decreased from 109 per 100,000 in 1865 to 2 per 100,000 in 1886. We have thus lower death-rates in typhus, diphtheria, typhoid, measles, and in scarlet fever with the exception of 1885, than there have been since 1865. Hooping-cough and diarrhœa are comparatively low, but do not show any signs of a permanent decrease in their mortality.

The Report gives many other interesting details. Cancer accounted for 2313 deaths, equal to 59 in every 100,000 persons living, the number of males dying being 822 and females 1491. In Edinburgh, 48·7 in every 1000 deaths were due to this disease; in Kilmarnock, 39; in Aberdeen, 33; in Dundee, 32; in Greenock and Leith, 31; in Paisley, 22; in Glasgow, 20; and in Perth, 14,—a rather curious order, and difficult to account for. Puerperal fever caused 270 deaths, surely too large a number, considering the widespread knowledge of its causation and mode of propagation.

The meteorological conditions during 1886 were roughly as follows:—Prevalence of easterly winds; deficiency of sunshine, and everywhere a lowered average temperature. The rainfall was also deficient in most parts, being 1·72 inches under the average over the whole of Scotland.

The Report, as was said before, contains a vast amount of interesting information, and deserves a careful perusal. The only point which we look forward to seeing changed is in regard to the time elapsing before publication.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXVIII.—MEETING XII.

Wednesday, 19th June 1889.—Dr SMITH, *President, in the Chair.*

DISCUSSION ON INTESTINAL OBSTRUCTION.

The Chairman, in introducing the subject for discussion, said the pathological conditions leading to this affection were various and very different indeed, but in a society of this kind they might assume that these were familiarly understood among the Members. Certain symptoms were common to all those affections; others, however, supposed to distinguish the special lesions, were obscure and difficult, and on those the discussion this evening would probably turn, as aiding in the diagnosis with a view to the treatment to be adopted. He supposed this would be the line upon which the discussion would be directed, viz., the diagnosis of the peculiar lesions which constituted those obstructions. He had great pleasure in asking Mr Miller to introduce the subject.

Mr A. G. Miller said,—Mr President and Gentlemen, I suppose that your Council has selected me to open this discussion because I suggested the subject as one worthy of consideration. I hope to be able to direct the discussion towards one point, so that a concentrated light may be thrown on it. This is all the more necessary because the subject is a very wide one, and the amount that has been written on it during the past twenty years is immense. Were I to attempt to give anything like a *résumé* of the literature of Intestinal Obstruction I would weary both myself and you. I will therefore limit myself as much as possible to the subject of *diagnosis*. But first as to definition, though I think this is hardly necessary.

By "intestinal obstruction" we understand, I suppose, that condition in which there is obstruction to the passage of fæces along the intestine, what is commonly called "stoppage of the bowels." For practical purposes, however, I think we will all agree to eliminate cases of strangulated (visible) hernia on the one hand, and malignant stricture of the rectum on the other.

I have selected the point of diagnosis because—(1) it comes first necessarily in point of time; (2) it comes first in importance, seeing treatment necessarily depends upon it; and (3) it is often —I may say, very often—difficult and doubtful. Diagnosis may, I think, be aided by dividing all cases of obstruction into three classes:—1. The very acute. 2. The less acute. 3. The chronic.

The first class are easily recognised. They have the general features of a case of recent strangulated hernia. These are—sudden pain, sickness, and collapse, the pain being referred to the umbilical region, the sickness being of the persistent character, quickly becoming fæcal, and the collapse being sudden and severe. In such a case, though there should be no visible hernia, we know at once that there is not only stoppage of the passage of fæces, but also a condition of the bowel (constriction or obstruction) which, if not speedily relieved or removed, proves quickly fatal.

The third class of cases also should be easily diagnosed. I will not therefore refer to them specially. One must remember, however, that there are instances in which obstruction arises from some very chronic cause, but in which the symptoms come on suddenly. Even in these cases, however, the previous history and condition of the patient, if gone into carefully, should make the nature of the obstruction clear enough.

It is in the second class of cases that there is greatest difficulty of diagnosis—those which lie between the very acute and the very chronic. It is of great importance to see them at the very first, for the initial symptoms very quickly change or become masked. A physician has a case of obstruction under his care for some time, and then sends for a surgeon. Why? Because his treatment has failed, or because he has not been able to come to an exact diagnosis. The consultant is placed at a double disadvantage. He is expected to make up his mind at once on a case that the other has been studying for some time, and, besides, he does not see the case under the most favourable circumstances. I consider, therefore, that in every case in which obstruction exists, but in which an exact diagnosis cannot be made out, it is important for the patient that a consultation should be held at once, both a physician and a surgeon being present. The one will act as a check on the other, for each doubtless will look at the case from a somewhat different point of view, and each will thus be able to supplement the other's examination. I would also suggest that at such an examination, if doubt exists, the patient should be placed under chloroform or ether. Palpation of the abdomen would thus be made more easy and accurate.

The next difficulty is the *masking of symptoms by remedies.*

These may be taken by the patients themselves, or administered by a doctor. Of these the commonest perhaps is a purgative. This does harm, not only by aggravating the existing symptoms and by weakening the patient, but by inducing symptoms that did not previously exist. On the other hand, one may doubt sometimes whether the sickness, hiccough, etc., are not mainly due to the medicines administered. Another remedy usually given is opium, or some other narcotic. These relieve pain, diminish sickness, and inhibit the tendency to collapse. To this extent they do good; but they also do harm by raising false hopes, and occasioning the postponement of the operation that otherwise might be at once determined on. Enemata are often useful, but sometimes interfere with diagnosis by causing distension of the abdomen.

Abdominal distension, it is quite plain, will make diagnosis difficult. Palpation will be most unsatisfactory, and the discovery of an intussusception, for instance, almost impossible. In cases of distension the administration of an anæsthetic may aid the examination very materially.

Peritonitis, which comes on sooner or later to a greater or less extent in every case, not only masks the general symptoms, but leads one to doubt sometimes whether the inflammatory affection is not the main or only one.

The last difficulty I wish to refer to is the *absence of some ordinary symptom*. In intestinal obstruction the one common symptom is stoppage of the fæcal passage. Every other symptom that I know of may be absent or greatly modified. Even that may be made uncertain, and therefore misleading, unless great care is taken; for the return of air injected during the administration of an enema may be mistaken for the passage of flatus, and unabsorbed meat suppositories may be mistaken for fæces. Even characteristic symptoms may be absent, and this may oblige one to hesitate in forming a diagnosis. For instance, in a case of intussusception there may be no bloody mucus discharged. In another case vomiting may be absent, or very slight; and this is most common, I believe, when the obstruction is below the ilio-cæcal valve. Pain may be modified by collapse, and we all know how one or even more motions may be passed after obstruction of the small intestine is complete.

The comparative frequency of these deviations from the common rule makes it necessary that we should be careful in forming a diagnosis, and should be willing and anxious in every difficult case to obtain at once the assistance of a colleague, seeing complications arise so quickly and mask the distinctive symptoms.

I believe that one of the best contributions to such a discussion as this is to give one's experience. I will try to do this as briefly as possible. I have performed abdominal section for unrelieved obstruction five times in the Royal Infirmary during the last four years.

CASE I.—M. B., æt. 36, female, recommended by Dr Affleck.

History.—Sudden pain in abdomen on 3rd January 1886; no vomiting. Took two wineglassfuls of castor-oil, which caused sickness. Enemata and croton-oil administered without effect.

On admission to Medical House, 11th January, abdomen very tense and tympanitic, even over liver; colon distinctly mapped out; nothing found per rectum. Seen by Mr Miller on 13th, who advised against an operation. Dr Thomas Keith was also brought into consultation at Mr Miller's request, and he agreed that there were not sufficient grounds for an operation at that time. Three days later, patient, having become much worse, was removed to Mr Miller's wards, where abdominal section was performed. The sigmoid flexure was found crossing and bound down to the sacrum by a band of fibrous tissue, and the colon enormously distended with hard fæces. The constricting band was cut and the bowel relieved, after which three copious evacuations took place. Patient died next morning.

Post-mortem.—No evidence of recent peritonitis; ring of congestion round sigmoid; omentum adherent to intestines; three perforations in colon from ulceration, apparently caused by fæcal infarction.

CASE II.—J. S., æt. 5½ months, male, recommended by Dr T. Duddingston Wilson. Admitted 3 P.M., 6th October 1886.

History.—Perfectly well till two days before admission, when he was suddenly seized with pain in the abdomen, followed by vomiting and discharge of blood and mucus from the bowels. Intussusception felt. Abdominal section recommended and performed at once. Invagination (of lower portion of ileum, cæcum, and ascending colon into transverse colon) undone, and symptoms relieved. Child died two days after, apparently from bronchitis.

Post-mortem.—No general peritonitis; matting of cæcum and ascending colon to adjacent intestines; lower end of ileum and cæcum congested; marked bronchitis; no pneumonia.

CASE III.—F. M., æt. 52, male, admitted from waiting-room, 17th September 1887.

History.—Subject to attacks of constipation during last nine months; present attack nine days ago; came on with pain in left iliac region; no vomiting. Used enemata and purgatives; retched on one day only—the day before admission.

Condition on Admission.—Abdomen distended; colon distinctly mapped out; pain in left iliac region and over sacrum, latter very severe; constipation, but flatus is passed occasionally. *Per Rectum.*—Piles; ring round rectum high up, evidently not malignant. Patient greatly emaciated and very weak. After consultation, abdominal section was delayed, but performed on 19th merely as a *dernier ressort.* At operation volvulus of sigmoid, contraction of bowel above volvulus, and distension of colon were found. Patient died three days after with symptoms of septicæmia.

Post-mortem.—No general peritonitis; fibrous contraction at upper part of sigmoid due to old inflammation; large septic abscess almost surrounding rectum at upper part.

Case IV.—M. H., æt. 18, female, recommended by Dr Wyllie.

History.—Sudden pain in abdomen on 1st January 1889; vomiting marked and immediate; complete constipation. Admitted to Medical House 3rd January.

Condition.—Pain at umbilicus and over sacrum; no vomiting after admission; abdomen distended; no mapping of intestines; treatment by morphia, enemata, etc. Patient removed to Mr Miller's Wards 9th January, when abdominal section was at once performed. Volvulus of sigmoid discovered and undone; bowels evacuated and symptoms relieved. Death two days after, suddenly, from the shock of a return of the obstruction.

Post-Mortem.—No general peritonitis; sigmoid flexure greatly enlarged and distended with flatus, and pressing backwards on descending colon, so as to completely obstruct it; constriction of upper part of sigmoid; stricture commencing at junction of sigmoid with rectum. (See full report of case by Dr Stoddart, Resident-Surgeon, *Edinburgh Medical Journal* for May 1889, p. 1031.)

Case V.—R. S., æt. 10, male, recommended by Dr Dickie; admitted 27th February 1889. Patient was seized with severe pain in abdomen on his way to school, and immediately after had a loose motion. This was on the 25th. He was seen by the doctor, who diagnosed intussusception, and sent him to me on the 27th for operation. The usual tumour was found in the usual situation. At the operation, which was performed at once, the invagination was readily reached; but, contrary to my expectation, could not be undone, owing to adhesions. In using considerable force the gut gave way at one or two points, its substance being very friable. Resection of the adherent portions was at once decided upon. On removing the intussusception it was found to be gangrenous and putrid. As the boy by this time was much collapsed, the ends of the bowel were brought out at the wound, and the formation of a fæcal fistula aimed at. The boy sank, and died the same night. The invagination was at the cæcum. No post-mortem was permitted.

In remarking on these cases I would like to say, first, that the fatal result, which unfortunately occurred in every instance, was largely due to complications — ulceration of colon, bronchitis, absorption from septic abscess, recurrence of obstruction, and gangrene of bowel.

In the two cases of intussusception diagnosis was easy, and the result should have been satisfactory, but was unfavourably influenced by complications.

In the other three cases it is interesting to note that they were all instances of sigmoid obstruction, in all vomiting was markedly absent, and in two at least severe pain over the sacrum was complained of. In all of them diagnosis was made more difficult by the occurrence of symptoms, partly due to the administration of remedies, and partly to the supervention of peritonitis, for which, unfortunately, time was given.

Mr President and Gentlemen,—I know that what I have said has been very commonplace, and anything but exhaustive of the subject. I hope, however, that valuable comments and relations of experience will follow these remarks.

Dr John Duncan said he feared the subject was not one which lent itself very easily to such a discussion on account of its immense magnitude, it being the usual experience that limited subjects were more useful in that way. Even from a surgical point of view there was an immense variety of questions, an immense mass of details connected with laparotomy, enterotomy, and particularly in relation to sepsis, so that it was difficult to select matter for comment. He was inclined to agree with the Chairman and Dr Miller that the most important part of the subject was that which was connected with diagnosis,—if they could make the body diaphanous they should have little difficulty as to what treatment ought to be adopted. In this, as in most other surgical operations, the mere operation probably required some brains, but it required extremely little manual dexterity.

Diagnosis depended so much, in every individual case, upon investigation of minute details, that it was almost impossible to lay before them, except in connexion with special cases, the points to be discussed. Perhaps it would be best that he should endeavour to indicate a classification of those difficulties rather than to deal with minute points; to ask, for instance, what were the diseases which were apt to be confounded with intestinal obstruction; and next, what were the difficulties in their way in isolating the various forms of obstruction, when they had decided that obstruction was the thing with which they had to do. He would be inclined to regard obstruction rather as divisible into the acute and the chronic than into the three classes which Mr Miller had mentioned, and to look upon the first class as those which presented, more or less severely, the three symptoms to which he had referred—the pain, the vomiting, and the collapse—because he regarded the constipation as perhaps the least valuable and most unreliable of all the symptoms they had to act upon in these cases. What little attempt he should make to classify would be rather in connexion with the acute than the chronic forms of obstruction.

In the first place, what were the diseases and the conditions that were liable to be confounded with obstruction? He thought they might put it, in the first place, that they had a great class of diseases which essentially consisted in inflammation of the lining, the internal coating of the bowels, in every one of which there were difficulties in diagnosis, and mistakes might be made by competent practitioners,—for instance, dysentery, irritant poisoning, and so on. Next in the process of classification they had inflammation of the external covering of the bowel—peritonitis. Under this head, the most usual, the most common and simple forms of peritonitis present little difficulty. They had, however, various cases in which extreme difficulty was met with. For instance, they had peritonitis itself leading to obstruction very quickly, either by bands of lymph or twistings. Then they had peritonitis often arising from obstruction, very speedily in some cases. They had, again, peritonitis the result of perforation of the vermiform appendix and elsewhere, giving symptoms that were extremely like the symptoms of acute obstruction. And, lastly, they had, as resulting from peritonitis, a very peculiar class of cases, in some of which even laparotomy had been done with some success. He thought it was Mr Humphrey who had a very curious case, in which he opened the abdomen and found nothing except peritonitis, and closed it up again with absolute success. Some of those cases, he thought, undoubtedly arose from that peculiar form of paralysis of the bowel which resulted from the inflammation passing through its coats. He had a case of hernia in which he operated—rather late, he must say—on a bowel upon which taxis had been employed. He found the bowel extremely bruised, with extreme extravasation in its coats. The patient was an old man, nearly 70, and he died within three or four days. They found, on *sectio*, simply this,—a paralytic condition of about six inches of the bowel, and above it a mass of soft fæces which should have had no difficulty in going down, and yet could not pass this, not obstructed, but paralyzed portion of the bowel. Another case, illustrating the difficulty of diagnosis, occurred to him in which a boy had tubercular peritonitis. He was supposed to be dyspeptic, but nothing more. However, one morning severe vomiting came on, becoming feculent in twenty-four hours. They diagnosed obstruction, and he opened the bowel, but found that it was simply tubercular peritonitis. The boy died shortly afterwards. These were examples of extreme difficulty in diagnosis. Lastly, there was a class of cases which they must bear in mind, which, under the third head, were apt to be confused with obstruction—that was a reflex irritation of the bowels. One very interesting case he remembered coming before him,—a case which was sent to him as a case of strangulated hernia in a boy. There was a lump, inflamed, and tender and painful, in his groin, and he had extreme collapse and vomiting. When he examined it before operating, he found that there was no testicle in the scrotum. He cut down upon this tumour, as they ought always to do in the hernial regions, and he found it to be a testicle partially descended which had become inflamed in its passage. These reflex conditions gave rise to symptoms exceedingly like obstruction. These, then, were the chief diseases—inflammation within and inflammation without the bowel, and reflex conditions—which were prone to be confounded with obstruction. When they came to the diagnosis of what was really obstruction, perhaps the first and most important class which they had to diagnose were those in which severe symptoms of obstruction supervened upon accumulations in the bowel, whether these accumulations were of foreign bodies or of fæces, or supervened upon chronic tumours, malignant or other, connected with the bowel. Out of a good many of such cases he would refer to two. One was a case which he was called to by Dr Foulis, of an old gentleman who had a very marked form of obstruction. They found a lump, which was to be felt in the left fossa, and they diagnosed the possibility of physical obstruction. He passed a long tube, which resulted in the passage of an enormous quantity of liquid fæces after it had gone through with a little pressure and with a sudden jerk. The success of the long tube here proved that in a good many cases they could pass the tube further up than in their books they were given to believe. No doubt in many cases it was utterly impossible, but the long tube was a thing which ought not to be neglected. But there was another set of cases belonging to that class, of which the following was an illustration,—the sudden supervention of acute symptoms upon a long-standing tumour. These tumours were very often not to be felt in any way. They were frequently to be diagnosed by preceding attacks of more or less acute obstruction from which the patient had recovered. An interesting case was that of an old person, who, a year before, had an attack of obstruction, from which she recovered, and during the whole year there had not been the slightest recurrence of any trouble with the bowel. Acute obstruction again came on. He operated then. She was exceedingly fat; but even had she been exceedingly thin, he believed they should not have been able to save her. They found, upon laparotomy, that there was a small, malignant, annular stricture of the ascending colon. The bowel was collapsed below and distended above. They found it impossible to do anything, except to separate the part to bring it to the umbilicus. In the process of separating it rupture occurred, and though the toilette of the peritoneum was made, she died of septic peritonitis. In such cases, if the acute obstruction was the first attack, and no tumour was to be felt, so far as was known at present it was absolutely impossible to tell them from any other sort of obstruction. He knew no means of doing it. Lastly, they had other cases of volvulus, internal hernia, intussusception, and so on, which required to be diagnosed from each other, and which, when they were diagnosed, were perhaps surgically the most satisfactory of all the rest. He remembered a very interesting case, in which, after a diagnosis of internal hernia of some sort, he opened the abdomen and found, after some search, running, as was a very good plan, from the cæcum backwards, a tight portion of collapsed bowel, which ran down into the pelvis, and they found a portion of the bowel protruding into the structures behind the uterus. This was pulled out, and things put right. The conclusion of this classification plainly was that they should endeavour, to the best of their power, that they make their diagnosis accurate, and, from a surgical point of view, try to eliminate, in any individual case, the diseases which were apt to be confused with strangulation. In the first place, especially inflammation of the bowel and reflex forms, because in these laparotomy could do no possible good, it was absolutely necessary that they eliminate these. It was essential that they should distinguish the various forms of obstruction from each other, rather with a view to putting in practice—in intussusception, for instance—of remedies which they knew to be successful; but it was still more important that they should diagnose early the cases in which laparotomy ought to be performed. Undoubtedly the great mortality which had attended laparotomy was almost entirely due to the late period at which it took place. It was precisely the same thing with regard to hernia. If they could operate on hernia soon, before severe taxis was tried, before or immediately after inflammation had time to set in, and, above all, before gangrene, there was no operation so successful. They were almost sure the patient would do well. If they could manage to diagnose cases of internal hernia, or volvulus, or intussusception early, their success would be exceedingly great.

Dr Affleck said that as this subject was one in which he felt considerable interest from sundry experiences connected with it, he might be excused for referring to some points. The diagnosis could, in his opinion, scarcely be considered separately from the other points in connexion with it; but the subject was such a large one, that one hardly knew where to begin or how to deal with it. He entirely concurred with Mr Duncan's remarks in regard to the difficulties which arose in diagnosing obstructions from other conditions which had no connexion with it,—such, for instance, as the inflammations of the coats, mucous membrane, or peritoneum. Even here, he repeated, the difficulties were exceedingly great, and had he time he would refer to some of his experiences. He had had cases where the abdomen was opened for all the symptoms of acute obstruction, and ulceration found. Any remarks that he had to make would bear reference to some clinical aspects of the question, and they were somewhat personal and general. He meant to confine himself to cases of acute intestinal obstruction. It had been his experience as a physician to encounter cases of this kind; and while he in one or two instances succeeded in applying treatment which rescued the patient from imminent death, in a great number of instances he had been obliged to witness the swift and painful progress of the case to its fatal issue. For example, he put upon record as far back as 1873, two cases of intussusception which occurred to two children six months old (and which he watched) within nine months of each other, where treatment failed in one case, and a *post-mortem* examination showed a large part of the small intestine intussuscepted and gangrenous. The other case proved a success. In it he inflated the bowel with a pair of bellows and the child recovered. There were here all the symptoms present, such as bleeding from the bowels. The child was in a near state of collapse when the operation was done. He was, however, rescued. It was not very long ago since a case occurred in the Infirmary, where a man was brought in said to be suffering from acute peritonitis. He (Dr Affleck) went up to him and observed that his abdominal muscles were straining hard, and he came to the conclusion that it was not a case of peritonitis so much as a case of obstruction. One of the surgeons saw the case with him, and also came to the conclusion that it was a case of intestinal obstruction, and recommended opening the abdomen. The man's wife was sitting by the bedside in great distress, and she asked him if nothing else could be done. He then resolved to try the interrupted current. The result was that the man passed a large motion and got immediate relief. He might say that in several of these instances he had invoked the aid of the surgeon, believing that his interference alone afforded a chance for life. While everything that surgical skill could accomplish in the matter of the operation was done, he regretted to say that in every case, notwithstanding that the obstruction was discovered and relieved, the result was unfavourable. In pondering over such unfortunate experience he sometimes felt inclined to blame himself for not sooner seeking and more earnestly pressing for the surgeon's aid, but he felt at the same time bound to state that sometimes the delay had been on the part of the surgeon rather than himself, and that therein lay the fact that delays often arising were particularly against the patient. He did not for a moment blame the surgeon. He had to go into the case knowing nothing previously about it, and this as well as the possibility of error in diagnosis, and the uncertainty of the locus, often tended to deter to some extent from immediate interference. Besides, there was the well-known fact that not very seldom cases which bear all the features of serious mechanical obstruction, whether truly so or not, undergo spontaneous relief in a very remarkable manner, a circumstance which tended to delay an operation, the risks attending which were so considerable. There were many instances where in cases presenting all the appearance of serious organic obstruction a sudden yielding had taken place and the patient recovered. The consequence has been that the operation has usually not been undertaken until symptoms had arisen which, to his mind, appeared to throw the balance of probable success heavily against the patient. He felt perfectly sure that when the temperature began to rise, the pulse to quicken, the face to get pinched, and the abdomen markedly tympanitic, it was nearly hopeless to expect any other than a fatal result. He was firmly convinced that success depended entirely upon early procedure. But here came in the momentous importance and grave responsibility of the physician in relation to the case. Now he was, he thought, bound to see that everything possible had been done, that every means of investigation was exhausted to exclude error in diagnosis. This naturally introduced the question of whether there was any means of attaining approximate accuracy in the diagnosis, in finding the locality of the disease. The physical examination sometimes yielded valuable positive evidence, as in the case of intussusception, but it was not very valuable as negative evidence, because there might be very serious obstruction without any evidence of it by physical examination. It was well, then, to guard themselves against supposing that any one symptom, or class of symptoms, was capable of settling the diagnosis in a given case. This would be as dangerous a view as to hold that they had no guides or clues at all to help them. When they considered the numerous causes of obstruction, even acute, and the fact that the same lesion appears to affect different individuals differently, and that in one it might be rapid and in others the reverse, it was easy to see the difficulty. They seemed thus to be driven to what was after all the most valuable element in the diagnosis, namely, the clinical history of the case. A careful consideration of this, with, of course, the character of the present symptoms, and it might be the physical signs, would often lead one very near or even actually to the truth as regards both the nature and seat of the lesion. Probably, however, they were oftener left on the ground of conjecture. Now he desired to refer to some of the points of evidence that might be trusted. In every case he advised them to examine for hernia, and also to explore the rectum. They might find intussusception in the rectum when there was no evidence of it on the wall of the abdomen. The age of the patient might help in diagnosis. If, for instance, they were called upon to examine a child for obstruction—the symptoms being to all appearance present—they could hardly be wrong in supposing or believing that in all probability it was intussusception, the cause of the great majority of cases of obstruction in children up to the age of ten years. They would also probably find it in the commonest of all localities, the cæcum. It would be still further confirmed if with the other symptoms they found the sausage tumour, and the red discharge from the bowels, with vomiting and collapse. In addition to this, inquiry ought to be made as to the habitual state of the patient's bowels, for here they might be led to fæcal impaction, or volvulus produced by overweighted bowels. Another thing that he sometimes found of use in inquiring into this cause was the history as regards any previous inflammatory condition of the bowels, if there had been at any time peritonitis, because some cases had occurred, such as the one to which Mr Miller had referred, in which they were very apt to think of adhesions, or bands, or things of that kind, that were likely from certain circumstances to be the cause of acute obstruction. So also with regard to gall stones, etc. Now, what were the main symptoms upon which they could rely as indicating obstruction? Constipation was of less value than some others, although it certainly was valuable, and he should say that constipation together with the absence of flatus were important points in the diagnosis. If they saw the intestines inflated and no discharge downwards, then that was a point that was not to be despised in regard to the diagnosis. Then pain was a very important indication. It was important, further, to study the character of the pain. Was it a constant or an intermittent pain? Did it come on suddenly? These points were valuable as indicating the position of matters. Vomiting was in his opinion a very important indication, and was generally said to be, and he believed it was, the more urgent the higher up the obstruction was, but that was not by any means conclusive. He did not think the vomiting necessarily depended, as some held it did, upon the greater activity of the smaller bowel as compared with the large bowel. He supposed that in many cases vomiting was a reflex condition, because it sometimes occurred very severely and constantly when the obstruction was low down. But vomiting more frequently occurred when the obstruction was in the small intestines, and the pain was more acute when the obstruction was in the small intestines. There was a means of diagnosis suggested by Mr Treves,—the injection of water and the auscultation of the colon and cæcum. He was not, however, sure that that would be useful, because obstruction might exist and allow water to go up, but not let fæces come down. The practical bearing of the question was, What relation had they to each other in the case when the physician called in the surgeon and got his help? Now they could hardly separate the treatment from the diagnosis. The usual treatment was the giving of purgatives, and he did not think that there was anything hurtful in that. It was the only thing open to the physician. It was only when ordinary remedies had failed that the thing began to dawn upon him. When the physician was first called upon he did not at once see that it was acute obstruction. In all cases of constipation they should avoid severe purgatives, although it seemed to him that the ordinary purgatives were of no hurtful effect. Opium not only quelled the pain but prevented collapse, and also appeared to act as a stimulus to the bowel if it was given in small quantity. Certainly some cases seemed to be relieved by a dose of opium. A great many cases were on record where the steady administration of opium relieved acute obstruction. Enemata were also valuable, although they, too, might irritate as well as purgatives, because they might bring on vomiting, but they were within the physician's reach. He had used massage, and had succeeded in relieving very serious obstruction. The question was when these remedies ought to stop. When does the case assume serious aspects and require other assistance? Now this was a very difficult matter, and each case should be judged upon its own merits. Certain it was that individuals differed as to their tolerance of obstruction. Some seemed to be killed very soon, others lived on for many days. It was, therefore, not easy to lay down rules to each case. He was not very sure whether he was right, but he would suggest that physicians might look upon this matter as surgeons did upon hernia. When vomiting occurred, and when there was no movement of the bowels, it would be time to seriously consider whether something more active should not be resorted to. Certainly when any threatened collapse in the appearance of the patient came, and when the strength yielded, and if possible before the temperature rose and the pulse got quick, then was the time for the interference of the surgeon. The great rule he thought applicable as regards the physician was that he should put his whole mind and conscience into the case. He should inquire into every point of the history that could throw any light upon it, and should attentively watch the patient. If he had even exhausted that he had certainly done a great deal in arriving at an approximate diagnosis. He should put it to himself thus—"If these symptoms were occurring in me with the knowledge I have of my condition, how should I desire to be treated?" Would he, identifying himself with the case, desire to continue temporizing or palliative measures, or would he desire the interference of the surgeon? He should certainly say that if he felt he had an obstruction which probably depended on a band or volvulus, he would not hesitate in his choice, for in one case die he might, but in the other die he must.

Dr Cathcart said he thought it would help them to come to some practical conclusion if they were to discuss some of the individual symptoms of intestinal obstruction, and try to find out how far some particular kinds of these symptoms might be peculiar to certain forms of obstruction. He would try to confine himself to one of the special symptoms—pain. If they asked themselves what was the cause of abdominal pain in the various conditions, which might be either obstruction or something simulating obstruction, it might be divided into three. In the first place, it might be due to direct injury to some of the abdominal viscera. And that injury might be a mechanical pressure upon the viscera, such as, for instance, the constriction of a hernia, or the pressure of a band, or the pressure of a tumour, or it might be due to some injury of the splanchnic nerves, as in the passage of the gall-stones or renal calculus. In all these there would be acute pain, and in that way they could understand there were a great number of different conditions. But there might be other symptoms which would depend upon the nerve irritation. For instance, they might have vomiting and collapse, or they might have the tense condition of the abdominal muscles. Then they also had pain as the result of inflammation. That inflammation might be primary or it might be secondary,—that was to say, it might be an inflammation which started round some mechanical injury, as in the inflammation which always followed a hernia or an obstruction; or it might be an inflammation which was a primary condition from the first, as in localized peritonitis, or the inflammation caused by irritant poisoning, which was not always very easy to treat. Then they had another cause of pain, viz., muscular spasms, and these might be present in conditions which were actually intestinal obstruction or something similar. For instance, they had a muscular spasm which was the result—which was a symptom of obstruction. They had a muscular spasm in ordinary colic, where some part of the alimentary canal was distended by wind. Having stated these, they might discuss the various seats of pain. In the first place, pain might be at the points where the lesion occurred, and in a certain number of cases that was so. He was not prepared to say that pain was always localized, but it seemed to him to be more easily localized in the great than in the small intestine. If pain were in the flanks, they would generally find the mischief there; but if it was across the abdomen, the real seat of a pain might be anywhere in the abdomen. It seemed to him to be a very important matter whether the pain was a constant or an intermittent one. From his own observations he would be inclined to say, that pain was more frequently intermittent in the great intestine, and that it was more frequently constant in the small intestine. Treves said that when the obstruction was constant the pain was always constant. He would simply instance, with regard to that, Mr Miller's last case of volvulus. The pain was very acute at first. According to the woman, she walked home doubled up and had extreme pain. He was not told so, but he rather thought she had spasms. In her case intestinal obstruction seemed to be complete, and very tight strangulated hernia would cause great pain by a mechanical injury, and therefore a constant pain was only to be expected, and paroxysmal pain must be left out of account in that case. He had just read a physician's account of a severe strangulated hernia, and he complained of a very constant pain. He had always found that in intussusception paroxysmal pain was a very marked symptom. In a very acute intestinal obstruction the pain was generally constant. Then, as regarded the onset of the pain and the progress of the case, where the pain came on very suddenly, it generally meant that there had been a sudden obstruction, but it did not necessarily mean that there had not been previously an obstruction—it only meant, probably, a sudden development to complete obstruction. Then, so far as he could believe, pain might be very acute at first; but it might be that, as the patient got worse, the pain got less. Then came the question of tenderness. There were cases in which tenderness might be very marked and where pain might not be excessive. There were other cases in which pain might be excessive where tenderness might be comparatively slight. Tenderness seemed to point to more or less peritonitis, and it was the presence of tenderness as a marked symptom, and hardness of the muscles over the affected region, which enabled them to tell between perforation and obstruction. In a case which he saw recently, where there was a rupture, tenderness as well as pain was a symptom. Vomiting was another symptom. It might be due to two things. It might be reflex, due to severe affection of the splanchnic nerves or to some obstruction of the alimentary canal. In one of Mr Miller's cases vomiting occurred very early; in the other cases there was very little vomiting. It was an obstruction of a band along the great intestine. His belief was that where they got a sudden injury to any part of the splanchnic system they got reflex vomiting, but that where they got vomiting due to intestinal obstruction it would be markedly in the small intestine, and would be a comparatively small symptom of comparatively less importance, as Dr Affleck said, than when it affected the great intestine. Then, as to the character of the vomiting, there was one symptom which, he thought, had been mentioned by Professor Spence,—wherever the patient had got a comparatively mild form of vomiting, merely belching up fluids, that was an extremely bad symptom. He had never seen a case recover where there was belching of fluids without any retching whatever. He remembered a case in which a patient seemed to be going on very well after operation for strangulated hernia, and the first thing observed was that he began to belch up the fluids he had been drinking, and that went on till he gradually sank and died from gangrene of the intestine. What it was due to he could not say, but it was a different thing to forcible retching; there was no strain, apparently, to the abdominal wall. There were one or two of Dr Affleck's remarks he would allude to. In referring to the age of the patient as throwing a light upon the possible cause of obstruction, he did not mention that in children constipation sometimes caused symptoms very like to intussusception. He remembered seeing a case where the physician in charge was just on the point of sending for a surgeon to open the abdomen, but he continued with opiate applications, externally and internally, and repeated enemata, and at last got fæcal evacuations, upon which the whole case depended. It was proved in that particular case that the obstruction was due to a young child being fed upon starchy foods, and he should always make careful inquiry on that point before coming to a conclusion. He referred to the case mentioned by Mr Miller where they gave the patient enemata just before the operation. These enemata passed through the volvulus and went into the cæcum, and yet did not return, because, at the operation, they found a quantity of the fluid, which had been sent into the cæcum, but which had not been able to pass down. Then as regarded the very important question of when to interfere. It all turned upon the question of diagnosis, and their effort should be to try to know what cases should be interfered with and what should not. There were cases, *i.e.*, such as those of obstruction by a band which ought to be interfered with the very moment they presented themselves. There were other cases which should never be interfered with at all—for instance, irritations of the alimentary canal, possibly local peritonitis. The point came to be this,—considering their inability to come to a definite conclusion, what should be the line of treatment to be adopted in all cases where there was a doubt, and at what period should interference be made? He fancied every one would be of opinion that the line of treatment should be to soothe the parts, to give a diet which should leave almost no solid matter, *i.e.*, to give no milk, to give only arrowroot, and broth, and beef-tea, and to avoid all irritants to the rectum. If it was merely obstruction, the inflammation would be lessened, and if it was merely inflammation, the bowels would move as soon as inflammation subsided.

Dr W. Russell said he thought, in the first place, that the physician should be able clearly to distinguish between all conditions of enteritis and peritonitis, and to separate them clearly and distinctly from cases of mechanical obstruction in the alimentary canal—that was from cases of obstruction not due simply to paralysis of the bowel. One of the difficulties they had was in deciding which cases would go on to grave obstruction. These cases were exceedingly common, and he had met with many cases in which there had been a continuance of constipation, local symptoms, and a degree of sickness, and in which a bold administration of purgatives had most certainly relieved the symptoms, with the result that the patient was cured. Now, a number of these cases no doubt did turn out to be cases of grave intestinal obstruction; but, as Dr Affleck had indicated, the difficulties of diagnosis here were considerable, and was often determined by the result of treatment. If they did not succeed in removing the obstruction by ordinary treatment, they regarded the case as one of grave intestinal obstruction. As to the causes of grave abdominal obstruction, one had had very varied experience. One of his first cases of this kind was in an old and very fat woman. The cause of the obstruction was quite obscure, and it was discussed whether the abdomen should be opened. The patient was considered a particularly bad subject for such an operation, and surgeons were not so ready then as now to open the abdomen, so she was left to die. At the post-mortem a very large gall-stone was found, which could easily have been removed. Another case of a foreign body was in a child who was suddenly seized with symptoms of abdominal obstruction. The interest of the case lay in the fact that at one part the coils of the intestines were united by old adhesions, and there was in this case an old history of inflammation. At the seat of the adhesions he found that the intestine was occluded by an onion, which the child had evidently swallowed whole. This case also might have been saved by interference. He at once put aside cases of internal hernia and cases of twist. In these cases one's experience would lead one to believe that the symptoms came on so very acutely, that there was an indication in the very acuteness of the condition to lead one to diagnose that probably there was mechanical obstruction to the canal. His experience of chronic obstruction, both in practice and in the post-mortem room, was that these cases were mainly due to malignant stricture of the intestines; but he had been struck with the fact that in a very large number of cases the stricture had been of such a limited size that it seemed to him to be suitable for removal and for resection of the bowel. This, he thought, was a fact which was worth noting. His impression for some time had distinctly been that there were a great many more cases of this kind than was generally believed. Then there was one other condition which sometimes led to more or less acute obstruction, but which sometimes ranked itself more properly in the cases of chronic obstruction. He referred to cases which were localized in the neighbourhood of the cæcum. Some of these cases, beginning, as he should expect they most frequently did, in the appendix, might be insidious in their onset—that was to say, in their sub-acute stage—going on rapidly to the more acute stage. Now, even in these cases where the lesion could be localized to the cæcum, there was a practical difficulty in knowing when the operation should take place, because a certain number of the cases got better under ordinary medicinal measures. Others, again, presented a history of pain recurring from time to time in the region of the cæcum, and the case might only come under observation in the stage of acute obstruction. Now, he took it that all cases of this kind ought to be recognised by the physician, perhaps more from the history of the case than from the present condition of the patient; and that was a condition which he thought the surgeon agreed with the physician in saying ought to be operated upon without delay. He thought that, after what they had heard from Mr Duncan that night, many of them would be able in the future the better to grasp and better able to classify the cases which they had seen in the past, and which they were certain to see in the future. He also thought that the discussion, although the field was an exceedingly large one, might ultimately tend to a very good purpose.

Dr James Young said that a case was brought to his mind by the remarks of Dr Affleck. The patient presented the symptoms of severe constipation, and was suffering pain. She was about 60 years of age, and had had no action of the bowels for twelve or fourteen days. He thought he might find some uterine affection, but that was not the case. On palpation of the abdomen there was distension, but no localized pain. On examination of the rectum he found, on passing the index finger, a stricture or tight contraction of a circular character. As the case was urgent, and the patient in great distress, he divided the band very cautiously, and a fæcal discharge followed. That same afternoon he passed a rectal bougie, and the patient has been well ever since. There has been no return.

Dr Wallace wished first to make a remark, and secondly to ask a question. The symptoms in obstruction varied according to whether the large or small intestine was the seat of the obstruction, and he wondered whether some light might not be got as to the seat of obstruction by contrasting the symptoms of strangulated hernia with those of intestinal obstruction. In hernia the small intestine was commonly the part affected, and was it not possible that the symptoms of complete obstruction in the small intestine would closely resemble those of hernia, in which there was complete strangulation, while partial obstruction in the small gut would be analogous to the Littre variety of hernia—the symptoms in both varieties differing from obstruction of the large gut. Recently it had been advocated, in cases where the patient was suffering from obstruction, but in a condition of great collapse, that *enterotomy* should be performed. A small incision to be made in the inguinal region, and the first piece of dilated intestine felt stitched to the abdominal wound. By that means the patient was saved as much as possible from shock by the operation. He would ask Mr Miller if that would not be a good operation to perform?

Dr Charles Kennedy said that last week he had a case of intestinal obstruction. The first symptoms were vomiting and constipation, obstinate. Strong purgatives had been given without effect. When he saw the child an enema gave a fair motion, and apparently gave the child complete relief for over twelve hours. After twelve hours, however, severe pain came on again, the child screaming, and symptoms of obstruction recurred. The most important point was that tenderness was present, indicating local peritonitis over the right flank. The vomiting was only present for a short time. Pain apparently was intermittent. He was unable to make up his mind as to the exact nature of the case. The child suddenly died, and on making the post-mortem he found that the cause of obstruction was an adhesion to the small intestine, quite near to the cæcum, and that the lower part of the ileum had slid through. The peritonitis was quite local, and was caused apparently by a caseous gland, of which there were a good many in the abdomen, to which the omentum had become adherent. What misled him largely in the diagnosis was the feeling of resistance in the right flank, the tenderness there, and the dulness on percussion in that position. Of course it was in his mind at the time to have operative interference, but the sudden death of the child prevented that.

Mr F. M. Caird recollected being called to operate in a case supposed to be volvulus. He could not come to the conclusion that it was so, but opened the abdomen, and met with a great mass of tubercular glands, and it was easy then to make the diagnosis of commencing tubercular peritonitis. In another case the diagnosis arrived at was probable intussusception, but after waiting, the child was relieved of a large mass of impacted fæces under chloroform. As an example of obstruction due to malignant disease, he mentioned a case in which he cut out a section of gut, including an annular stricture of the descending colon, with deposits at a higher level. In two cases associated with inflammation of the vermiform appendix, the pain was referred to the region of the umbilicus, and it was only as the cases advanced that one was able to notice the fixation of the abdominal parietes in the iliac region during respiration. What struck one in many of these cases was that occasionally temperature was not very high, and that although the patient perhaps experienced great relief after a copious and extremely offensive stool, immediately afterwards there was not a relief, but a development of at least peritonitis; and the patient sank, and then it was found that there had been an abscess. One would be tempted under those circumstances, in the event of such a history, and in the event of being able to localize, where it was not taken at the beginning, to make an incision in the region of the appendix, and to try to enter the cavity, and to wash it out and introduce a drainage-tube prior to the rupture, and then to go on and open in the middle line and wash it out. He was sorry he could not say that he had met with any success whatever in the performance of abdominal section for the relief of intestinal obstruction.

Dr Harold Stiles put a question regarding the pathology of intussusception. He wished to know, in those cases of intussusception beginning at the ileo-cæcal valve, what part of the gut was felt *per rectum*, and if the ileo-cæcal valve could be felt in that position?

Dr Scott Lang said Mr Treves's view in cases of intussusception at the ileo-cæcal valve was that the intussusception took place at the expense of the entering tube, and the position of the large intestine remained constant. There were three tubes—the receiving, the entering, and the reflected tube. When intussusception took place at the expense of the entering tube, and the ascending colon remained in its place, the ileum was practically doubled down upon itself; but in all other cases the intussusception went on at the expense of the reflected and receiving tubes. He was not sure, however, that this had been verified by post-mortem.

Dr Duncan mentioned a case in a child on whom he performed laparotomy, and reduced the intussusception by compressing upwards. He could feel it through the rectum, and when he opened the abdomen in one case he found a very remarkable state of affairs. The small intestine was invaginated, but the cæcum and the small intestine had passed into a second intussusception. There was a double intussusception, first the small intestine right round, and then a second.

Mr Miller, in concluding the discussion, said that since he had opened the debate, he had looked up Treves's article in the *Dictionary of Surgery*, and he found everything that he could imagine laid down in the most succinct and complete manner as regards the surgical aspect. Everything, he thought, had been written that could be written upon this subject, and he was very much pleased that the idea of giving prominence to the diagnosis had proved of value in bringing forth so many valuable remarks.

SESSION LXVIII.—MEETING XIII.

Wednesday, 3rd *July* 1889.—Dr Smith, *President*, *in the Chair*.

I. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—Hugh Jamieson, M.B., C.M., Edinburgh, and David Gair Braidwood, M.B., C.M., Bonnyrigg.

II. Exhibition of Patient.

Mr Francis M. Caird showed a case of TRANSPLANTATION OF BONE AFTER COMPOUND FRACTURE AND TREPHINING. T. L., æt. 27, private Cameron Highlanders, was admitted to the Royal Infirmary early on the morning of 24th March 1889, suffering from a compound depressed fracture on the *right side* of the head, rather in front of the upper part of the coronal suture. The patient was unable to speak, but he looked quite intelligent, and carried out various actions, such as putting out his tongue, raising his hand, etc., when asked to do so. Under chloroform, it was found necessary to remove two circles of bone and an intervening bridge before the dura rose to its normal level, and before sufficient access could be obtained to get rid of detached fragments. The trephine discs were cut up after Macewen's method and replaced on the dura; the flaps were laid over them and sutured. A further examination now showed a cause for the aphasic condition of the patient in the shape of a small penetrating wound on the *left side* of the head, 3½ inches above and ½ inch in front of the external auditory meatus, and this led down to a punctured fracture. An incision permitted the elevation and removal of the depressed fragments, and an aperture in the skull large enough to admit the finger tip was left. The dura had been wounded. As some hair and grit had been driven into the wound, it was not thought safe to attempt transplantation of the bone. Towards morning the patient became conscious, and could speak. He gave an account of the assault by which the fractures were caused, and expressed surprise that he had been unable to speak, but stated that he felt assured that he could have written what he desired to say. Recovery was uninterrupted, the wounds remaining aseptic. At present there is no formation of bone at the left wound, and the cicatrix pulsates distinctly. On the right side, however, the gap is largely filled by bone, and there is only slight pulsation.

III. Exhibition of Pathological Specimens.

1. *Dr Maxwell Ross* showed a specimen of EPITHELIOMA OF THE LARYNX which had been removed from the body of a Goorkha soldier at Mandalay by Dr W. B. Bannerman, of the Indian Medical Service. The whole left side of the larynx from the ary-epiglottic fold to a point half an inch above the vocal cord was involved in the disease, as also the arytenoids of both sides. On the outer aspect of the box of the larynx a mass of cancerous infiltration had formed. Below this, and partly in its substance, was an abscess cavity which extended between the cricoid cartilage and lower border of the wing of the thyroid. Beneath the left arytenoid was an ulcerated spot communicating with the abscess, through which it had burst into the larynx, causing death by suffocation. The left inferior corner of the thyroid cartilage was eaten away, and dislocation of the crico-thyroid articulation produced. The thyroid gland was greatly enlarged. The case was interesting from the complication of the malignant disease with abscess and bronchocele. A full account of the clinical symptoms and post-mortem appearances was published in the *Brit. Med. Journal* for 22nd September 1888.

2. *Dr Harvey Littlejohn* showed—(*a.*) A RUPTURE OF THE HEART through the wall of the left ventricle. The specimen was obtained from the body of a man, æt. 64, a patient of Dr T. A. G. Balfour. Up to a week before death he had not shown signs of heart affection. He then felt pain in the præcordial region, for which Dr

Balfour ordered rest in bed. The day before death a peculiar whirring murmur was heard a little below the apex beat. On the following morning, when raising himself in bed to have his breakfast, he suddenly expired. The rupture of the left ventricle was found post-mortem. (*b.*) An ANEURISM OF THE AORTA from the body of a man who fell down dead in the street. Blood was seen to come from his mouth, and post-mortem his stomach was found to contain about a gallon of blood. There was also a rupture of the mucous membrane at the cardiac end of the stomach about two inches in length, evidently caused by the pressure of this large quantity of blood. The aneurism had eroded the vertebræ from the eighth to the eleventh, and had ruptured into the œsophagus.

IV. ORIGINAL COMMUNICATION.

Dr D. Berry Hart read his paper on THE MINUTE ANATOMY OF THE PLACENTA IN EXTRA-UTERINE GESTATION, which appeared at p. 344 of this Journal.

Dr Barbour considered that the important point Dr Berry Hart had brought forward in his paper, and been the first to demonstrate, was that gestation might go on to an advanced period when the fœtus was extra-peritoneal. He was not quite clear that in Fallopian-tube gestation the fœtus should be considered a displacing cause, as advocated by Dr Hart. It lay in amniotic fluid, and was unable to cause displacement in the way suggested. It was a very remarkable thing to have cellular tissue, as in the broad ligament, taking on the functions of the uterine mucous membrane, and supplying a maternal placenta for the extra-uterine fœtus. He was not certain that there was proof to show that pregnancies in which the placenta was found attached to abdominal wall or intestines were Fallopian-tube gestations to commence with.

Dr Haycraft thought the more one could learn about the placenta in these abnormal conditions the better would its functions in the uterus be understood. It seemed to him that the placenta in its new position performed exactly the same functions as in utero. The interest of this was as bearing on the question of uterine glands. In the Fallopian tubes they had a structure very similar to the uterine mucous membrane, but no glands. The glands in the uterus were supposed to secrete fluids, and he did not think every one was perfectly satisfied that they played no part in gestation. It seemed a matter of particular interest to find that in these cases, practically without the assistance of uterine glands, in simple connective tissue the fœtal villi were able to produce a placenta without the same structures one found in the uterus.

Dr Berry Hart, answering Dr Barbour's criticism as to the fœtus being a displacing cause, held that in extra-peritoneal extra-uterine gestation there was little or no liquor amnii. He could see no other explanation for cases of extra-uterine pregnancy with extra-peritoneal abdominal wall attachment of the placenta than that it had begun in the Fallopian tube, passed between the layers of the broad ligament, and developed extra-peritoneally. He had not fully considered the question of the functions of the placenta in connexion with these investigations, but the Fallopian tube structure was histologically similar to that of the uterine mucous membrane. Perhaps the use of the term "uterine glands" was a begging of the question, as they were little more than pits or depressions in the mucous membrane, and similar pits were found in the Fallopian tube.

Part Fourth.

PERISCOPE.

MEDICAL PERISCOPE.

By Francis Troup, M.D.

Berlin. Klin. Wochenschrift, No. 20.—Paul Guttmann relates his experience of "Hydracetin," or as known here by the name of "Pyrodin," as an antipyretic. After one dose of 10 centigrammes, and two hours later another 5, the temperature falls in from two to three hours as much as 2 to 3 degrees Centigrade. It only remains a very short time at the deepest level, and in two or three hours is as high as before the medication was begun. It is possible, by administering one 15 centigramme dose, in most cases to lower the temperature for a period of six hours.

The fall of temperature is accompanied by more or less perspiration, which ceases when the temperature minimum is reached.

The pulse and respiratory rate also fall, the latter, of course, only if due to the previous high temperature.

There is no shiver or rigor when temperature again begins to rise. No alteration of the normal course of feverish diseases seems to follow the use of hydracetin any more than other antipyretics. In cases of acute rheumatism, in doses of 10 to 20 centigrammes pro die, it seemed like other similar remedies to relieve the joint pains, but did not prevent the invasion of new regions. The same is true in sciatica. The strongly reducing properties of hydracetin induced a trial of it outwardly in psoriasis; a ten per cent. ointment was used, with the result of healing in about a week the cases experimented on.

It was noticed in several cases where it was used internally that the face became very pale and anæmic, although neither hæmoglobin nor albumen was found in the urine. Guttmann believes that the drug caused a blood dissolution, and recommends caution in its use. The dose for adults should not exceed 1 gramme pro die, and should not be given longer than three days consecutively, and then stopped for several days.

Ibidem, Nos. 19, 20, 21, 22.—Dr Sehrwald, in the laboratory of Professor Rossbach of Jena, has four papers on Naphthalin in typhus abdominalis, and finds—

1. Naphthalin retards the development of the micro-organisms of typhoid, of putrefaction, and of the fæces only in a minor degree at the temperature of a room.

2. Extent of surface of the naphthalin powder or thorough shaking up a naphthalin-containing fluid increases its disinfecting power.

3. At the body temperature the influence of naphthalin on all those sorts of bacteria is more intense, and thus it seems probable that it is as a gas that it is most destructive to germs.

4. It must consequently act, in liquids, more energetically on aerobes than anaerobes. It must also attack germs growing on a solid medium more vigorously than those living in liquids.

5. The conditions favourable to the effect of naphthalin are much more powerful in the bowel than in the test-tube. There is the body warmth to bring about a vaporizing of the drug, and peristalsis to cause a continual motion of the fluid contents, and bring new portions of them in contact with the naphthalin gas.

6. Naphthalin outside the body added to fæces reduces their germ contents by one-half. Given internally, it at first reduces them by one third, and then the germs gradually increase to their original numbers; probably because some of them are more resistant to the naphthalin than others and get the upperhand.

7. Naphthalin reduces the bacteria of the typhoid stools very considerably, even making them ten times less. The bacteria which had survived the exhibition of calomel, and were still present in the stool in a reproductive condition, were also notably damaged by the naphthalin.

8. Because typhoid is to be looked upon as a "mixed infection" ("*Mischinfection*"), the exhibition of naphthalin is strongly indicated from the beginning of the disease in the hope that the naphthalin gas, formed by the body heat, will be able to check the growth of the typhoid bacilli.

9. Since calomel also works in a disinfectant way on one set of the typhoid fæcal bacteria, and is inoperative on other sets which are more accessible to the working of the naphthalin, it seems rational to begin the treatment of this fever, as early as possible, with naphthalin plus calomel.

10. Part of the naphthalin is absorbed, changed in the body and separated by the urine; but those metabolic products present in the blood exercise no restraining influence on the typhoid bacilli. Sehrwald goes on to say that the aim of his researches has been to demonstrate that it is not so questionable as Fürbringer would make it out to be, this administration of naphthalin in typhoid. His investigations have proved the restraining power of naphthalin not only on the growth of typhoid bacilli, but also on that of the

other micro-organisms which the typhus stool contains. These experimental facts may also be true in the typhoid bowel, and entitle one to use the drug in the fever. Of course the ultimate verdict, whether naphthalin is to be practically useful or not, can only be given after extensive trial on typhoid patients.

Ibidem, No. 31.—A case of brain tumour in the klinik of Liebermeister in Tübingen is given in detail. A trauma 5 years before; giddiness when head was raised, none when lying down; walking impossible on account of giddiness; frequent vomiting; pupils small, reacting promptly to light, the left somewhat larger than right, congestion of optic disc. Diagnosis was made of a tumour, not at the base but in the cerebellum. At first the pulse was slow and temperature normal, but the heart's action became more frequent and irregular latterly. The giddiness increased so that the mere turning of the head brought it on. No paralyses came on of bladder or rectum. Three days before death temperature rose considerably (104°), and the pulse to 104 per minute, and the end came very suddenly.

At the post-mortem the cerebellum was found adherent to the dura in the region of the transverse sinus, and the left cerebellar hemisphere contained a rounded tumour 3 to 3½ centimetres in diameter. On section it was found to consist of nodules of a miliary character. It had evidently formed in the pia, compressed the cerebellar substance, and become adherent to the dura. In the neighbourhood of the tumour, the cerebellar substance was soft even to the middle line.

In the lungs, fresh deposits of miliary tubercles were found as well as old tubercular foci in both apices.

The course of events here seems to have been the following:—Old tubercular apical lung disease; partial healing of it; secondary infection of the cerebellum, and shortly before death, owing to the involvement of the sinus transversus with the tumour, an irruption of bacilli into the vascular system which had only time to cause the miliary tuberculosis of the lung before death happened. This lung tuberculosis also explains the fever of the last days.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. ALLAN JAMIESON, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

CLINICAL CONVERSATIONS AT THE ST LOUIS HOSPITAL, PARIS. —*Influence of Erysipelas on a Chancre of the Upper Lip.*—M. Besnier showed a young man, addicted to stimulants, who had a chancre of the centre of the upper lip, with ulceration and considerable œdema, and enlargement of the submaxillary glands on one side. Soon after he was shown he became affected with severe erysipelas of the head. This soon subsided under treatment, and the ulceration and œdema coincidently vanished, but the glands became inflamed and suppurated. He concludes that erysipelas appears to accelerate the cure of the initial lesions of syphilis as well as that of the more remote. There is an antagonism of the two irritant elements worth remembering in future.

Soft Chancres exhibiting Fictitious Induration of Inflammatory Origin, due to the Contact of the Urine.—M. Fournier has already insisted on this, and showed a patient with several chancres of the edge of the prepuce having the characters of the soft chancre, yet which to touch felt indurated. There was no glandular enlargement, and inoculation on the arm gave positive results. The induration was irritative, and caused by the urine.

Cicatricial Alopecia.—M. Besnier drew attention to a form of baldness due to the formation of scars, yet differing *in toto* from the destructive folliculitis described by Quinquand, and not due either to lupus erythematosus or to favus. An illustrative case showed all the front part of the scalp studded with bald patches, unequal in form and dimensions. They were somewhat serpiginous, and had a depressed, smooth, ivory-like centre entirely bald. At the margins of these the scalp was red, and showed here and there minute purulent deposits occupying a hair follicle, but superficial. Besides these there were parts denuded of hair, not to be distinguished from an ordinary alopecia areata. Hairs examined presented no other features than ordinary irritative lesions, and no micro-organisms save common ones were found. Besnier so far can only describe this as an unnamed form of alopecia. All treatment has failed. Any energetic medication only makes the process worse.

Traumatic Paradoxal Zoster.—M. Besnier showed a man who had received an injury to the left shoulder, and who subsequently developed a zoster of the right shoulder. On the 24th of last November he fell from a carriage, and sustained a violent contusion of his left shoulder. He applied tincture of iodine and of arnica. On the 4th December sharp pain was experienced in the right shoulder, and the following day groups of vesicles appeared on the subclavicular region of the same side. He was cramped and feverish. On the next day the eruption of zoster continued to come out round the shoulder. A furuncular ecthyma also appeared. Sensibility to pricking was diminished over all the affected region. Besnier described the case as one of zoster by transference. More than that, he pointed out how difficult it was to reconcile cases of traumatic zoster with the zymotic theory propounded by Erb and Landouzy.

Varieties of Syphilitic Cephalalgia.—M. Fournier remarked, "I have for long studied syphilitic headaches, and have endeavoured to classify them. There are several types. Primarily the neuralgic headache and the headache of osseous origin, but there are others. Among these there is, during the secondary period, a headache which I have named the neurasthenic cephalalgia of the syphilitic: for syphilis often, especially in women, arouses nervous phenomena. Hysteria in particular does so, and especially this neuropathic state which one calls neurasthenia. Among the complaints emanating from neurasthenia we find headache. This type is peculiarly rebellious; it persists during two or three months or more. The treatment which has succeeded best has been hydrotherapy. At another period of syphilis there is a headache which persists; it is the cephalalgia of tabes, the tenacity of which is well known. These two causes, then, must be remembered." Quinquand finds that the neuralgic form sometimes yields to potassium iodide. Failing this he employs atropine and gelsemium.—*Annales de dermatologie et de syphiligraphie*, February 1889.

THE TREATMENT OF SKIN DISEASES BY CHLORIDE OF HYDROXYLAMIN.—This substance (NH_2 OH HCL, hydroxyl-ammonium-chloride), was recommended by Binz for trial in the treatment of skin diseases on account of its resemblance to pyrogallic acid and chrysarobin in its active reducing powers. Eichhoff (*Monatshefte f. prakt. Dermatologie*, 1, 1889) treated five cases of lupus vulgaris, five of ringworm of the scalp, and one of parasitic sycosis of the face, with the following solution:—

℞	Hydroxylamin Hydrochlor.,		1.
	Spiritus vini,		
	Glycerini, . .	āā	500.

The affected parts were first well washed with soft soap, and the solution painted on three to five times daily. In one bad case of lupus hypertrophicus of the face the hypertrophy disappeared in eight days, and the healing of the sores quickly followed. The whole affected area was covered with a clear white scar within four weeks. Eichhoff did not, however, suppose that the case was finally healed in so short a time. Fabry (*Viertlj. f. Derm. und Syph.*, Heft 2, 1889) has made a long series of experiments on the action of this substance on psoriasis. On account of its very poisonous nature (narcotic, paralysing the nerve centres; in larger doses causing convulsions and death, Binz), no attempts were made to exhibit the drug internally. A spirit solution of 0·2–0·5 per cent. was painted on twice daily, or compresses wet with a 0·1 per cent. watery solution were kept continuously applied. Both solutions were neutralized before use by the addition of carbonate of lime. In action it has proved to be as powerful as chrysarobin or pyrogallol, and it has the enormous advantage over both, that it does not stain the skin or the linen. It must be administered with care, on account of its poisonous properties. Patients present very varying degrees of tolerance of its local action, some being unable to endure the pain which it causes, while others experience no unpleasant sensations. In case it causes any irritation, recourse should be had at once to a dusting powder, or mild (zinc) salve, until the skin is fit for the renewed application of the hydroxylamin. With care, the action of the drug is quite satisfactory. Fabry tried it in 24 cases—in 21 synchronously with chrysarobin, pyrogallic acid, and anthrarobin; like the two former, it cleared the skin in from four to six weeks, but did not prevent recurrences. Anthrarobin is much weaker than chrysarobin or pyrogallol, and fully as disagreeable.—*British Journal of Dermatology*, July 1889.

TREATMENT OF PSORIASIS BY IODIDE OF POTASSIUM.—Barduzzi (*Gazetta di Ospedali*, 17, 1889) has found that his experience of the effect of iodide of potassium tallied with that of Greve, Boeck, and Haslund. In three diffuse universal cases of very inveterate character, which had been treated with transient success by all the usual remedies, he obtained results from iodide of potassium which he had never hoped to get. In none of the cases was the amount of the drug larger than 105 grains daily.—*British Journal of Dermatology*, July 1889.

TREATMENT OF RINGWORM OF THE SCALP.—Leviseur recommends a lotion consisting of simple tincture of benzoin and perchloride of mercury in the proportion of 1 to 300, applied by means of a soft nail brush at intervals of a week. When the irritation occasioned by the lotion had subsided, as was usually the case by the next day, a 10 per cent. salicylic acid ointment in vaseline was rubbed in. The strength of the lotion and ointment was doubled in course of time. The hair was kept short, and the worst of the patches were epilated as a preliminary. In all, 98 children, whose ages ranged from 6 to 16 years, were treated. Treatment by this method was begun in the commencement of October, and nearly half were cured by the end of February. (As the relative numbers at each age are not stated, the results obtained do not appear much better, if so good, as by other means, W. A. J.).—*New York Medical Record*, 1st June 1889.

TRICHORREXIS NODOSA.—This condition in which the hairs, usually of the beard alone, seem swollen at intervals, as if studded with white specks, and which break off easily at these points, has always been difficult to treat. Roeser recommends that the beard should be kept short, and that tincture of cantharides should be applied very carefully to the roots of the affected hairs by means of a fine brush.—*Annales de dermatologie et de syphiligraphie*, April 1889.

SOAPS: PARTICULARLY MEDICINAL ONES.—Eichhoff has taken up this question at the request of Unna, and has materially added to our acquaintance with it. After pointing out how much the public are imposed upon by specious advertisements, he shows that a good soap should remove the superficial fat and the accumulated dirt without injury to the epidermic cells. The lubrication of the horny layer, according to the most recent researches, is effected by the imbibition of fat from two sources—from the eleidin originating in the epithelium, and the fatty secretion of the oil glands. This fat forms the natural protection of the horny layer, and a soap which contains any free alkali not only removes this but acts destructively on the superficial cells. All soaps made of, or which contain cocoa-nut oil are unsuitable for the skin. Besides its cheapness, cocoa-nut oil saponifies at a much lower temperature than other fats, but it needs a stronger ley. Such cocoa-nut soaps lather well, and clean the skin admirably, but in consequence of their high percentage of free alkali, they remove with the dirt some of the outer layers of the skin and leave it "chapped." Most persons who suffer from chapped hands owe this to the use of an unsuitable soap. To prove the neutrality of a soap, a simple test is applicable. A little of a hot solution of corrosive sublimate is allowed to drop on a freshly cut dry surface of the soap. Should a trace of yellow appear, due to the formation of oxide of mercury, free alkali exists in the soap; it is not neutral, as it should be. A neutral soap ought not to cause a pricking sensation when touched by the tongue (or cause the least smarting of the eyes when the face is washed with it). It is not, however, easy to make, and the price is necessarily high. It can be made by Liebreich's method of double centrifugal action. But of most value are the over-fatty or superfatted soaps introduced by Unna. The formula for such is—

Best beef tallow,		59·3 per cent.
Olive oil,		7·4 per cent.
Soda ley,	38°	22·2 per cent.
Potash ley,	Beaumé	11·1 per cent.
		100 per cent.

In the preparation of this, about 4 per cent. of the olive oil remains over unsaponified in excess. This is the basis of the various medicinal soaps now to be referrred to.

PERISCOPE OF LARYNGOLOGY AND RHINOLOGY.

By J. Maxwell Ross, F.R.C.S. Ed.

Osteomata of the Nasal Cavity.—Dr Fred. Kammerer of New York, writing in the *Annals of Surgery* for August 1889, refers to cases recorded by Legouest, Tillmanns, Habermaas, Mott, and Fenger, and reports one which came under his notice at the surgical clinic in Freiburg. The patient was a girl, aged 17, who had sustained an injury to her nose by a fall six years before. Symptoms of right nasal obstruction occurred about four and a half years after the fall. When admitted to the clinic the right nostril was completely blocked by a tumour covered with mucous membrane, which was visible both anteriorly and posteriorly (rhinoscopically), and could be felt by the finger passed behind the soft palate. On opening into the nasal cavity the tumour was found attached to the frontal or ethmoid bone only, and this attachment was readily severed by a few strokes of the mallet and chisel. Before it could be removed the soft parts had to be reflected from the superior maxilla, about half-an-inch of which was then chiselled off between the infraorbital margin and the nasal spine, after which the aperture was found sufficiently large to admit of the tumour being seized by a forceps and removed. The turbinated bones had disappeared, as also the antrum of Highmore, while the septum was deflected to the left. The orbital plate of the ethmoid and the lachrymal bones had also disappeared. The patient made a good recovery. After maceration and section the tumour proved to be an ivory osteoma weighing 2¼ oz., in which no nucleus of spongy tissue was discernible.

A New Insufflator.—Dr Philpots in the *Provincial Medical Journal*, August 1889, figures and describes a new instrument for insufflation of suitable powders in the early stages of nasal catarrh and hay-fever. The advantages claimed for it are that it is small enough to be carried in the waistcoat pocket, and that it may be kept charged with the powder ready for use. By an ingenious arrangement of a screw-lid, the powder is prevented falling out or becoming damp. Dover's powder is recommended for insufflation in nasal catarrh; while for hay-fever either of the following two are said to be of use:—Sodæ salicylat ℈iv.; acidi borici (pulv.), ℥j.; cocain hydrochlor, grs. 22, M.; or of salol and boric acid, equal parts.

Two Cases of Scald of the Upper Air Passages from inhaling Steam.—Dr Sutliff of Sacramento, Cal., reports these in the *Occidental Medical Times*, July 1889. Both patients were plumbers, and had been working at the water-back of a stove, the pipes of which were obstructed. The first man, after blowing into the pipe, unmindful of the high temperature of the water, began to suck it, when his mouth was suddenly filled by a jet of steam and water. Tracheotomy at one time seemed necessary, but in less than twenty-four hours the breathing began to improve, and in a few days he had quite recovered. The second patient, while working at the same pipe in the afternoon of the same day, got some hot water into his mouth, but not of a higher temperature than was sufficient to cause a smart pharyngitis.

Mercury Biniodide in Throat Affections.—Dr Illingworth in the *Provincial Medical Journal*, Aug. 1889, recommends that the biniodide should be prepared by adding a solution of sodic iodide to the solution of mercury perchloride. He advocates a strength of 1 in 2000 for use as a spray in scarlet fever and diphtheria, and as an inhalation and douche in hay-asthma; and of 1 in 4000 as a spray in ozæna.

THE TREATMENT OF LARYNGEAL GROWTHS.—Dr Barclay J. Baron (*Bristol Medico-Chir. Journ.*, June 1889) relates several interesting cases of benign sessile growths in which removal by instruments was impossible, but where frequent applications of astringents, such as solutions of zinc chloride and of iron perchloride, compresses, inhalations of vapor bezoin, and rest of the voice, proved of the greatest value. In one instance he first rubbed off the growth and applied the astringent solution to the raw surface. (He does not say whether he has any objection to the use of the galvano-cautery in these cases.) The growths sometimes seen in phthisical larynges he considers are best left alone; but when ulceration takes place, lactic acid is the best local medication. In syphilis (tertiary) and malignant disease of the larynx tracheotomy ought to be performed much earlier than it usually is.

CURE OF THE FALSETTO VOICE.—Dr J. C. Mulhall of St Louis reports (*Intern. Journ. of the Med. Sciences*, Aug. 1889) the case of a man æt. 25, in whom the puerile voice persisted. Though it was high-pitched and squeaky, the important fact was elicited that in laughing it was of low pitch. Two laryngologists had diagnosed paralysis, and applied electricity. Dr Mulhall, however, observed that, so far from there being any paretic condition present, the true and false cords approximated during phonation more closely than they should have done. There was apparently hypertension of the thyro-arytenoid muscles. A cure was effected by teaching the patient to speak in low tones. The same condition was observed in other two cases, and a similar treatment successful.

ANÆSTHETICS IN NASO-PHARYNGEAL OPERATIONS.—Dr J. F. W. Silk deals with this subject in the *Journal of Laryngology and Rhinology* for May and June 1889. He dismisses the local anæsthetic cocaine in a rather summary fashion, on the grounds that he has little personal experience of it, and that it is unsatisfactory and uncertain. For operations of a very short duration, such as tonsillotomies, he prefers nitrous oxide, as "it is at once pleasant, certain, and safe, and can be administered in any position of the patient." Ether may be used as an adjunct in the production of anæsthesia by nitrous oxide gas, or alone. Its use is preferable to chloroform because of its greater diffusibility, and therefore greater safety and the more rapid recovery from its effects. The spasm and bronchial irritation usually ascribed to it are neither so important nor do they appear to occur so frequently as is usually taught. Chloroform vapour being heavier tends to collect in the air-passages in a concentrated state. Hence the attacks of syncope and apnœa occurring some time *after* the administration has been withdrawn. The position of the patient will sometimes influence the choice of the anæsthetic. Chloroform can be given only in the recumbent posture. This is usually supine, because though the prone position would facilitate the removal of blood, and prevent the falling back of the velum palati and tongue, the compression of the chest would materially embarrass the respirations. When the supine position is adopted, the primary narcosis may be rendered more profound. But the question then arises, whether it is better to put the patient so far under the influence of the anæsthetic as to abolish all reflex acts, or to put him over just as far as to prevent pain, but retain for him the power of swallowing accumulated blood and mucus. When ether is the anæsthetic, Dr Silk has no hesitation in pushing the administration to its extreme limit, because with the mouth widely open the glottis can be easily commanded and the blood removed almost as quickly as it collects, while the extreme diffusibility of the ether vapour greatly reduces the risk of over-anæsthetization. With chloroform the alternative procedure is the correct one, he thinks, though it is the more troublesome, and requires extreme vigilance on the part of the administrator. For slight operations, when profound anæsthesia and complete relaxation are not required, the sitting posture is a good one. The narcosis may be maintained for some time, and there is less danger of over-anæsthetizing the patient, but, as already stated, chloroform cannot be used. For the removal of adenoid growths by scraping, the patient may be first anæsthetized while sitting, and his body then bent forwards so that his head is over a basin placed between his knees. The operation is then performed, the blood flowing out by the side of the surgeon's finger. Ether may be given in small quantity along with nitrous oxide gas, by arranging a modified Clover's portable ether chamber between the face-piece and gas-bag. The gas is given first by itself, then passed over the ether before being inhaled. Ether may also be given to maintain anæsthesia induced by chloroform, and is then administered effectually upon a Skinner's cage, but primary narcosis is difficult to produce by this means.

In the following (July) number of the *Journal*, the same writer considers the importance of nasal obstruction in its relation to the administration of anæsthetics. Some of the asphyxic phenomena he believes to be due to this rather than to the so-called aryteno-epiglottidean relaxation, and insists on the importance of keeping a free oral airway when an anæsthetic is being administered.

PERISCOPE OF GYNÆCOLOGY AND MIDWIFERY.

By J. MILNE CHAPMAN, M.D., Inverness.

NEW USES FOR THE SHARP CURETTE (Geo. J. Englemann, *St Louis Polyclinic*).—The author states that his views of this subject and his practice differ widely from those generally accepted and taught in this country. He professes a decided preference to the sharp curette, saying that one might as well recommend a dull knife as a dull curette, and that no one should use it who cannot use it properly; while, when properly used, the sharp instrument possesses many advantages, being more effective, and admitting of greater precision in its use. He says that he now uses the instrument for the treatment of many forms of uterine disease which have hitherto been the most tedious and unyielding, such as flexions resulting from metritis and endo-metritis, and some forms of endometritis and of subinvolution, especially after miscarriage. He lays great stress upon the secondary results of the operation—that is, the process of absorption or involution which frequently follows the operation—and attributes to this the excellent results in a case of chronic interstitial endo-metritis and displacement. He invariably precedes the performance of Emmet's operation by thoroughly curretting, and attaches to this proceeding great importance. He performs the operation under chloroform, and with the strictest antiseptic precautions.

BELLADONNA AND CANNABIS INDICA USED BY THE RECTUM (Farlow, *Boston Med. and Surg. Journal*).—The writer recommends that the above-named drugs should be used when required in gynæcological practice by the rectum rather than as pessaries or by the mouth. He draws attention to their sedative action on the uterus and pelvic contents; their relaxing effects on the bowels, and their value in irritable conditions of the bladder and urethra, as also in cases of sensitive and painful ovaries. He believes that in the nervous headaches of women with pelvic troubles Cannabis indica has no equal, while in the vague neuroses of the menopause both drugs are extremely useful. He employs them in doses of a quarter grain each of the extracts nightly, or night and morning.

VENTRAL FIXATION OF THE RETROFLEXED UTERUS (Leopold, Volkmann's *Sammlung klinischer vorträge,* No. 333).—Nine cases coming under the personal care of Leopold are described, with their clinical histories and the technique of the operations performed. The indication for surgical interference in three cases was pure retroflexion, the uterus being in a state of chronic inflammatory swelling and sensitiveness; in one the retroflexion was caused by a subserous myoma of the fundus; and in five the ovaries and tubes were bi- or unilaterally inflamed and matted to the uterus. All cases were followed by uniformly good results. Leopold does not decry non-surgical treatment for this affection; on the contrary, it is good practice to make use of the various local and systemic methods, particularly the patient and judicious use of pessaries and the more lately introduced massage, which every practitioner resorts to often with good success. Those possessing the time and means, and whose surroundings and mode of life will insure a painstaking course of treatment, will undoubtedly be able to defer the operation. But among the less fortunate—those in whom daily tasks, manifold cares, and lack of means interfere with their proper regimen—will find a boon in this operation, which cures in a short time, and cures permanently. Leopold's cases show that the following may be taken as indications for the performance of the operation:—(1), First of all, n performing oöphorectomy or salpingotomy for the relief of chronic oöphoritis or salpingitis, whether the retroflexed uterus is bound down by adhesions or not; (2), in the removal of growths which have induced permanent retroflexion (subserous myomata of the uterus, tumours of the ovaries); and (3), in pure retroflexion of the mobile and non-adherent uterus, when the patient's condition is to be attributed solely to this, and when all other methods for giving relief have had a thorough trial. In the first class of cases we have to do mainly with infectious endometritis, salpingitis, oöphoritis, and perimetritis of gonorrhœal or puerperal origin, hence the removal of both ovaries is imperatively called for, even though the uterine adnexa of one side are as yet in a state of commencing inflammatory swelling and perimetritic adhesions. In the second and third classes the ovaries and tubes will generally be found healthy. Here we should be guided by conservatism. He then describes the technique of the operation, and concludes with the declaration that it leaves us still far removed from the ideal.

ANTIPYRIN CONTRAINDICATED DURING MENSTRUATION (Huchard, *Philadelphia Med. News*).—The writer reports the following case:—He administered to a woman suffering from severe dysmenorrhœa 15 grains of antipyrin. After a short time the menstrual flow ceased, the patient was taken with a severe chill and chattering of the teeth, the extremities and face became cold and cyanotic, and there occurred repeated fainting fits. Soon the face became red and flushed, the pulse small and soft, and the patient complained of severe headache. She recovered. The writer has observed several other cases with less severe symptoms, and now does not administer antipyrin during the first two or three days of the catamenial epoch.

INTRA-UTERINE INFECTION WITH PNEUMONIA (Netter, *Progrès Méd.*)—The writer relates a case of a woman who gave birth to a child when she was suffering from acute pneumonia. The child lived five days, and at the post-mortem examination pneumonia of the apex of the right lung was found, along with double pleurisy, suppuration, pericarditis, and cerebro-spinal meningitis. A microscopic examination showed the pneumococcus. The rarity of such pneumonia at that age contributes to the support of the conclusion that it was in this case due to infection from the mother. A similar infection of the fœtus has been observed in rabbits, guinea-pigs, and mice, when the pregnant animal has been infected with pneumonia. It seems sometimes to be the case that the pneumococci may produce pneumonia in the mother and pericarditis in the child; or inversely, suppurative meningitis in the mother and pneumonia in the child. Netter has only found three cases of this pneumonic infection in women.

BROMIDE OF POTASSIUM IN OVARIAN ACNE (Arthur Jamison, M.D., *Practitioner*, May 1889).—The writer details a case at length, and refers to two others where acne was associated with profuse menstruation, and disappeared under the use of potassium bromide, though special local treatment had been continuously tried before without any appreciable benefit. One case had the bromide rash produced, but there was no difficulty in differentiating one rash from the other, inasmuch as the medicinal rash never affects the sebaceous glands. He says that we may safely conclude that amongst the causes of acne in women ovarian congestion is one, and that when acne is due to that cause, potassium bromide is at present our most efficient remedy.

On the Indications for Laparotomy in Acute Processes (Gersung, *Wiener Med. Presse*).—The writer collects all those cases under the name of acute in which danger of a sudden fatal termination compels the physician to decide immediately whether a laparotomy should be made, or whether other remedies are to be used which place less responsibility upon the physician, and endeavours to decide at what moment safety can be expected only from operation, or how long it may be deferred without depriving the patient of this last possibility of a cure. Laparotomy should be performed for injuries to large vessels or to organs of the abdominal cavity containing many bloodvessels; traumatic, or, in very rare cases, spontaneous rupture of liver or spleen; rupture of the fœtal sac in tubular pregnancy; dangerous vomiting of blood, and perhaps also a bleeding carcinoma of the stomach. As to peritonitis, laparotomy is of value chiefly as a preventive operation; in general peritonitis already existing the prospect of success is very limited. Laparotomy is indicated whenever the abdominal cavity is opened (by wounds) in order to prevent septic processes, and in perforation (also traumatic) of intestines, rupture of an abscess and flow of pus into the peritoneal cavity, rupture of the bladder, impacted gallstones, obstructions in the intestines, whether caused by invagination or volvulus, by strictures resulting from scars or new formations, by obstruction from an internal hernia, or by foreign bodies that entered into the intestinal channel from outside, or by a gall-stone remaining in the intestine, by sloughing of a portion of an intestine as a result of inflammatory processes, or by inherited malformation. It seems to be generally admitted that laparotomy and suture of the intestines for gun-shot wounds can only be performed with any hope of success within eighteen hours of receipt of the injury.

Part Fifth.

MEDICAL NEWS.

Royal Colleges of Physicians and Surgeons of Edinburgh, and Faculty of Physicians and Surgeons of Glasgow.—The following candidates passed their final examination for the triple qualification in Medicine and Surgery at the sittings held in October 1889, and were admitted L.R.C.P. & S.E., and L.F.P. & S.G.:—Mary Anne Smith, Bacup; Charles William Percival Orr, Colombo; William John Leitch, County Tyrone; David Richmond Johnston, County Tyrone; Eveline

Ada Cargill, London; Eleanor Sophia Warner, Winchester; Agnes Elizabeth Henderson, Aberdeen; Orby Russell Morgan Wood, Blackheath; Richard Drinkwater, Llangollen; Edward Gray, Kirkcaldy; William Lawton, Yorkshire; Alfred Benedict Santiagopulle, Ceylon; Margaret Mary Sharpe, Cumberland; Herbert Henry Hodgetts Addenbrooke, Smethwick; Henry Reynell Bellamy, Cheshire; Patrick James Byrne, County Cork; Matthew Wiseman, County Cork; Hugh Ferrers Knyvett, Windsor; Robert Johnstone Stirling, Peebles; Godfrey William Waller de Courcy Baldwin, Tipperary; Robert Love, County Antrim; Henry Robert Wray Strange, Madras; Thomas Sale Pennington Sparrow, Southsea; Archibald Robertson Douglas, Newcastle-on-Tyne; John Francis Roden, Edinburgh; James Patrick Kelly, Melbourne; Augustus Lower Paliologus, Calcutta; John Columba Byrne, County Cork; Henry Francis Townshend Chambers, New South Wales; Samuel Arthur Ewing, Melbourne; John Walter Corns, Manchester; Richard Kingston Bird, Melbourne; Hugh M'Ewen, Beckwith; James Simpson Steele, County Antrim; Henry Ernest Wells, Maulmain, Burma; Clarence George Godfrey, Melbourne; Adam Edward Hilker, Port Elgin, Ontario; Arthur Beecroft, Hyde, Cheshire; Dominick Hugh M'Closky, Penang; Thomas William Fisher, Belfast; John Stewart, County Tyrone; John Charles Hetherington Beaumont, India; Harry George Tillman, Worcestershire; Daniel Kerr, Largs; William Matthews Joyce, Ashby-de-la-Zouch; and James Edmund Leach, Rochdale.

MEDICO-CHIRURGICAL SOCIETY OF GLASGOW.—At the first meeting for the Session, held on the evening of the 4th October, the following gentlemen were elected office-bearers for session 1889–90:—*President*, Dr William Macewen. Section of Medicine:—*Vice-president*, Dr J. Wallace Anderson; *Council*, Dr Hugh Thomson and Dr W. G. Dun; *Secretary*, Dr Geo. S. Middleton. Section of Surgery:—*Vice-president*, Dr David W. Knox; *Council*, Dr W. J. Fleming and Dr J. C. Renton; *Secretary*, Dr John Barlow. Section of Pathology:—*Vice-president*, Dr W. T. Gairdner; *Council*, Dr H. Rutherford and Dr Joseph Coats; *Secretary*, Dr T. Kennedy Dalziel. Section of Obstetrics:—*Vice-President*, Dr W. L. Reid; *Council*, Dr M. Cameron and Dr Samuel Sloan; *Secretary*, Dr Lawrence Oliphant; *Treasurer*, Dr Henry E. Clark; *General Secretary*, Dr Walker Downie.

THE OLD OAKEN BUCKET.

(As Revised and Edited by a "Sanitarian.")

WITH what anguish of mind I remember my childhood,
Recalled in the light of a knowledge since gained;
The malarious farm, the wet, fungus-grown wild-wood;
The chills then contracted that since have remained;
The scum-covered duck pond, the pig-sty close by it,
The ditch where the sour-smelling house drainage fell;
The damp, shaded dwelling; the foul barn-yard nigh it;
But worse than all else was that terrible well;
And the old oaken bucket, the mould-crusted bucket,
The moss-covered bucket that hung in the well.

Just think of it! *Moss* on the vessel that lifted
The water I drank in the days called to mind,
Ere I knew what professors and scientists gifted
In the water of wells by analysis find.
The rotting wood fibre, the oxide of iron,
The algæ, the frog of unusual size,
The water, impure as the verses of Byron,
Are things I remember with tears in my eyes.

And to tell the sad truth—though I shudder to think it—
I considered that water uncommonly clear,
And often at noon, when I went there to drink it,
I enjoyed it as much as I now enjoy beer.
How ardent I seized it with hands that were grimy,
And quick to the mud-covered bottom it fell;
Then reeking with nitrates and nitrites, and slimy
With matter organic, it rose from the well.

Oh! had I but realized, in time to avoid them,
The dangers that lurked in that pestilent draught,
I'd have tested for organic germs and destroyed them
With potassic permanganate ere I had quaffed;
Or, perchance, I'd have boiled it, and afterward strained it
Through filters of charcoal and gravel combined;
Or, after distilling, condensed and regained it,
In potable form, with its filth left behind.
How little I knew of the dread typhoid fever
Which lurked in the water I ventured to drink;
But since I've become a devoted believer
In the teachings of science I shudder to think.
And now, far removed from the scenes I'm describing,
The story for warning to others I tell,
As memory reverts to my youthful imbibing,
And I gag at the thought of that horrible well,
And the old oaken bucket, the fungus-grown bucket—
In fact, the slop bucket—that hung in the well.

Brooklyn Medical Journal, April 1889. J. C. Bayles.

CORRESPONDENCE.

(*To the Editor of the Edinburgh Medical Journal.*)

MEDICAL AID ASSOCIATIONS AND FRIENDLY SOCIETIES.

Sir,—An attempt having been made here to start a branch of the "National Medical Aid Company" and a Medical Aid Fund for the families of a lodge of Oddfellows, a meeting of the medical men of this district was called to discuss the question, and, as a result, the following pledge has been signed by 21 out of 23 of those either in or retired from practice; one of the exceptions is a retired surgeon, the other a practitioner who has promised to sign it as soon as he is free from his medical refereeship, of which he has sent in his resignation to the National Medical Aid Company.

Such societies are so pernicious, and have been so disastrous elsewhere to our profession, that we felt it necessary to make a stand against their establishment here, and a copy of the pledge is sent to you for publication with this, in order that our profession in other places may be encouraged and strengthened against such combinations against its welfare.

"We, the undersigned Members of the Medical Profession of Malvern and its neighbourhood, taking into consideration the fact that there are well-established Provident Dispensaries in our midst, pledge ourselves not to undertake the care of families of members of Friendly Societies at a contract rate of payment."—Yours faithfully, Stanley Haynes, M.D.

Malvern, 30th September 1889.

Part First.

ORIGINAL COMMUNICATIONS.

I.—ADDRESS TO THE MEMBERS OF THE ROYAL MEDICAL SOCIETY on the Occasion of the Opening of the Society's 153rd Session.

By Professor Sir Douglas Maclagan, M.D., F.R.C.P. Ed.

I suppose that it will not surprise you if I say, that when I was asked to give the opening address of the Royal Medical Society I took a day or two to think over the proposal.

There were two conflicting considerations. First, that by a specially heavy summer's work as a teacher and examiner I had earned the right to give myself a long holiday, and had no need to undertake any fresh duty. But, *per contra*, there was the desire on my part to comply with the expressed wishes of a Society which had bestowed upon me a full share of its honours, and to which I stood indebted for the great benefits which it had bestowed on me during my student years. This latter consideration prevailed, and I am here to-night to comply with your request. But this was only the beginning of fresh difficulties, because during a rather long excursion on the Continent, enjoying the beauties of Stockholm and breathing a little of the academic air of the time honoured University of Upsala, I had other things to engage my attention than the thought of an introductory address, and could not very well make up my mind as to what should be the subject of it. I own to a small mental rigor when I found myself fairly confronted with the announcement of my name in full capitals and red ink, to meet you here this evening. After many hesitations, however, it occurred to me that I might set before you one or two reminiscences of former days, which might serve as pegs on which to hang up some observations, which might possibly suggest topics for papers or discussions at some of your future meetings. Don't be alarmed at the word reminiscence. I am not going to give a string of anecdotes, much less to terrify you with the threat of an autobiography, a species of literature which seems to be somewhat epidemic in these days. I have no desire to add another to the many uninteresting and indifferent publications of that sort, which so frequently emanate from the press.

It is well in most undertakings to commence with the commencement, or as near it as possible; and I think you will acknowledge that I have pretty nearly achieved this, when I say that the reminiscence which I am about to bring before you refers to an occurrence which happened when George III. was king, and when the present speaker was *five* years of age. You will naturally be doubtful if any reminiscence at so far back a period can be accepted as reliable, but you must bear in mind that the memory of a child is like a highly sensitised photograph plate, readily receiving impressions when something new and unusual has occurred, and requiring no outward agency to develop them and retain them through a lifetime.

The occurrence to which I then allude was a thunderstorm, and it remains as clear to my memory as was the terrible outburst on 12th August 1884, to the full fury of which I happened to be exposed on a Highland moor. It was, I presume, the first time that I had ever been in a thunderstorm when away from the shelter of the parental roof, and hence the depth and durability of the impression on my mind. It happened on this wise. My nurse had been sent to carry to Boog, the cutler in the High Street, a case of my father's lancets, and Boog was the most reliable man at that time in Edinburgh for making and "setting" such instruments.

Now the lancet case of a busy practitioner of those days almost invariably contained at least four lancets. There is in the possession of a member of my family a solid silver case, presented to my father in 1831, which I believe he never used, and capable of containing six lancets. How many of the busy practitioners of the present day carry about with them such an *armamentarium* as that? I have asked a number of my professional brethren, whom I know to be busy men, if they carried a lancet case about with them, and their replies were invariably that they did not, except in one instance, where my friend to whom I put the question replied, that he carried it about as a matter of form, but did not use it. I do possess one lancet myself, but I would hesitate in these antiseptic days to use it for the purpose of phlebotomy, for I have only applied it for years past to such purposes as puncturing the pustules of an acne.

How comes it that there is this remarkable difference between the custom of the modern practitioner and that of the practitioner of fifty years ago? Of course you all know that this is due to the almost universal abandonment of the use of the lancet for its original special use in venesection. Now you will see how I have brought you from the contemplation of a child in a thunderstorm to the consideration of one of the most interesting questions, theoretical or practical, belonging to the modern practice of medicine. I do not know what were the leading subjects of the papers and discussions of your later sessions, and it may have occupied your attention at a comparatively recent period. Of course in its strictly practical aspect, the question of the abandonment of venesection must essentially be one for the men of age and experience, but there are connected with it theoretical questions, both physiological and pathological, which may fairly form topics of discussion in this hall in the future, as they may have done in the past.

I need not spend a moment in proving that phlebotomy, that constant resource of our professional fathers and grandfathers, has gone out of use in these days. It is admitted on all hands, and must have presented itself, conspicuous by its absence, to the notice of every clinical student. How many of you, who can tell of the numerous amputations and other surgical achievements which you have witnessed, have ever seen this simple operation performed? Have any of you seen a good sample of the buffy coat, or heard in a clinical report that the blood was "buffed and cupped?"

The fact, therefore, of blood-letting having fallen into desuetude being admitted, we may naturally ask, what are the causes that have led to this remarkable change?

The fact is well known beyond the limits of the profession. The public know that blood-letting is now rarely practised, and thoughtless and sneering persons may be heard to say that it is just an instance of fashion on the part of doctors. Such an aspersion on our profession may be dismissed with contempt. Medical men do not shape their practice according to fashion, as ladies do their dresses at the capricious dictation of a fashionable Parisian *modiste* of the Rue de la Paix.

To admit this in any degree would be to take from medicine its title to assert that it founds its practice on scientific grounds (be they right or wrong) conscientiously entertained. And if we come to examine the question in a true spirit, we shall, I think, arrive at the conclusion, that the reasons for refraining from the use of the lancet are drawn from the true advance which has been made in medical science during recent years. My contention is that it is not to the discredit of the profession, as it would be if the change of practice were founded on fashion or caprice, but that it is founded on real advances in medical science.

I had once a discussion with a very prominent deceased member of the legal profession,—a name, if I were to mention it, as the phrase goes, to conjure with,—as to the progress of medical science. I had to hold my own as best I could with one of the acutest minds and most logical reasoners whom I knew, and it was no easy task, even under the stimulus of his admirable *cuisine* and excellent claret, for it was at his own table that I had this to do. He admitted that surgery had made wonderful progress, and let me add that this was before the Listerian epoch of antiseptic surgery. I ventured to stand up for medicine, and to maintain that though we had no distinct epochs to present, yet that the progress of scientific and clinical medicine was such as to entitle us to say that its advance had been as great as that of the sister department. This very subject of blood-letting came up, and whilst my courteous adversary cited it as an example of the empirical work of medicine, I maintained that, on the contrary, the disuse of the lancet was one of the chief proofs of its advancement. I do so now.

But any arguments which we may maintain on this subject do not solve the question which obtrudes itself on the notice of every thoughtful mind, be it medical or laic, were our professional forefathers wrong in their constant employment of venesection, and are we right in our modern rejection of this therapeutic agency, which must be admitted to be powerful for good or evil? Let us therefore, for a few minutes, consider what are the causes which have been assigned for the modern disuse of blood-letting.

The chief of these is the theory of the change of type in disease, and the doctrine that whilst people formerly bore and profited by blood-letting, this is no longer the case, and that therefore there must have been in disease a change of type from the sthenic to the asthenic. The great battlefield of this discussion has been pneumonia, although there have been some considerable skirmishes in the territories of continued fevers and acute rheumatism. I do not think that this is exactly the place, and in not a few respects I am not the man to go into the details of this question, but I am speaking of reminiscences, and it sends me back to the earlier period of my professional life. For brevity's sake I limit myself to pneumonia.

I think that the *status questionis* may be formulated very briefly, somewhat in this fashion. The experience of centuries of physicians, who regarded blood-letting as the one great remedy in inflammations, and the recorded results of their practice by such masters of our profession as Cullen and Gregory, show that it was the remedy which they placed in the foreground of their therapeutic agencies. They were men of too acute powers of observation not to have seen that if it had been an inefficient, or still more a hurtful proceeding, they must have discontinued its use; and, therefore, if bleeding in pneumonia is no longer called for, or is found to be injurious, this must be owing to a change in the type of the disease. I have stated that I would not attempt to enter on a full consideration of this question, but I may refer any of you who take an interest in it to the important—I may call it classical—series of papers and discussions thereanent which took place in the Medico-Chirurgical Society of Edinburgh in 1856 and 1857, which you will find in the first three vols. of the *Edinburgh Medical Journal.*

Now in reference to this it is maintained, on the one hand, that the results given by such men as Cullen and Gregory are not reliable. Of course this means not that they are unworthy of credit as records of fact, but that they are not reliable as foundations for therapeutic doctrine—that they were misled by what they conscientiously observed, but that the results which they obtained were in spite of, not in consequence of, the use of the lancet. It happens that the beginning of my professional life was coincident with the period when blood-letting was beginning to wane, but when it had not been set aside as it is now. The seniors of my profession still continued to bleed, and I had my own small share in the proceeding, and I am not prepared to say that we were always wrong when we did this. I have clearly in my reminiscence cases where a second soup-plateful of blood was drawn within twenty-four hours, and where the results were satisfactory, not merely as regards the patient's escape from death, but in the duration of his convalescence. Now you will at once ask me, Do you now see any cases calling for the use of the lancet? Do you bleed now? And I answer unequivocally, No. Surely then, you reply, this is positive proof, so far as your experience goes, of the change of type in disease. Again I answer, No. I am not prepared, from what I have seen, to accept the practice of the present day as the proof of a pathological theory, which, if pushed to its legitimate conclusions, goes far outside of the limited field of pneumonia.

There is a host of circumstances to be taken into account in judging, by the practice of the present day, of the truth of the theory of the change in type of disease. I do not think that there is anything fundamentally irrational in accepting such a theory. We know that many diseases, especially the eruptive fevers, take on from time to time wonderful differences in their type,—sometimes an epidemic, say of scarlatina or measles, having a general malignancy of type, whilst at other times the same disease will be extensively epidemic, but so trifling as hardly to need the attendance of a medical man. And as there may be acute changes in type of disease, so may there not be chronic changes, which become recognisable only after long periods? This may hypothetically be true, but it is not yet proved. I repeat, as I have just said, that there are many things to be considered before we can accept from the results of our modern practice the theory of the change of type in such a disease as pneumonia. Let us look at some of these.

First, may we not, whilst discarding the theory of change of type in the disease, acknowledge some change of type on the part of the patient? I know that this is sneered at by many whose opinion I am bound to respect, but I cannot put it out of my own view as a matter of theory; and I think that perhaps we ought not to discard this opinion so readily, as we may be disposed to do with the change of type of disease. I think that most people who have given attention to social questions will admit that we lead what may be called *faster* lives than the people of the end of last or beginning of this century did. I do not use the term "faster" in its slangy sense. I hope that we of the present day, at least in all but the lowest classes, are somewhat improved in our habits, for instance as regards intemperance. But our lives are more actively spent, competition in all spheres of life is greater, and the machine is worked with more pressure on the boiler. May not this have to do with the manifestations of disease when it attacks us, and thus the change of type be not so much in the disease as in the patient? We are too apt, I think, to look upon a disease, which we designate by a special technical term as a pathological entity (let us call it pneumonia), and to forget that it is a series of morbid phenomena having a common origin. . Hence our tendency to look for change of type rather in the disease than in the patient, and to him I am more disposed to look. As regards hospital practice, from which most of our experience must be derived, there can be little hesitation that the type of patient has deteriorated. There is no more marked social phenomenon of the present day than the tendency of the population to crowd into our larger towns, and I need not waste words in commenting upon the difference in physical, ay, and unhappily in moral tone and powers of endurance, between the denizen of the town and of the country. Hence we see fewer of those sthenic disorders in which our forefathers had no hesitation in using the lancet. It is true, rustics are liable to acute disease as well as townsmen, but they do not come to our city hospitals as they used to do; and it is to the credit of our profession that this is the case, because with improved professional education there is hardly a part of the country where the services of a skilled medical man cannot be found competent to deal with such cases on the spot.

And this, therefore, leads us to consider what truth there may be in the question (still with pneumonia in view), whether the disease has altered in type or not, why patients do not now bear blood-letting so well as in former days?

Can we get any enlightenment on this subject from the modern experience of surgeons in civil and military practice? I know of nothing to show that the recuperative powers of human beings now, after hæmorrhages of any kind, has deteriorated, and if they do not bear blood-letting now as they used to do, this would seem to be something of an argument in favour of the theory of change of type; but do we really know that pneumonic subjects do not bear it well now, and do not profit by it? If people are never bled now in pneumonia, what facts have we to show that it would harm instead of benefiting them? Are we justified in withholding this remedy which was the sheet-anchor of our professional forefathers? I think we are, and for sufficient reasons without involving change of type. We do so, not because we think they would not bear it, but because we think they do not require it; and to this conclusion we are led, I think, by the more accurate diagnosis and better knowledge of its nature and course than in these former times.

These older physicians employed it early, and used it freely, with a view to cutting short the disease, just as a powerful emetic was sometimes employed to try to jugulate the malady by a sort of revulsive action. No man thinks nowadays of cutting short a pneumonia, any more than a continued fever, by any heroic remedy. We regard it now, not as a mere local disease with concomitant pyrexia, but rather as a general disease, of which the state of the lung is but one of the manifestations, albeit we do not know much as to what this general disease is; and we know that the disorder will follow a certain course, and will not be cut short by us in the sense in which this expression used to be employed. Such considerations as these justify us in abstaining from blood-letting, on the ground not that the patient cannot bear it, but that he does not need it.

Are we blandly to say that our forefathers were utterly wrong in their addiction to blood-letting, and that they acted under the influence of blind authority, following their forefathers like a set of sheep? That they did yield too much to authority I think we can hardly deny; but let us, as one extenuating circumstance, bear in mind that they had the authority of centuries to shake off. Let us frankly admit this, and also that they used the lancet needlessly, but let us not blame them indiscriminately. They had more than the mere authority of the Fathers of Medicine to form a justification of their practice. They saw things which misled them into the belief that benefits were due to the lancet which would have accrued without its use. They did not then know, so clearly as we do now, that pneumonia runs a definite course. They saw, no doubt, as we do every day, the marked improvement which occurs in this disease in the way of crisis about the sixth or seventh day; but as most, if not all, of their patients had been bled, they naturally ascribed this cutting short of the disease to the phlebotomy. But something more may be said for the men of the old school, and I can say this from personal observation, because my entrance into the profession, as I have already said, was coincident with the time when the lancet was not an instrument so nearly practically extinct as the flint-headed arrow of prehistoric times. If any of you had seen, which is almost certainly not the case, the immediate effects of venesection in pneumonia, you would not wonder that they persevered in its use. I have seen it, and I have done it. We had the patient before us after his rigor, with his hot skin (Wunderlich had not then taught us to estimate his temperature by the thermometer), but he had his hot skin, his flushed and dusky face, his rapid short breathing, his quick pulse, his oppression of the chest, and probably the stitch in his side, whether muscular or pleuritic, and we bled him. We marked how his face became less bloated, even though we did not bleed him *ad deliquium*. We saw his breathing relieved, his side stitch abated, and that he could then take a breath with that avidity with which a parched pedestrian takes a gulp of the first stream of water he comes to, and we saw in this the potency of the lancet. He perhaps was relieved only for a time, and next day we bled him again, and then on the sixth or seventh day he abruptly lost his fever—still, it is true, coughed, but expectorated more freely, and he now entered on the stage of convalescence, and speedily recovering appetite, came round to health like a man. Can you wonder if in such circumstances we traced all this to the blood-letting, which we now know might or would have occurred whether we had bled him or not? We had no accurate statistics to inform us of the fact—for I hold it to be a fact—that sthenic uncomplicated croupous pneumonia is, taking it over all, not a fatal disease, and we were pleased.

I make these remarks not so much in the way of discussing a line of practice which would be unsuitable for this time and place, as for enforcing a warning to all of us, especially to you, the rising members of the profession, not to be conceited as to the superiority of our present ways of working over those of our predecessors, but to exercise due humility in the recognition of the advantages which we now possess from the progress of medical science.

I have done no more than show you what were the circumstances which led our forefathers to follow a line of practice which we now regard, perhaps not absolutely as dangerous, but as unnecessary. I vindicate them from blindly following a method in obedience to authority, but I am bound in all honesty to say, that in the light of our modern science we must regard them as having used the lancet needlessly.

It is certainly not for me personally to say anything against the use of the lancet in chest inflammation. I bear over the median cephalic vein of my right arm the mark of a venesection which was done before that thunderstorm of which we spoke. It is a small but distinct cicatrix befitting a small vein, and it was made when I was eighteen months old, as I learned in after years from him who was the operator, my own Grandfather, who was, as my friend Dr John Smith says or sings—

> "The fine old school practitioner,
> All of the olden time."

I am not going to discuss, after the lapse of three-quarters of a century, whether the practice was good or bad.

You will at least admit its boldness; but all that I have to say is, that whether good or bad the result is before you in the flesh.

But we have come by a rather circuitous and retrograde route from the boy of five in the thunderstorm to the child of eighteen months old in the nursery, and this makes me feel that I am becoming egotistical. I must therefore leave our present subject, and in a few words say what is the sum of what I have spoken.

1*st*, That taking blood-letting as our example, we have to mark the wonderful change which has taken place in therapeutics, as illustrated by its use, chiefly within the professional lifetime of him who now addresses you.

2nd, That we believe that as regards this therapeutic method we have wiser and better views than the best practitioners of a former age had.

3rd, That this improvement in practice is not due to the superior wisdom or skill of this age (*vixere ante Agamemnona fortes*), but to the advantages which we enjoy in the progress of scientific medicine, not a little of which is an inheritance which has come to us from our scientific ancestors ; and, lastly, whilst we criticise, or, if you will, stigmatise the phlebotomising practices of these older men, we find, if not a justification, at least an explanation, in their non-possession of the advantages which we enjoy. I have for brevity's sake restricted myself to pneumonia. Are we sure that in some other forms of disease we do not unduly neglect it ? What of apoplexy ? No doubt our fathers bled every one who showed any appearances of incipient paralysis, and in many cases they were undoubtedly wrong, as viewed in the light of our present knowledge. They did not know that a man might become hemiplegic because his middle cerebral artery was plugged by an embolus, and bleeding in such a case was of course more than useless. But I hesitate not to say that there are cases with marked apoplectic phenomena where we would certainly not be wrong in having recourse to venesection.

There are many other reminiscences which I could bring before you, without so long a commentary upon them as that in which I have indulged, but I fear to weary you. It is impossible that the dullest medical life which has extended for something more than half a century should not afford reminiscences which might form the material for comment. I should like to go into some detail as to my reminiscences of the important Medical School of Berlin as I saw it in 1831. I should like to speak of the excellent prelections on Comparative Osteology given by Rudolphi, the best and clearest lecturer whom I ever heard, unless I except that remarkable but self-sufficient man, Dr Robert Knox of this city. I should like to narrate the operative dexterity of Dieffenbach, renowned at that time for his taliacotian skill in rhinoplastic surgery, which led a Wurtemberger fellow-student of mine to make a remark on the small but fine collection of antique busts in the Berlin collection. They had all been carried off by Napoleon to France, but came back with broken noses and ears restored in fresh marble, which stood contrasted in a sort of leucodermic fashion with the warm toned old marble. "Potztausend !" said my genial friend, "Der Dieffenbach muss in Paris gewesen sein."

I should like to cull some bits of reminiscence from the records which I have of the journey which I made in company with my late honoured and lamented friend Professor Allen Thomson, during which we visited, in a cursory style, almost all the universities of Germany, and to say a word as to the many illustrious men whom it was our good fortune to see. I would like to speak of the Dresden Gallery, the first great collection of ancient art which I had the privilege of seeing, and which thus carried home to me, in a practical form, those ideas of painting which have more or less been a delight of my life since boyhood. I should like to recall the clinical lecture at Leipsic of Professor Clarus, which was given entirely in Latin. I should like to acknowledge the courtesy at Halle of Meckel the anatomist, and Sprengel the botanist, with whom I smoked a pipe, and to whom I had the pleasure of giving a specimen of our beautiful floral rarity, the Primula scotica. I should like to speak of the surgical prowess of Langenbeck at Göttingen, and above all of the genial chat of old Blumenbach seated amongst his then unrivalled collection of crania, but which I think my colleage Sir William Turner could now put out of court. I should like to say a word as to Tiedeman, the fellow-worker with Gmelin in the domain of physiology, and in whose lecture-room I sat alongside of five students, each of whom had had his nose slit in duels, but I must pass over all these things and ask you to permit me to mention one reminiscence which is purely egotistical, but which I hope you will pardon because it is unique. I allude to the opportunity which, on 8th August 1831, during our brief visit to Weimar, then the acknowledged capital of German literature, Allen Thomson and I had, through the kindness of Dr Froriep of Jena, of seeing and conversing with Goethe. Naturally when that great name is mentioned the thoughts run at once to the Sorrows of Werther, Götz von Berlichingen and Faust. But let it not be forgot that Goethe, though *par excellence* the poet, was also the man of science. He was a man of science from his youth. I do not say before he was a poet, because *poeta nascitur non fit,* but he continued to love science to the end. He was 82 when we saw him, and retained all the clearness of intellect, and geniality of intercourse, of a much younger man. He was full of interest in all that concerned science, talked with us young men as to the scientific subjects which we had been studying, as well as of what was new in English literature, and thus left upon the mind of my travelling companion and myself an impression which never can be obliterated. He died in March 1832. Pardon me for so enlarging upon this, because I am sure that not in Edinburgh or in all broad Scotland, but probably not in Britain, are there ten men who have seen and conversed with Goethe.

These records, or rather let me say this imperfect catalogue of illustrious men whom it is my good fortune to have seen, is not one extracted from a memory which is probably not so trustworthy as it has been. I have them all in black and white, and in regard to this I beg you to allow me to offer a word of advice founded on my own experience. I hope that some of you, I trust that most of you, and I would desire that all of you, when you have taken your degrees here, should go to visit some of the Continental medical schools. I do not say a word as to the advantage of this. What I wish to suggest to you is, whenever you are away from home, the habit of keeping a regular diary. You will find it a great means of methodizing your search for instruction and mental enlargement, and you will find no small pleasure, in after years, to result from a reperusal of your early journals. Keep the record of your daily doings apart from your strictly professional notes. Your diary may be of much interest to affectionate parents, kind friends, and it may be fair eyes, to whom the professional matters would be not merely uninteresting but repulsive. Don't be afraid that in future years you may be led to despise your youthful lucubrations. It is profitable, if not pleasant (why should it not be ?) to compare what your thoughts and feelings were in those days with what they are now, and there is a sort of cynical satisfaction in thinking how mercilessly you would now deal with the man who would venture to place before the world these, it may be, crude ideas of his youth. This bright season does not return. As the Studenten Lied says:—

Denkt oft ihr Brüder an uns're jugendfrölichkeit
Sie khert nicht wieder die goldne zeit.

There is one reminiscence, however, which, speaking as I do in this place, it would be unpardonable for me to omit, I mean the remembrances of the Royal Medical Society. I cannot allude to this without a feeling of melancholy when I call to mind the number of my fellow-members of that day who have passed away. My reminiscences of the Royal Medical do not refer to the spacious hall in which we are now assembled. They go back to a small, comparatively dingy, building in Surgeons' Square, now improved off the face of the earth, but they carry me back to many a full meeting, and to many an interesting debate which we had in those days. I am reminded by the mural tablet which stands before me that one of those who was my fellow President died during his incumbency, and that few indeed of those who were the active members of that day remain to speak for themselves now, as they so well could do in those past times. I know not how many remain, but I know of two, at least, who yet survive to keep me in remembrance of those dear old days. One is Sir James Risdon Bennett of London, the first, I rather think the only, medical graduate of Edinburgh who attained to the dignity of President of the Royal College of Physicians of London. The other is my much beloved personal friend, Dr George Paterson, F.R.C.P.E., now retired from the work of his profession, but who, when he was an active man here, was the valuable and valued Chairman of the Finance Committee. It was always a great night when George Paterson brought up the Finance Report, and he had that special characteristic of a good Chancellor of the

Exchequer, that, however matters stood, he had always the knack of diffusing over them a warm colour, which made our financial evenings pleasant and genial as a landscape by Claude Lorraine.

I recall these remembrances that I may show you how pleasant they may be to you, should you be spared by that good Providence which I humbly and gratefully acknowledge, to be found among the seniors of your profession. You hardly need any words of mine to encourage you, by earnest attendance and studious participation in the debates, to maintain the character of the Royal Medical; and I trust that this Session, which it has been my privilege to inaugurate, may be even more prosperous than those which have preceded it. *Quod felix faustum sit.*

II.—DISEASED CRAVINGS AND PARALYSED CONTROL: DIPSOMANIA; MORPHINOMANIA; CHLORALISM; COCAINISM.

By T. S. Clouston, M.D., F.R.C.P.E., Physician-Superintendent, Royal Edinburgh Asylum for the Insane; Lecturer on Mental Diseases, Edinburgh University.

Before we can in any true way appreciate the significance of a diseased craving or desire we must endeavour to realize the meaning of a normal or physiological craving. What is it? How does it arise? How is it stopped? What is its physiological use? Its normal place in the bodily and mental life must be looked at before its existence as an abnormality can be understood. In the same way, before we can estimate in any way the meaning of paralysed control, we need to study the controlling powers and agencies in the normal physiological life of the higher organisms, more especially in man. What are they? How are they manifested? What are their normal limits? What are their uses? Any attempt to study, to understand, or to treat effectually the "diseases" called Dipsomania, Morphinomania, Chloralism, or Cocainism without direct and constant reference to the physiological and psychological aspects of desire and control as brain functions and as essential attributes of the higher organisms, would be equally unphilosophical and unpractical.

Diseased Cravings.—A desire, or in its stronger degree a craving, in a healthy organism represents a necessity or an advantage for the individual or the race. It is essentially independent of reasoning. It stands related to control, inhibition, or will as the energies of heat and motion stand to the engineer—things necessary, but needing regulation. Desire and its opposite, repulsion, which represents injury to the organism, are the foundation of all the higher feelings and emotions. There is no definable line of demarcation between chemical or magnetic attraction and repulsion and the vital selections and repulsions of the living and active cell. The choice of digestible particles and the non-selection of indigestible molecules by the amœba are examples of a process which is evolved in the higher and more integrated organisms into desire, craving, and conscious choice or repulsion. The term "desire," like "memory," may therefore be applied to an organic and unconscious process.

Why does the earthworm actively strive to get into darkness? Its organic necessities, and thence its desires, are thus exhibited. As mind and consciousness develop in the higher animals, desire and craving for what is necessary to them are evidently *felt* more and more keenly the higher in the scale we rise; but efforts to gratify their needs are just as strongly put forth in the lower as in the highest animals. The regulation of desire, stopping the efforts for its gratification altogether, allowing only temporary or partial gratification, postponing gratification to regulated times—those are only seen in any marked degree in man, or in animals that have been taught by man. But in a perfectly healthy man all his desires and cravings can be safely gratified in some way or at some time; none of them need absolute inhibition; all are good, and do good to the body and the mind through their gratification. Reason and experience, ancestral and personal, come in at every point to *regulate* the gratification of desire, and will carries out the conclusions of reason and the lessons of experience at every turn. Good and happiness always result from such regulated and reasonable gratification. The exigencies of social conditions at all points and in innumerable ways compel restraint and regulation of craving, and constitute them into a moral code. But the great fact about desire remains. It is the revelation to consciousness of the needs of the organism, individual and social, and its gratification is a direct fufilment of organic law. If we act according to our desires, under the limitations of knowledge, experience, and law, it should give happiness and do good; otherwise there is something wrong with us or our desires. Desires and repulsions should be Nature's infallible safety and danger signals. If they lead to danger and death, there is necessarily disease and abnormality in the organism. A craving that leads to ill is a diseased craving. In fact, no better definition of a diseased craving could be constructed than that it is one which leads to harm. One of the essential laws of Nature has, through disease, become changed when the two things that Nature has conjoined, desire and necessity, are found in antagonism. Desire and hurt is a pathological combination. The conscious pleasure that in all the higher animals results from gratified desire represents the good done to the organism. Where we find desire, conscious pleasure in its gratification, and harm following in sequence, we, as physicians, should at once seek an explanation in disease, hereditary or personal. Such a result should be the same to us as when the politician finds that his laws are leading to theft and violence. He then knows there is something wrong in the body politic or social, and at once seeks out an explanation and a remedy.

By far the strongest and the most subtle cravings of a morbid sort necessarily originate in the brain, without any necessary disturbance of function in the lower centres, such as the sympathetic or visceral ganglia, or in the organs of the body or their functions. We may go further, and say that such intense and subtle cravings necessarily arise, not in the brain as a whole, but in its very highest centres. They originate in the "emotional centres," being in their essence emotional in character. Take the craving of the mother for her sucking child. It is a part of the love she bears it—the strongest emotion being the maternal. Or the morbid craving for death, which I have seen so strong as to overmaster all motives, all natural repugnances, and all moral and religious feelings, the reasoning powers being almost unimpaired. The seat of such a craving must lie in disturbed dynamical action of the highest centres of the cortex, where, no doubt, the normal love of life, the strongest instinct of man and all living creatures, resides.

It is a fair and a scientific inference that complexity and delicacy of structure must bear a direct relationship to importance and delicacy of function. Structure cannot become delicate without its nerve supply also becoming sensitive. The most essential quality of the nerve cell is its instability. A stable nerve cell would neither be receptive of peripheral impressions nor able to liberate its energy rightly in mental or motor impulses. This kind of instability of a nerve cell is a source of strength, for it means that nervous and mental power is great, and readily called into intense action. But there is another kind that is a source of weakness and of death, for it is explosive and destructive. If this bad quality resides in a motor centre, we are apt to have convulsions, hysterical motor acts, chorea, and exaggerated reflexes. If it exists in an emotional centre in the brain, we are apt to have explosions of passion and temper and morbid cravings set up. This bad quality is commonly a hereditary transmission from an ancestry whose brains had been subjected to undue excitement and strain, to poisoning by alcohol, or opium, or syphilis, or, to unphysiological conditions generally. Given a certain tendency to this bad quality of explosiveness due to heredity, we find it is easily converted into an actual disease by many cerebral causes; chief of these are the stimulation of the higher brain centres by those substances that have a special affinity for the highest organized substance in Nature—the brain cortex of man. By virtue of this affinity they stimulate thought, and especially emotion, causing brilliancy and fanciful views of things, with a conscious feeling of intense pleasure. This stimulation being often repeated, leaves a strong organic memory in the tissue and a conscious memory in the mind. Both unite with the unstable dynamical state of the organ in causing a craving that tends to grow stronger and stronger. The pleasurable effects simulate those resulting from the gratification of cravings for food when hungry. The repeated stimulation by alcohol of such brains seems to have the sort of effect that repeated explosions have in the motor areas. Ferrier says that a monkey's brain subjected to a very strong electrical stimulation, so that general convulsions result, is ever after more liable to convulsions, and that localized stimulation is in such a brain very apt to spread and to cause general convulsions instead of local muscular spasms.

Let the fluids of the body evaporate, and the result is a craving for water; let the *vis nervosa* become exhausted, and we have a craving for a stimulant or a restorative of some sort to the nerve energy. It is a fact that some foods are more stimulating to the brain cortex than others, *e.g.*, strong beef-tea than milk, flesh than bread. Let us first take a keenly-working sensitive brain, then let us imagine that brain stimulated by life in a large city, by hourly converse with men, by anxious employments, or by intense study. If from childhood upwards the possessor of such a brain has depended on stimulating diet and drink for its restoration when exhausted, there is an intense and irresistible craving set up for such food and drink stimulants whenever there is fatigue. Such a brain has developed an affinity for them, and for such alone. Milk and farinaceous diet often become repugnant, and when taken do not satisfy the brain craving. Its owner becomes physiologically a flesh eater and an alcohol drinker. I am in the habit of preaching an anti-flesh crusade for the children that by heredity have such brains; not that flesh won't give more work, but that in the long run the happiness, the health, and the longevity of such persons are increased by the non-stimulating foods, and that the risk of many nervous diseases is avoided. To stimulate strongly and constantly the receptivity and intensity of a hereditarily receptive and intense brain in most cases has the effect of making the organ too keen in working for the time at the expense of the general bodily and trophic vigour. And it is not always remembered that for every increase in the power to energize and to enjoy, the penalty is exacted by Nature of a subsequent and consequent increase in the feeling of exhaustion and of power to feel pain. This effect is still further increased, and the penalty paid is still greater, if in such a brain as I have been supposing there are hereditary potentialities towards the neuroses.

If we now look at the effects of the nerve stimulants nearest food, viz., tea and coffee and cocoa: the custom of taking these for the dissipation of the feeling of exhaustion sets up in brains of the keenly-working but unstable type a commanding craving and a dominant impulse in many cases. The chemical constitution of all such substances have an affinity for the highest centres in the brain, stimulating and soothing some of them in a very subtle way, most delightful to the consciousness of their owners. Alcohol, opium, and cocaine are still more immediate and more intense in their effects on such brains, they seem more consciously restorative; the present joy is greater, and the craving begotten of habit far more masterful. After a time every indication points to a change in the innate mode of working of the higher brain centres as the result of the repeated use of such cortical stimulants: the evolution of energy is less regular and more spasmodic, the trophic nerve energy is less, the reserve stock of power less. The brain stimulant and the resulting spasm of brain energizing become so correlated that the steady development of brain energy from food is interfered with. The want of the stimulant for any time is intensely felt as a constant and ardent craving, which can only be resisted by the exercise of a strongly put forth inhibition. The cortex is, in fact, starved of what has become to it a necessity of working: the conscious feeling is that of a thirsty man for water. A substitute for necessary food has been used with more intense and more immediate effects. The natural food craving has become transformed into a much more intense stimulant craving.

It is certain that the existence of a craving indicates a *need of some sort,—but it may not be the thing craved for that is needed.* It is the very essential principle of the treatment and cure of cases of dipsomania, morphomania, and cocainism that the cravings are sure signals of distress, that must be attended to.

To gratify unregulated desire may be a sin, but to inhibit it altogether may be still more sinful, because more dangerous,—for is not all sin disobedience of law? And if the physiological law in regard to the matter be as I have stated, it is to thwart a law of Nature not to gratify a craving in some way, and put a stop to it thereby. It will be the highest duty of the physician of the future to interpret cravings and repulsions, and to satisfy them safely by his treatment. Perhaps the same rôle will apply to the politician and priest of the future. I by no means homologate the witty reply of the Epicurean to the question how he "got rid of temptation?"—"By yielding to it." If a consumptive craves milk and fresh air, if a gouty man loathes tobacco and flesh, if a pregnant woman craves pickles and fruit, we refuse to gratify and quench these cravings and repugnances in some way at peril of sinning physiologically, just as we should sin by refusing a thirsty man water or a starving man food.

A diseased or pathological craving is one whose *direct* gratification would injure the organism in its own life or that of its descendants. It is a separation of two things that ought to be indissoluble, viz., desire and benefit, the performance of a function and the pleasure or organic satisfaction which that performance should give. When eating causes pain, when energizing is always distasteful, when breathing cool air is repugnant, we have no hesitation in concluding that a condition of disease exists. The organism is, peripherally in some of its organs, or centrally in its co-ordinating, and regulating, and feeling brain, in a pathological state. If a man desires alcohol or morphine when the system needs food, there is a condition analogous to a paræsthesia.

In the higher animals every function has become exceedingly complex, and for its proper performance a concurrence of organs is necessary, all working normally. To take normal digestion, for instance, we must have amongst other conditions—(1), the gastric juice glands healthy; (2), the immediate regulating apparatus of these glands, the nervous ganglia and plexus in the coats of the stomach, and the abdominal sympathetic also sound; (3), the regulating motor centres for the gastric muscles normally excitable; (4), soundness of structure and function of the pneumogastric centres; and (5), healthiness of the highest centres in the cortex, where the stomach and the digestive function are represented, where they are connected with feelings and ideas that depend on and relate to alimentation, and through which conscious craving is established and conscious satisfaction is felt when the craving is satisfied with suitable food. Theoretically we may assume that if the gastric glands (1) are secreting the gastric ferments in abnormal proportion of ingredients,—*e.g.*, if the hydrochloric acid is deficient, there will be a feeling of discomfort after food, and a craving for something will be established in the highest regions (5), which will be stopped when acid is taken, the normal sense of organic satisfaction being then experienced. When again the digestive ferments are so changed that some of them are irritating the sensory nerves (4) of the stomach, then also a gastric pain or dissatisfaction is felt by 5 that is only converted into the normal satisfaction by the administration of an alkali or bismuth. In either of these cases the administration of whisky or morphine might stop the feeling of discomfort, and establish a false and pathological feeling of alimentary satisfaction, which we know as physicians would not represent a cure of the disorder. The conscious memory of this transient relief, and the organic memory of it by the stomach and the brain would certainly, after many repetitions, set up a craving for whisky or morphia every time the same gastric discomfort was felt. A morbid craving would thus result from a false medical treatment.

Let us now imagine the original disorder to be in the higher brain centres. If through a pathological condition of these centres there arose to consciousness a feeling of alimentary dissatisfaction or gastric pain analogous to a neuralgia or a paræsthesia of cerebral origin. If through such a purely cerebral cause normal appetite was never felt, but instead thereof a sinking, low feeling of emptiness, exhaustion, and depression. Alcohol or morphia is given to remedy this. Blood is determined to the alimentary centre in the brain, the cells of which are also directly stimulated dynamically by the drugs. To consciousness a feeling of satisfaction and pleasure is substituted for one of discomfort. The normal food appetite returns in some degree. Every time the same feelings are experienced the same delusive remedy is craved. The man's brain was exhausted by mental exertion or emotional distress, or was unstimulated by sufficient oxygen in the blood freely circulated by muscular exercise. He craved for some remedy for his discomfort;

he learnt to have his craving satisfied by his physician, who put a false interpretation on his desire, and adopted a delusive remedy. The true restoratives of rest, fresh air and exercise, and of tempting, easily digested food, were not given, but instead he got a brain stimulant or sedative that was more suddenly productive of relief than even Nature's true remedies. This sudden relief made a more vivid impression on the memory, conscious and organic, than the slow and gradual but physiological cure of the disordered condition. It is therefore more keenly and directly craved for when the sinking feeling and discomfort arise, just because the memory of the sudden pleasure it gave is more vivid.

Assume now that this brain was by heredity full of the potentialities of diseased working, that it was "unstable" and "non-resistive" to internal or external causes of disease. Assume that it was easily tired and unduly sensitive to its own exhaustion. Stimulants or sedatives are taken, the present comfort and relief are even greater to such a brain than to one with no such heredity, because it is more sensitive and less resistive. Its essential constitution, too, has more affinity with brain stimulants and brain poisons than that of an ordinary brain. A craving is very soon indeed set up in such a brain, and is very intense. I knew the case of a boy who got a glass of whisky from a companion when tired, and from that moment had a masterful craving for such a stimulant whenever he was hungry. His mother had been insane, and no doubt he had derived a brain from her that was full of potentialities of such evil. It is in some such cases that there seems to be a hereditary memory of drunkenness shown by a craving for stimulants in the daughters of a drunkard, arising at puberty before alcohol is even tasted, and felt at every menstrual period during the reproductive life. It does not need even the disturbing influences of menstruation on the brain for such a craving to arise. Plenty of boys hereditarily tainted acquire a strong craving for alcohol from the beer they get at school and from the wine they have the chance of getting at dancing parties. I knew a boy of fifteen, the son of a neurotic but perfectly sober mother, but whose maternal uncle was a dipsomaniac, who began to take whisky on the sly, and who at once developed into a drunkard of the secret sort. From that time forth he took whisky at intervals in large quantities, always secretly. In many of these cases I think it a reasonable explanation that there is a hereditary memory of the craving for a brain stimulant. The fathers had eaten sour grapes, and the children's teeth had been set on edge.

Paralysed Control.—Control exists and must necessarily exist wherever there is life. Growth must cease at some point in every organism and in every organ—that is, cell growth must be controlled through some law. The shape, and the size, and the composition of every cell of every organ must be under a controlling power, and muscular motion must ever be under control from the lowest up to the highest animal. The fixity of the hereditary laws that govern all life, "like begetting like," is a form of control; the fixed limits to all vital energies and powers, evolutions and developments, are a form of control. A controlling force is as much an essential quality of all living things as weight is of all inorganic substances. The idea of evolution is founded on control; for "natural selection" and the "survival of the fittest," may be regarded as other modes of expressing it. But it is when we come to special organs of the human body, and special qualities of mind through which control is exercised, that we realize its incalculable importance, physiologically and hygienically. Every system of morals has, as its practical side, not the knowledge of good and evil, not the feeling of love for the one and hatred of the other, not the "sense of right and wrong," but the power of controlling thought, feeling, imagination, and action. The former would constitute but a very incomplete system of morality without the existence of the latter. So, physiologically, all organs would fail in their organic uses to life and to the organism were there no apparatus of control or inhibition. What use would the bladder, as a temporary receptacle of urine, be but for the elaborate controlling apparatus to close and open its outlet, to put its muscles into action, and to stop them at the proper times? How would the heart and lungs provide for the conditions of exertion and repose, heat and cold, rarity and density of atmosphere, without an elaborate apparatus of inhibitory nervous ganglia? Life would be impossible as we understand it without the power of adaptation to varying conditions and environments. This is largely due to the controlling apparatus. Much of this is not "voluntary." It is automatic, like the governor of a steam engine; but it is control notwithstanding.

The highest regions where control is exercised are where it is consciously exercised to regulate or inhibit felt desires or craving in man. This, like all nervous and mental function, must have an encephalic basis. It is not merely a "moral quality," with no substratum of cerebral cells and cell energy. Is not its prolonged exercise always accompanied by a conscious feeling of strain, by a determination of blood to the brain, and of succeeding fatigue just like muscular exertion? Let any man control and fix his attention for some time on a subject that is not interesting, or inhibit a desire for food, or resist a craving for fresh air when in a stuffy room, and the results are physical weariness, drowsiness, or muscular fidgets. It is still doubtful whether there are localized "inhibitory centres" in the brain for all the control that is seen to be exercised, or whether control and limitation of energizing are not essential qualities of every centre—whether, in fact, the power of liberating nerve energy does not imply some power of controlling its liberation. No doubt control is the highest function of nerve substance. The higher the animal the more control in various forms is seen. A high control over the expression of the emotions and over conduct is the last product of evolution. The moral qualities are thus the highest physiologically and evolutionally as well as psychologically. In all systems and classifications of mind, volition is placed at the head of the faculties.

There is a certain power of inhibition over desire and of regulation of craving which all men and women must possess to be able to live in any community, however primitive, however barbarous. In the primitive community that power of control is limited in extent, but is often very intense in certain directions where it has been gradually strengthened by practice in many preceding generations. The Red Indian will inhibit all signs of pain when tortured and all signs of astonishment before strangers, but he has no power whatever to control his craving for firewater after his brain has once felt the effect of the fatal stimulant. I do not agree with Maudsley when he says that the savage craves for alcohol after having once felt its effects because it opens the ideal to him. "This eager use (of alcohol) running headlong into abuse is evidence of the longing that there is in human nature for the ideal: for an elation of feeling, an expansion of sympathy. A freedom of mental power, an exaltation of the whole nature, mental and bodily, are obtained thereby which are denied to it by the real."[1] This, I think, rather describes the overmastering motive to excess in the use of nerve stimulants by the men who by ancestry and cultivation have acquired the faculty of being idealists in any degree. The savage and many of the congenital dipsomaniacs and the habitual drunkards among our labourers and our criminals cannot by any use or abuse of alcohol have any ideal opened out to them. We must seek a lower motive than this for the craving and for the inability to resist it. Maudsley's description exactly applies to De Quincey's motive for giving himself up to opium. It does not apply to the Red Indian or to the Whitechapel victims of "Jack the Ripper." In them the motive is more analogous to that which prompts the stallion to seek sexual congress with the mare—it is a simple organic craving of great intensity.

The study of mental inhibition as it gradually arises in children is one of the most instructive lessons in psychology, from the physiological side, that it is possible for any one to learn. At six months there is no such brain power existent as the higher inhibitions. It shows itself about the end of the first year in an average child. It gradually develops just as muscular co-ordination and equilibration, as the power of attention and reasoning gradually develop. But as between different children the difference is enormous as to when one can stop eating a sweetmeat by a mental effort as compared with another. One child will accomplish this act of mental inhibition at twelve months while another cannot do it at three years. The sense of right and wrong, and the power to follow the one and avoid the other, are earliest seen in a good stock—even precociously so sometimes where there is a nervous strain in the stock. But in many of the children of habitual criminals and of drunkards, and of the insane, it is often long in appearing, and frequently never attains much strength. I have had brought to me on several occasions the daughter of one of my asylum female patients, who in features is precisely like her mother, whose father was a drunken tramp, and who now at 14 seems utterly devoid of any power of higher control whatever. From the time she could walk and speak she has exhibited the impulsiveness of an untrained dog of a bad breed. She has often nearly killed her brothers and sisters. She never showed any desire to share anything with them till she was done with it. She would steal and lie as if these practices came natural to her. She was sent some years ago to a sort of religious reformatory school, and was so dangerous to the other girls that she was the terror of the place—and all this without any sort of adequate cause, for she was kindly treated. It seems to take many generations to redevelop an atrophied conscience when it has by heredity nearly disappeared. There are many cases where control was normally developed in childhood and youth, but where its organic basis seemed to be weak and unresistive to outward causes of disintegration. I knew the case of a youth of normal character at 18, who when subjected to temptation during his college life gradually lost his inhibitory power, so that by 25 he was a simple automaton so far as resistance to drink and to his sensual appetites were concerned. It seemed as if in his case a neurotic heredity produced a brain that needed always to be guarded against any strain on its inhibitory qualities. This brain being poisoned by alcohol in the stage of development during adolescence, it underwent deterioration, and was never again able to exercise normal inhibitory power, just as some stomachs after being subjected to a course of indigestible food never again gain their normal power of digestion. Many of us have seen "stomach" drinkers,—men who have lived on sheep runs, eating for years nothing but damper and mutton—in this condition of chronic dyspepsia, who could not live without the gastric and cerebral stimulus of whisky.

---

[1] *Body and Will*, p. 247.

A long and continued course of poisoning the brain with alcohol will often destroy its power of inhibition in even the strongest and most normal brains and with the best heredity. We have all seen such men of splendid powers of self-control at 30, who by continuous drinking or opium taking have first weakened and then destroyed their controlling powers, so that at 45 or 60 they not only were drunken sots, but disregarded the common decencies of life and society; their word could not be believed, and they became as regards control far below the level of a well-trained dog.

When we come to obvious disease of the brain in function or structure, the symptom of lack of control is one of the most common and marked features. Insanity has been well defined as in essence loss of control. In the explosive motor diseases of the epileptic type, impulsiveness and irritability are the most common mental symptoms. In brain softenings of all sorts, more especially where they are extensive, irritability and loss of control over the expression of the emotions are two of the most marked features. Loss of control in all these diseases is regarded as so perfectly natural a symptom that no one regards it with wonder or doubt. It is only when we have the symptom of loss of control standing alone, or nearly so, that doubts are thrown on it as being a real disease. If a man is melancholic, but yet in outward conduct so natural, and in judgment so sound that he goes into the market-place among his fellow men and transacts complicated business well, and yet he puts an end to his own life that night, we do not now look on that terrible and absolute loss of control as being other than disease, and our feeling is not one of blame; yet if that man's brother, with the same bad neurotic heredity, at the same age and under much the same circumstances, becomes run down in nervous tone, gets somewhat depressed, takes to drink or opium, cannot stop his craving for such a neurine stimulant or sedative, but yields to it and allows it to destroy his health, injure his reputation, and finally put an end to his life,—in such a case the law does not regard it as a disease, society blames instead of pitying him, and even the medical mind has much more difficulty in realising that the uncontrollable craving for drink in him was a disease as much as the uncontrollable act of self-destruction in his brother's case. Control, in fact, is not fully recognised as a brain function as yet; it is instinctively considered dangerous to do so by some, while most think it wrong to do so. It looks to some people like knocking away the chief pillar on which society rests, and on which law majestically reposes, if any question is raised as to a craving being diseased or as to man's control being "paralysed." To admit that a man's conduct depends on his heredity, on the working of his brain, or on the pathological conditions to which that brain is subject, seems like robbing him of his "free will," depriving him of his conscience, and reducing him to the level of the "brutes that perish;" yet the facts of science must sooner or later be recognised, and must be acted on. About one-fourth of my insane male patients in this asylum would soon kill themselves with whisky, or any other strong brain stimulant, if they had full access to it. But no one regards this as strange in any way. When the judging and emotional power and memory are impaired by disease, it is assumed that volition and control will also be disturbed. When a woman's emotional nature is so changed that she takes dislikes to her own children, when a man's judgment is so perverted that he thinks himself the king of England, when a man's memory is so impaired that he cannot tell his own name, these cases being otherwise for the most part sound in mental working, we have no difficulty in recognising that such persons are mentally unsound and that their brain working is pathological. But when a man loses his power of control in regard to drink, or opium, or sexual conduct, he is not necessarily regarded as labouring under a diseased condition of brain at all. I have now a patient who is in judgment acute and in conduct normal in all respects but one. He exposes his person before the other sex whenever he can do so undetected. He schemes for opportunities, yet he deplores the hold this diseased craving has acquired over him. Motives of every sort have been used, and have utterly failed to control this bestial impulse. He has lost his professional position through it; the ties of family, of wife, and of children have failed to check it; he has been punished by the law and put in gaol—yet it is masterful as ever. You may spend days with him, and find him agreeable, intelligent, and without a trace of delusion or other mental perversion. He has voluntarily taken refuge in an asylum for cure if possible; for control if cure cannot be attained. Such conduct, motiveless, yet existing as a hard pathological fact, makes the case very instructive as an analogy to some cases of dipsomania, a stepping-stone of belief and realization that enables us better and more firmly to comprehend that a paralysed control over a craving for drink, or opium, or cocaine may be a disease as much as suicidal melancholia.

Diseased Cravings and Paralysed Control regarded together.—Diseased craving and paralysed control, though differing essentially in physiological and pathological aspects, and capable of being looked at as I have now done, separately, yet must commonly be regarded together when we come to study and treat individual cases of brain disease. No doubt there are innumerable cases where there are diseased cravings, but with normal control through which these cravings are kept in check. Stronger motives and desires than such cravings exist, and act inhibitorily. Society rests on this fact. In many cases of insanity in their early stages this is seen, and in many other cases the disease is recovered from before it goes further than the still controllable craving. I lately saw a young man who had the most acute desire to touch filthy sores and to put his fingers into filth, yet his power of control to resist this in most cases remained, and in a few months under change of scene and circumstances, fresh air, tonics, and cheerful company, he got over it entirely. There are thousands of persons who have suicidal feelings, yet control them from being put into action. How many thousands of men and women there are who have a strong desire for drink, and yet control it! So, on the other hand, we may have paralysed control, but with no great craving to do wrong acts. I saw a woman once whose power of control in every direction was quite gone, who could originate nothing, who was sometimes tempted to act wrongly by suggestion from without, like a person hypnotised, but not by craving from within, who, by the help of a little gentle but constant guidance from without, did nothing very much amiss, and recovered after a year or two's duration of this state of paralysed volition.

Neither of these conditions by themselves are, however, so common as the one where we have the two morbid states combined, the morbid craving and the lessened control. It is not quite clear, on physiological grounds, why they should commonly go together, but it is certainly so in fact. It seems as if the lessened power of nervous energizing, the brain disease, or the degeneration which sets up the one weakens the other. Normal desire representing physical and organic need, normal inhibition representing the co-ordination, the correlation, the limitation, and the selection that are necessary to the existence of all higher animal and social life, it is realizable how, in disturbances of the higher centres in the brain cortex where all organs and functions are represented and are correlated, and where most of the strong hereditary qualities reside, craving and control should stand or fall together. Both relate in the closest way to the continuance of the individual and of the species—the two primary ends and motives of life.

The chief varieties of diseased craving with paralysed control are the following:—

I. Congenital absence of inhibitory power; the craving towards drink, lying, unnatural sexual acts, violence, and lawlessness arising from physiological causes, or by suggestion, or by the direct teaching of criminal associates. Such cases are always of a stock hereditarily tainted with the neuroses or with social lawlessness.

II. Loss of the power of inhibition with exaggerated cravings occurring as one of the many neuroses of adolescence, standing as one of the developmental dangers in hereditarily weak subjects. It then ranks with hysteria, chorea, consumption, rickets, adolescent insanity, developmental epilepsy, Friedrich's disease, etc. It often appears as "incompatible" and "peculiar" temper, or as impulsive wandering from home, or causeless lying, drinking, uncontrolled sexuality, stealing, taking to unnatural foods, to strange ways of living, to eccentric religious impulses, to outbursts of talking when alone, to association with persons of lower social status, and to "impracticablenesses" and unconventionalities of all kinds in theory and conduct.

III. Development of craving, and at the same time loss of power of control, from over-indulgence in sexual vice, in drink or opium, or other drugs having special affinity with the brain cortex. In such cases there is often an innate tendency to inhibitory breakdown from some cause. There is often over-sensitiveness, the keen feeling tending to produce nervous exhaustion, and this suggesting a counteracting stimulant. But, as I have said, continued and excessive indulgence in the use of alcohol, opium, or cocaine will produce a craving and destroy control in any brain, however strong its original constitution may have been.

IV. Over-work, over-anxiety, distress and affliction, life in unhygienic conditions often produce morbid cravings for nerve stimulants or sedatives, and will diminish the inhibitory power to resist these.

V. Injuries to the head, gross brain disease, and sunstroke, frequently diminish the controlling power and produce impulse and craving of a diseased sort.

VI. Actual insanity is accompanied by morbid craving and paralysed inhibition to a marked extent, the "Dipsomania" symptom in many cases of ordinary insanity being very troublesome indeed. Very many cases of simple mania, some cases of simple melancholia, many cases of delusional insanity, many cases of early general paralysis, and many of dementia have this symptom. This has not been drawn attention to sufficiently; the more apparent symptoms of insanity have overshadowed the less obvious failures of inhibition, or, as in some cases where the disease was caused by drink, they are regarded as dipsomaniacs long after they have become ordinary maniacs.

VII. The conditions occur sometimes in relation to recognised insanity between the attacks, then taking the place of an attack of mental disease with delusions, or excitement, or depression. I have seen many cases who were always either "very mad" or "very bad," but not both together.

VIII. Certain of the physiological functions and conditions, such as menstruation, pregnancy, parturition, lactation, and the climacteric, set up morbid cravings of all sorts, and paralyse control in certain women. For a physiological function thus to be accompanied by a morbid volitional state a neurotic heredity is commonly needed.

IX. Absolute lack of education of the controlling faculties, with nothing to observe or imitate but bad example, and escape from the natural consequences of bad acts in youth, may leave the controlling power undeveloped.

X. The absence of normal mental stimuli, social intercourse, amusements, family life, in some cases tend to set up cravings for brain stimulants of the alcoholic sort.

XI. Senile degenerative changes in the brain will sometimes set up morbid cravings never before experienced, or weaken control over ordinary cravings. Senility often shows its commencement by immorality, by over-drinking, by gross sexuality, and by a loss of self-respect.

XII. Diseased craving and lessened control alone or combined in lesser forms and degrees are often associated with ordinary nervous diseases, and even with diseases of the bodily organs other than the brain. They are seen in some cases of epilepsy, diabetes, locomotor ataxia, anæmia, chlorosis, cardiac disease, myxœdema, phthisis pulmonalis, pernicious anæmia, and many other complaints.

(*To be continued.*)

III.—A CASE OF OBTURATOR HERNIA.

By W. Scott Lang, F.R.C.S.E., Lecturer on Surgery, Edinburgh School of Medicine.

Cases of obturator hernia are so rare that I venture, by the kindness of Dr Symington, to place on record an account of the anatomical facts ascertained by dissection of a case accidentally met with in Minto House anatomical rooms.

The body was that of an old woman, much emaciated. On opening the abdominal cavity the mouth of the sac was at once seen on the left side at a point corresponding to the upper border of the obturator foramen. The descent of the sac had taken place on the inner side of the round ligament of the ovary, and internal also to the obliterated hypogastric and deep epigastric arteries. The peritoneal pouch was empty, and its size was such as to admit the little finger to the extent of about one inch. Around the neck of the sac the structures felt very unyielding in every direction. Movements at the hip-joint had no appreciable effect on the neck of the sac, except that forcible rotation of the thigh inwards made the neck rather tighter by acting on the obturator externus muscle and its origin from the obturator membrane, thus rendering the latter tense. The orifice of the sac could be reached without difficulty by the forefinger in the vagina. The superficial division of the obturator nerve lay in close relation to the sac anteriorly; the deep division was on the postero-external aspect. The obturator artery was found descending from the deep epigastric on to the neck of the sac, and winding round its inner side in very close relation with it. The obturator vein also lay on the inner side. The position of the sac was one inch and a quarter internal to the femoral vein—internal, therefore, to the position of an ordinary femoral hernia.

Coverings.—The extra-peritoneal structures were very delicate, but the sac had a covering derived from the fascia of the obturator externus muscle. The sac lay under the pectineus muscle, and seemed to protrude through a gap in the much emaciated obturator externus. A few fibres of the obturator externus arose from the upper margin of the obturator foramen superior to the neck of the sac.

Most of the facts elicited in this case confirm, except in one important point, what is written in the text-books regarding obturator hernia.

Statistics show that the condition is much more common in women than in men, and emaciation is said to be a predisposing cause, owing to weakening of the structures closing the obturator foramen by atrophy of the obturator externus muscle.

During life one of the prominent symptoms of obturator hernia is pain shooting down the inner side of the thigh. This pain is said to be aggravated by rotation of the thigh *outwards* (Pimbet).

Doubtless rotation outwards may aggravate the pain by rendering tense the fascia lata, and so compressing the sac; but it must be borne in mind that this same rotation outwards favours reduction of the hernia by relaxing the obturator externus muscle and the parts around the neck of the sac. This was proved to demonstration in the case here described.

Rotation inwards, although relieving the pain and pressure on the fundus of the sac, must inevitably constrict the *neck* of the sac by rendering tense the obturator externus muscle and obturator membrane. Attention to this point might prove valuable in attempting the reduction of such herniæ by taxis.

As regards division of the strangulating structure. In a case successfully operated upon by Obre it was divided *upwards*, but the obturator vein was cut, and had to be tied. Erichsen states that the structure must be divided directly *downwards*, and from the anatomical relations above described, such would appear to be the safest course.

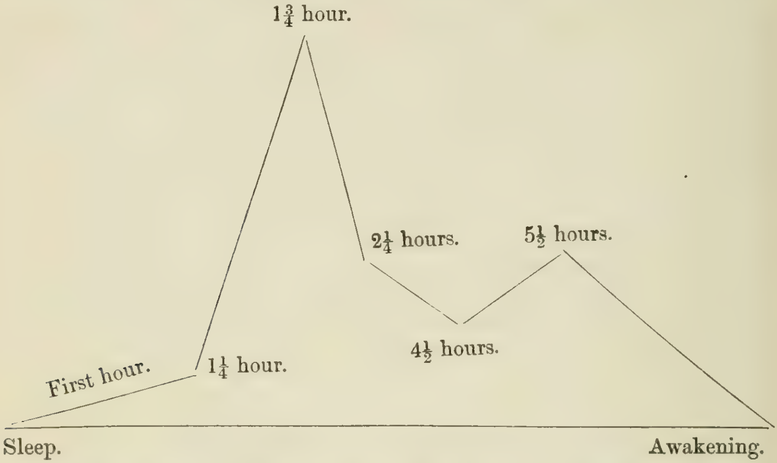
IV.—NOCTURNAL PAROXYSMS IN WHOOPING-COUGH: AN EXPLANATION OF THEIR FREQUENCY.

By C. Clark Burman, L.R.C.P. & S. Ed.

It is a well-established fact that the paroxysms of whooping-cough are more numerous and severe during the night than during the day; while closer observation shows that the nocturnal attacks have a special tendency to develop at particular hours. This tendency is not peculiar to whooping-cough, but is noticed in other affections associated with paroxysmal dyspnœal attacks, *i.e.*, asthma, chronic bronchitis with emphysema, certain cardiac affections, etc. I endeavoured to explain the physiology of the dyspnœa in the latter class of diseases in a communication published in the *Provincial Medical Journal* for December 1888, *et seq.* I propose in this paper to adopt a similar line of argument in explanation of the greater frequency and severity of nocturnal paroxysms of whooping-cough and the specialized hours of their appearance.

Mönninghoff and Piesbergen, two of Vierordt's pupils, ascertained by experiment the depth of sleep, using as a sensory stimulus the auditory sensation produced by dropping a lead ball from a given height. For a perfectly healthy man, during the first hour the slumber is very light; after one hour and a quarter, the depth of sleep increases rapidly, and reaches its maximum point at one hour and forty-five minutes, then falling quickly to about two hours and a quarter; but later on, after four hours and a half, there is a second small rise, which reaches its maximum at five hours and a half, and gradually falls again until the time of awakening.

I have attempted to show this in a diagrammatic manner, and I think most of my readers will agree with me in saying that the evidence of parents, nurses, and others attending upon whooping-cough patients will pretty nearly confirm my statement, that the number of paroxysms are more frequent during the hours between the time at which the children retire and their own bedtime, and again in the early hours of the morning, after the parents have been in bed for some time.



It is not infrequent to hear the friends say, "What a night we have had with the child, not only before *we* went to bed, *but early on in the morning*,—we only got rest *after daylight broke*."

Such statements are unconscious proofs of the connexion of the paroxysms with the depth of sleep. Allowance must be made for the altered conditions due to the disease, since the standard is taken from a perfectly healthy subject.

Another factor to be considered in the causation of the paroxysms is the reduction of respiration both in depth and frequency during sleep, and especially that of the first sleep, which is more profound than that of early morning.

Let us say the child goes to bed about seven o'clock; provided he is not put into a cold bed in a cold room, and goes to sleep at once, we would not expect, and in fact do not meet with many, if any, paroxysms during the first hour; but from 8.30 to 9.30 or 10 o'clock, and especially about this latter time, we meet with several attacks. The parents usually retire about this hour, and for an hour or two the child is pretty quiet; then about one o'clock, or perhaps somewhat later, depending upon the time at which the child fell asleep, the attacks recur; after this we usually find he rests better and sleeps quietly until morning, when the street doors are opened, admitting a great influx of cold air, with the invariable result of such sudden atmospheric changes—a succession of paroxysms.

There is no doubt that the accumulation of bronchial secretion during sleep has an important influence in determining the attacks so frequently noticed when the child first awakens, much the same as we find in chronic bronchitic patients who invariably have their morning cough with copious expectoration, only that the excited condition of the nervous mechanism in a child with whooping-cough produces the characteristic paroxysm.

So far, I trust, I have demonstrated that the frequency of the nocturnal paroxysms bears an intimate relation to those periods when sleep is most profound, as demonstrated by Mönninghoff and Piesbergen, and thus traced an analogy between them and other forms of nocturnal dyspnœa.

Let me quote from one or two authorities regarding the cause.

Trousseau, *Clinical Medicine*, vol. i., page 665, writes—"It is a remarkable circumstance that they (the attacks) are commonly more frequent by night than day, or to speak still more accurately, from six o'clock in the evening to six in the morning, than from six in the morning to six in the evening. *Can one attempt to account for this difference by saying that at night the child is no longer, as during the day, under the influence of excitation which occupied his nervous system in another direction?* Whether this explanation be true or not, the fact is no less true, and deserves to be noticed."

Bristowe, *Theory and Practice of Medicine*, 1876, page 143, writes—"The paroxysms . . . . . are almost always more numerous, as well as more severe, at night-time."

West, *Diseases of Infancy and Childhood*, sixth edition, page 459, *et seq.*, writes—"The nocturnal exacerbation is sometimes so marked, that the fits of coughing are not only severer, but are actually more numerous by night-time than by day. In very mild cases of whooping-cough there is but little difference between the frequency and severity of the paroxysms at night-time and by day; and in other instances, while the child rests quietly through the greater part of the night, there is yet a marked aggravation of the cough on first lying down at night and on first waking in the morning. When the exacerbations occur at those two periods, the evening exacerbation is often induced by the child being removed to a bedroom less warm than the apartment in which it spends the day, while the morning attack results from the accumulation of mucus in the bronchi during the hours of sleep."

"Neither of these causes, however, is the sole occasion of the increased severity of the disease at night, *nor is the occurrence peculiar to whooping-cough, but is observed also in asthma and in many other affections of the respiratory organs in adults.* The severity of the nocturnal paroxysms is often a very good criterion of the general severity of the disease; and any exacerbation of the disease is usually attended with special increase of the nocturnal paroxysms, and not merely by more frequent coughing and whooping, but likewise by a marked *increase of dyspnœa.* On the other hand, a diminution of the nocturnal exacerbation is one of the most frequent indications that the disease has begun to lose something of its previous severity, and the cough often ceases entirely at night for some time before it disappears completely during the day-time."

It is satisfactory to note that West recognises the similarity of the attacks of asthma and whooping-cough in exhibiting a preference for the night; he does not, however, offer any explanation.

Dr W. Squire, in *Quain's Dictionary of Medicine*, page 1772, article "Whooping-Cough," writes, in reference to the pathology of this disease—"But the nerve-centres both of the respiratory and sympathetic system are disturbed in the earlier stages, and the vagus specially implicated before the enlarged bronchial glands, or other local causes of excitation are established. The impaired function of the pneumogastric is shown by the rapid and weak pulse, epigastric tenderness, loss of appetite, *weak respiration and pulmonary congestion;* this aids the impulsion of morbid products to the bronchial surface, hence a specific secretion and a further source of *reflex irritation, hyperæsthesia, and the special character of the cough.* . . . . . As in the exanthemata, the rash shows impaired vasomotor nerve control, so in whooping-cough the irritability of the surface of the pharynx, glottis, and trachea is early evidence of disturbed innervation. . . . . . Spasmodic cough is not merely a reflex from the upper laryngeal nerve; *shallow respiration and insufficient oxygenation excite respiratory efforts, but if no more air gets to the blood, expiration is accentuated and goes on in a convulsive repetition, due to the stimulation of the medulla by the venous blood.* This happens on any interference with the supply of oxygenated blood to the respiratory centre, and from these efforts exhaustion results. *When the centre recovers its excitability, inspiratory effort follows,* and on the rapid alternations of these states much of the frequency, force, and duration of the paroxysms depends."

Carpenter, *Human Physiology*, edited by Power, ninth edition, page 361, in describing the influence of the vagi upon respiration, as demonstrated by the application of an electrical stimulus of moderate intensity to the central extremity of the divided nerves of one side, states—"The effect will be observed to vary according to whether the animal is breathing naturally or is in a state of apnœa or dyspnœa . . . . . In dyspnœa, when the blood is deficient in oxygen, all the accessory muscles are brought into action, and *the chest remains for a time in a state of tetanic expansion.* When the animal is depressed by chloral, and its nerves are exhausted, stimulation of the vagi, instead of being followed by contraction, is followed by relaxation of the diaphragm, which is regarded as the effect of the stimulation of the inhibitory fibres probably contained in the recurrents."

Whatever the ætiology of whooping-cough may be,—whether zymotic, neurotic, or catarrhal, I do not now propose to discuss,—one fact is perfectly plain, that the nervous element preponderates largely over the catarrhal. We may have irritation produced by the development of some special form of bacillus, but we have also hyperæsthesia of the terminal ends of the vagi in the lungs—such irritation taking a peculiar form in whooping-cough and giving rise to well-marked and characteristic phenomena; this I take to be admitted, however much we may differ as to the cause producing it. Granting, then, an excitable condition of that part of the nervous system included in producing a paroxysm of whooping-cough, it is but natural to suppose that, certain conditions being favourable, the nervous mechanism inducing an attack would be more active during the night than during the day, if, as has been shown, the respiratory centre exhibits less resisting power during the night, from the fact of the gradual depression of the breathing occurring during sleep, and most particularly at certain times, when the sleep is more profound, than at others, it permits the carbonic acid to increase in the blood from less frequent interchange, and at the same time facilitates the engorgement of the lungs, when the propulsive power of the heart is deficient (for venosity of the blood causes slowing of the cardiac rhythm), but only to the extent of rousing the respiratory centre to an action more or less excited; but as it is well remarked by Squire in *Quain's Dictionary of Medicine*, page 1772, shallow respiration and insufficient oxygenation (both of which obtain during sleep, and more particularly at those times when sleep is most profound) excite respiratory efforts; but if no more air gets to the blood, *expiration is accentuated and goes on in a convulsive repetition*, due to the stimulation of the medulla by the venous blood; but when the centre recovers its excitability (due, no doubt, to the excessive increase of carbonic acid), *inspiratory effort follows.*

If, then, we have during sleep a condition favourable for the action of the causes, whatever they may be, which induce a paroxysm of whooping-cough, when shall we look for them, if not at those times when the resisting power of the respiratory centre is weakened and to a certain extent disabled by the conditions resulting from sleep, and more particularly *profound sleep?* And if profound sleep is found to be more marked at certain times, then at these particular times we find the actual condition most favourable for the production of those effects which in whooping-cough is a paroxysm—in other cases, as those of emphysema, cardiac disease, or idiopathic asthma, result in a condition of dyspnœa sufficiently well known not to need description.

The reflex irritation passing along the afferent nerves of the pneumogastric to the respiratory centre leads in one case to an attack of asthma, in the other to a paroxysm of whooping-cough, and from the presence of conditions already described, is more likely to meet with less resistance than during the day, when the active state of respiration prevents these conditions attaining anything like the same proportions.

Let the circulation through the respiratory centre be strongly charged with carbonic acid, or, more strictly speaking, be deficient in oxygen, then the action of the centre tends to be excited and not rhythmical. If an irritation be transmitted when the centre is supplied with blood thus contaminated, an explosive nervous, as contrasted with a rhythmical, action takes place: in one case, breathing, deep and laboured, calling in the extraordinary but natural assistance of the auxiliary muscles; in the other, a condition of respiration in which spasm—prolonged nervous action—is manifested.

The susceptibility of the vagus to irritation varies in each individual, hence we find that in some children nocturnal paroxysms are less frequent or less severe, but we must not argue from this that other children will be the same; asthma, for instance, is induced in some patients with a great predisposition to the disease by very different causes than in others. The idiosyncrasy of the nervous system of each patient varies, and in a family of several children we may meet with quite opposite types of the same disease. This is well marked in whooping-cough: one child may exhibit less susceptibility to variation of temperature than his brother or sister, yet we must not infer from this that variations of temperature do not tend to induce and keep up the tendency to the occurrence of the paroxysms.

The effect of certain drugs used in the treatment of whooping-cough must never be overlooked. Narcotics, by their tendency to reduce the activity of the respiratory centre, add to the embarrassment already existing, and their administration must be carefully watched when from the peculiarities of any case they may be considered necessary, bearing in mind what I have quoted from *Quain's Dictionary of Medicine*, that shallow respiration and insufficient oxygenation excite respiratory efforts; but if no more air gets to the blood, expiration is accentuated and goes on in a convulsive repetition, the long-drawn effort of inspiration—the whoop—is but the centre rousing itself as a consequence of its excitability being brought to a culminating point. It is excessive, no doubt, but the exigencies of the case demand it. The chest has been emptied of air to a very unusual degree by the accentuation of the act of expiration, and a corresponding accentuation of the inspiratory act is necessary to compensate it; but the lungs are now receiving a much larger supply of oxygen owing to the tidal and residual air lost during the repeated expiratory act being made up, and we have no need for the repeated short inspiration such as was witnessed in the excited expiratory efforts. One long breath will compensate for all the short expirations, and will fill the chest as full as before the paroxysm took place; but, by the administration of drugs which do away with the influence of this compensating power of the respiratory centre, we permit danger to reach our patient. Let our remedies be such, that while tending to reduce the hyperæsthetic condition of the terminations of the pneumogastric in the bronchial mucous membrane, which is the chief cause in producing the paroxysm, we do not so influence the centre as to make it unable to appreciate the call of the organism for air, and thus allow our patients to die of asphyxia, from the non-excitability of the respiratory centre induced by remedies which we administered with a view to prevent the occurrence of those causes leading to a demand upon the susceptibility of the centre from increased venosity of the blood circulating through it.

This explanation of the causation of the increased frequency and severity of nocturnal as contrasted with diurnal paroxysms of whooping-cough is published with a view of supplying a want which is noticeable in our modern text-books in reference to this particular point.

The conclusion arrived at is due—(1), to my thinking over and comparing the tendency of attacks of asthma and whooping-cough to occur at particular hours of the night, these hours corresponding somewhat closely to the times at which sleep has been demonstrated by experiment to be most profound; (2), to the fact that the state of the respiratory centre at these times is in a condition most favourable for the production of excited, explosive, or spasmodic respiratory action, according to the condition of the nervous system induced by the respective disorders from which the patient is suffering, making allowance in all cases for individual peculiarity or idiosyncrasy.

I must express my indebtedness to the various writings of the late Dr Milner Fothergill for the clear manner in which he has written upon the necessity for paying strict attention to the condition of the respiratory centre, more particularly in those diseases in which we find the respiration is embarrassed either primarily, or as secondary to some concomitant disorder.

V.—CASE OF CONGENITAL ABSENCE OF THE RECTUM; COLOTOMY; AUTOPSY.

By Francis D. Boyd, M.B., C.M., Resident Surgeon, Royal Hospital for Sick Children, Edinburgh.

On Sunday, September 29, a child was brought to the Royal Hospital for Sick Children, with the history that it had been born at 3 a.m., and that it had been found to have no anus.

On examination the abdomen is rather distended, especially in umbilical region. There is an area of redness round the base of the cord. The urethra and penis are normal in appearance, but there is meconium and flatus passing per urethram. The perinæum shows entire absence of anus, no depression to be felt at the point where anus normally is.

An incision was made into the perinæum on to the tip of the coccyx, about two inches in depth, but no rectum could be found, and no appearance of any communication with the urethra. The patient was put on small quantities of Valentine's beef essence and wine whey, and kept warm.

On the following day, as Mr Bell was absent, Dr Playfair and Dr Burn Murdoch kindly saw the case with me, and we decided that the only chance for the child was to make an artificial anus. Accordingly, on October 1, the patient was put under chloroform, and, with Dr Burn Murdoch's assistance, I performed an inguinal colotomy.

An incision was made half an inch above the outer third of Poupart's ligament, extending upwards and outwards for two inches parallel to the line of Poupart's ligament. The peritoneum being opened, the portion of bowel which first presented was seen to be small intestine. On pushing that back we came upon what seemed to be the colon. The structure of the bowel could not be distinguished from the small intestine. It occupied, however, a position external and inferior to the portion which first presented. It passed, as far as could be made out, down into the pelvis. It seemed to have its mesentery attached to the left, and it was of a dark colour, and distended with meconium. A loop of this bowel was accordingly stitched to the abdominal wall with 14 to 16 silk sutures, which were passed through skin, peritoneum, and bowel. A dry dressing was applied, and patient put to bed—Valentine and wine whey being continued alternately every hour. In the afternoon patient was sick.

Oct. 2.—Has been quiet all day. Wound is looking well. Umbilicus not so inflamed. In the evening the bowel was opened under chloroform.

Oct. 3.—Wound looking well. Is passing meconium freely by wound. Put on bottle containing—Swiss milk, ʒ¾; barley water, ℥j.; lime water, ℥ss.; wine whey, ℥ss. Every two hours.

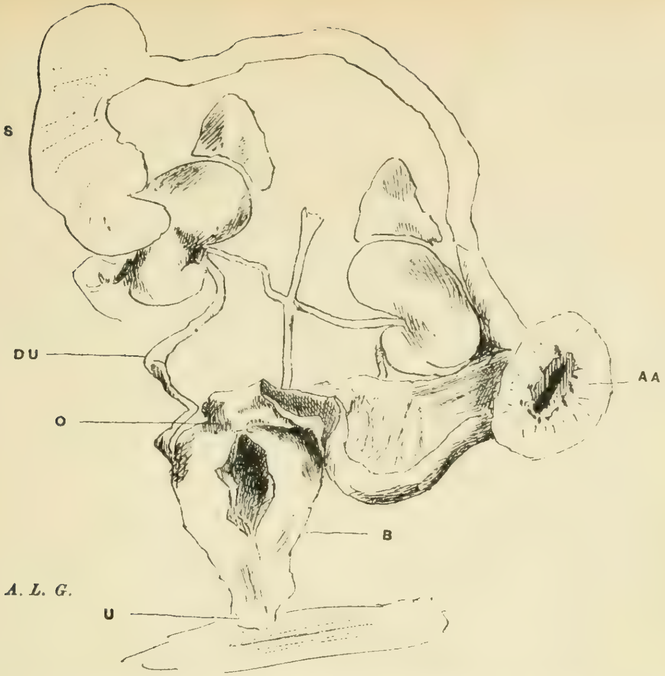
Oct. 4.—A little fretful to-day. Motions coming freely by fistula: are fairly well digested. Taking food well. There is now only urine with a dark stain coming by urethra.

Patient made good progress for some time: by the 6th the urine had become quite clear.

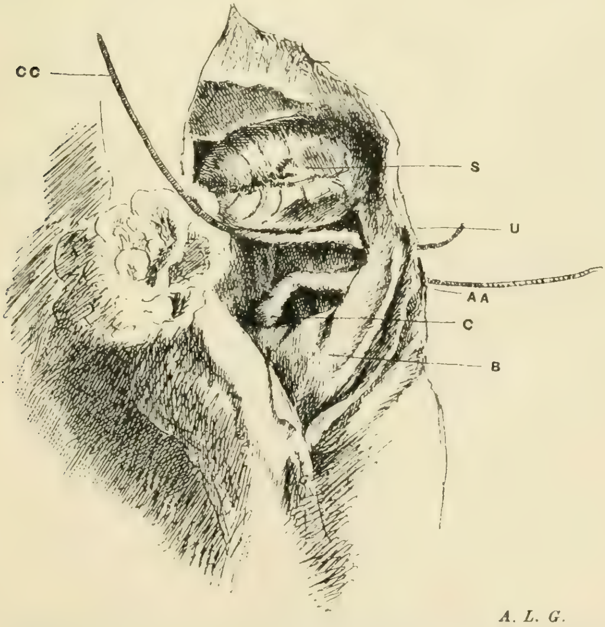
On the 9th patient was sick, and did not take his food so well. Became rapidly weaker; was put on whisky, ♏10 every two hours. By the 10th was too weak to suck. He gradually sank, and died on the morning of the 11th, ten days after operation.

The *post-mortem* examination disclosed the following peculiar abnormality:—

The stomach was much dilated. The large intestine below the splenic flexure was adherent to the abdominal wall, where it had



AA. Artificial Anus. B. Bladder, cut open. O. Connexion of Bowel with Bladder. DU. Dilated Ureter. U. Umbilicus. S. Cæcum.



AA. Artificial Anus. B. Pyriform Bladder. U. Umbilicus. C. Connexion of Bowel with Bladder. S. Stomach. CC. Catheter passed along Transverse Colon and through small slough close to Artificial Anus.

been stitched to the lips of the incision. The intestine below this was small and contracted. The bladder with its round ligaments was of a pyriform shape, and reached to within ¾ inch of the umbilicus. It had a firm fleshy feeling, and on incising it the walls were found to be extremely thick, measuring 1 centimetre.

At the lower part of the posterior wall of the bladder the bowel was firmly adherent at the point where the sigmoid flexure of the colon terminates in the rectum. At the point of adhesion there was a slight constriction, and above this a somewhat cæcum-like dilatation. On laying the gut open a catheter could easily be passed into the bladder. This was the only opening from the gut, the colon thus ending in the base of the bladder. The cone diameter of the colon above the fistula was ½ inch, that below the fistula $\frac{3}{10}$ of inch. Contraction had thus taken place to the extent of ⅕ of an inch. The colon was attached by a mesentery of 2½ inches in length.

The ureters were a good deal dilated. This, I think, was probably due to the high pressure within the bladder during the period before the bowel was opened.

The case from an operative point of view may, I think, be considered fairly successful. As the bowel was opened at the proper point there was no peritonitis, and the bowel had become completely adherent to the lips of the incision.

On examining the literature on the subject of congenital malformations of the rectum, I find there are records of about 120 cases. Of those there are 28 in which there is a communication between the intestine and the bladder or urethra. In the most of those cases the rectum communicated with the membranous portion of the urethra. There are only three cases recorded where the communication was between the bowel and the bladder. One recorded by Desault, where the rectum opened into the bladder. Colotomy was performed in the groin, with a fatal result in four days. One recorded by Amussat, where the rectum opened into the bladder between the ureters. Incision was made into the peritoneum, but the gut was not found, and the child died in two days. One recorded by Godard, where the colon terminated in a blind extremity at the brim of the pelvis, a small canal being prolonged to the base of the bladder. Incision was made into the perinæum with no result, the child dying in eleven days.

For the illustrations I am indebted to the kindness of my colleague Dr Gillespie.

VI.—HYPNOTISM, OR PSYCHO-THERAPEUTICS.

By R. W. FELKIN, M.D., etc., Lecturer on Diseases of the Tropics and Climatology, Edinburgh.

(*Continued from page* 445.)

PROCEEDING to examine certain objective signs, I may first refer to the behaviour of the respiration, and also the rigidity of the arm in the cataleptic state. If the arm is connected to a registering drum, it will be found that no curve will be produced in a hypnotised person, and that the respirations will be quiet and even. In an impostor the respiration will be quickened, and a distinct curve will be noticed upon the drum. Again, it is impossible for an impostor to simulate the neuro-muscular phenomena noticed in the lethargic state, as described by Charcot. The application of the faradic current will usually unmask an impostor if it is suddenly applied. In some cases it may be well to examine the supposed impostor with the ophthalmoscope, when anæmia of the retina will not be present if he is simulating, or the sphygmograph may be applied to the radial, as increased blood-pressure cannot be simulated.

Although the points just mentioned are, I think, sufficient for the purpose of detecting fraud, yet it should be stated that the absence of any one given symptom does not prove fraud. Lastly, it will not always do to take the confession of a person that he has been imposing as absolutely true. There are very many cases on record in which persons who have been really hypnotised have been perfectly unconscious of all that has happened, and who have been convinced in their own minds that they have never even shut their eyes, although it has been conclusively proved that they had been in the deepest hypnotic sleep for no inconsiderable time. It is as difficult, if not more so, to prove that a person is simulating hypnotism as it is to prove the simulation of insanity.[1]

---

[1] For full particulars on these points, see *Der Hypnotismus*, Moll, p. 168, *et seq.* With regard to this question, Van Renterghem and Van Eeden stated at the International Congress on Hypnotism at Paris in August 1889,—"In the majority of cases it is not difficult to ascertain scientifically the absence of all simulation or of intention on the part of the patient with respect to the phenomena presented (during hypnosis). However, we cannot produce absolute proof of the point in question. The anæsthesia, the catalepsy, may serve to convince the incredulous, but they are not incontestable proofs. Free will must be taken into account, and the sceptic crediting this acquiescence of the patient attributes to it the phenomena developed. This is a question of but little moment from our point of view. *Our sole desire is to cure our patients!* You could easily argue that the cure thus obtained might itself be only simulation. But such simulation is impossible. Is it impossible, for instance, for a patient to simulate the cure of cholera, asthma, incontinence of urine? A melancholic or a neuræsthenic can easily simulate amelioration should he wish. Granting such a possibility, what matter? It is still evident that this result is but the beginning of the cure. Surely, when the patients cease to

I think that it will be convenient to refer now to the real and supposed dangers with which the practice of hypnotism is surrounded; and in order to hold the balance level, a few quotations will be necessary, the value of which I will afterwards venture to criticise.

I gave a quotation from Crichton Browne. In the same letter from which that quotation was taken he goes on to say,—"But, as regards the genuine phenomena of hypnotism, the residuum that is left when all conscious and unconscious imposture has been winnowed out, a word of warning is desirable. There has been a revival of hypnotism of late years; popular works on the subject have been published. Scientific interest in it has been reawakened in connexion with recent physiological discoveries, and I have heard of hypnotic experiments having been introduced as a drawing-room entertainment. It is well, therefore, that it should be generally understood that hypnotism is not, as is sometimes imagined, a state of mental exaltation in which, or through which, glimpses may be obtained of an unseen universe, but a morbid condition tending towards mental enfeeblement and nervous degradation. I can at once recall three cases of insanity which have fallen under my observation, in which the mental derangement was brought on by mesmeric experiments operating upon persons of delicate organization; and I have no doubt that, were I to search my experience, I might bring a much more serious indictment against mesmerism or hypnotism as a cause of disease. My belief is that hypnotism is demoralising and dangerous to those who practise it, and that the amount of instruction to be derived from it is infinitesimal. It ought never to be resorted to for amusement or for the gratification of curiosity, and its employment for scientific purposes should be as guarded and economical as other experiments on living animals. Were it widely recognised that hypnotism is a depraved state of the brain and nervous system, and that only a small percentage of Englishmen and Englishwomen, and these certainly not the best of their race, are capable of being hypnotised, hysterical girls, effeminate youths, and credulous adults would be less prone to dabble in its tainted mysteries."[1]

When the Paris Académie des Sciences appointed a commission to investigate the subject of hypnotism in 1882, Milne-Edwardes, the president, read before the Académie a paper characterizing hypnotic experiments as dangerous to a condition of health already weakened by disease. Professor Harting of Utrecht told Milne-

---

complain, no longer desire to pose as invalids, this constitutes the suppression of the most disagreeable and tenacious of symptoms in this class of cases. Neuro-muscular hyperexcitability—phenomena which do not allow of simulation, have never been observed by us unless produced by suggestion (during hypnosis)."

[1] *British Medical Journal*, 1881, vol. ii. p. 378.

Edwardes that six fowls which he had often hypnotised all died of paralysis, so that Milne-Edwardes is of the opinion that experiments of this nature, capable of producing serious lesions of the nervous centres, ought not to be made on the human subject without the greatest caution.[1] Charcot strongly objected to this opinion.[2]

The upper Sanitary Council of Italy, after summoning to its aid some of the leading Italian physiologists and psychologists in 1886, passed a resolution to the effect that, "for the protection of the liberty of every person, it is essential to prevent experiments which, while abolishing the consciousness of actions, produce morbid psychical effects on predisposed persons, and render them subject to the will of others." The Council further expressed the opinion "that public displays of hypnotism must produce great disturbance on the nervous susceptibilities of the spectators, and maintains that physiological and clinical experience offer decisive proof of this assertion."[3]

The *Journal de Médecine de Bordeaux* publishes some facts concerning a case of hypnotism treated by Dr Pitres. The patient was one of those experimented on by Dr Gonetto in his public demonstrations at Bordeaux. This patient subsequently became subject to spontaneous attacks of sleep, during one of which he attempted to commit suicide. Dr Pitres treated him by cerebral electrisation, and prescribed complete intellectual repose and prolonged baths. This case affords strong evidence of the dangers attending hypnotic experiments by inexperienced and ignorant persons. Under Dr Pitres's treatment the patient is recovering; the periods of sleep are shorter, and occur less frequently.[4]

Referring to an exhibition in Paris, in which a hypnotised woman is made to expose herself to the most serious risks in a den of lions, the *British Medical Journal* says,—"We have frequently urged that the phenomena of hypnotism are of such a nature, and are attended by such disastrous results to the subjects, that they are quite unsuited to be made matter for public amusement. We venture to hope that, should this exhibition find its way to England, it will be promptly checked, in the interest alike of both the 'subject' and the public, whose minds cannot fail to be degraded by such unhealthy excitement."[5]

I may next notice that in 1887 Charcot himself publishes in the *Progrès Médical* a letter to Dr Melotti, in which he fully concurs with the official interdiction of public exhibitions of hypnotism. "It is clear," says he, "that they are not free from prejudicial effects. In the hands of amateurs it is not possible to point out the indications or contraindications of cases in which these may be participated in. This, however, is not all. The vulgar propagation of hypnotism in a more or less serious degree may follow in the assistance of these séances." Instances of this have occurred recently in Turin, Milan, and other cities. "Hypnotism," Charcot further observes, "is closely allied to hysteria, and may in the same manner become contagious or epidemic. Medicine alone can discover the therapeutic and physiological applications of hypnotism."[1]

---

[1] *British Medical Journal*, 1888, vol. i. p. 232.

[2] *Ibid.*, 551.

[3] *Ibid.*, 1886, vol. i. p. 1225.

[4] *Ibid.*, 1887, vol. ii. p. 636.

[5] *Ibid.*, 1887, vol. i. p. 1121.

Dr S. L. Trivus of St Petersburg relates a case in which a young man of 25, after having been hypnotised for the sake of experiment and subsequently awakened, could not return to his normal state for several hours. When left alone he was seized with a kind of hypnosis, and on his way home fell into a deep swoon in the street.[2]

In Belgium the subject of hypnotism has also created much interest,—so much so, indeed, that a Committee of the Belgian Academy of Medicine was charged with the task of investigating and reporting upon hypnotic experiments. The Committee has come to the conclusion that "they are dangerous, both morally and physically, and recommends the prohibition of them."[3]

In New York last year (1888) a discussion took place on the subject of hypnotism at the Academy of Medicine, and the conclusion was arrived at that, "however scientifically the subject may be treated, it is open to abuse, and its introduction into general therapeutics might be disastrous."

In so far as I know, these are all, or nearly all, the expressions of opinion which have been published in this country in recent years with regard to the subject in question. And I cannot think them of very great value. As will be noticed, they are chiefly from Continental sources, and the one or two independent views which we have quoted are given by men in this country who have had little or no experience of hypnotism. It will therefore be necessary to turn once more to the writings of Continental observers, and to seek for their expressions of opinion upon which to base a more satisfactory view of the whole subject.

It has been said by various authorities that hypnotism may exert an injurious or even irretrievable injury in many ways. Some opine that it may produce grave disorders of the nervous system, such as paralysis; others state that it will rob the individuals of their will-power, that it may convert a strong-minded person into a weak, vacillating fool. Others, again, believe that hypnotism greatly damages the intellectual powers, and others that insanity may be produced by its repeated exhibitions. Some there are who say that persons may be induced to commit crime which is instigated by the hypnotiser, and very many writers express the opinion that a person once having been hypnotised is liable to fall an easy prey to any charlatan he may come in contact with. Apart from these various charges which have been brought against hypnotism, we must remember those cases in which it is said that during the hypnotic sleep grave crimes against the person have been committed. It would be, I think, unnatural were not the power of hypnotism overrated by the laity, and it is, I suppose, as natural that scientific men should view it with suspicion. For these reasons I will endeavour to offer as much evidence as I can with regard to the possible, I do not say probable, injurious effects which hypnotism may produce.

---

[1] *Provincial Medical Journal*, 1887, p. 463.

[2] *Vratch*, No. 31, 1887, p. 607.

[3] *British Medical Journal*, 1888, vol. i. p. 567.

Baierlacher writes as follows:—"No unbiassed observer would deny that all kinds of mischief may be wrought by hypnotism, for the prevention of which legal regulations are necessary to forbid any save professional men to practise the art. It is true that thefts are committed, notwithstanding that stealing is forbidden and punished, and with little doubt we shall live to see many evils brought about by hypnotism. But on this account should we be justified in decrying a highly important therapeutic agent? I think not. In this connexion I am convinced that I must agree with Bernheim and Forel's aphorism—'Hypnotic suggestion used for therapeutic purposes in the hands of an experienced doctor is absolutely free from danger.'"[1]

I will now turn to Bernheim's opinions and give a summary of them. He has no hesitation in asserting that, according to his experience, hypnotism when well managed can do no possible harm. It does not interfere with the functions of organic life, nor are respiration and circulation affected in persons whose minds are free from fear, and even should nervous manifestations present themselves, such as muscular twitchings, shortness of breath, discomfort, and the acceleration of the pulse, they always disappear at a second sitting when the patient has gained confidence. In all his experience Bernheim has never seen any harm done by hypnotism induced by the suggestive method. He does not deny the possibility of some subjects preserving a disposition to sleep spontaneously after repeated sittings. This, however, he states, can be invariably avoided by the operator forbidding the patient to sleep save at proper times. He acknowledges, too, that there are individuals who possess such a susceptibility to hypnotism that they are in real danger should they come into contact with an unscrupulous operator, and he says that moralists are certainly justified in calling attention to this danger, and that they are right to condemn a practice which may rob a man of his free will without the power of resistance on his part. "They would," says he, "be a thousand times right if the remedy were not side by side with the evil." All that is necessary is for the operator to say to the patient, "Nobody will be able to hypnotise you in order to relieve you unless it be your physician." And he adds a remarkable case to exemplify the utility of such a procedure. He says,—"One day I tried to hypnotise an excellent somnambulist whom I had already hypnotised several times. I could not succeed. I called M. Liebault to aid me. He hypnotised her in a few seconds. I then asked her why I had not succeeded. She told me that, several months before, M. Beaumis had suggested during sleep that M. Liebault and himself were the only ones who could hypnotise her. This idea, written in her mind and of which she was not conscious in her waking condition, had forewarned her against me. Thus the danger of too great susceptibility to suggestion may be forestalled by suggestion itself."

---

[1] *Die Suggestions Thérapie*, Stuttgart, 1889, p. 30.

Referring to the objection that hypnotism may do grave harm to the mental faculties, even if used cautiously and with a therapeutic end in view,—that, for instance, the mind may be dulled, the intelligence demoralized, mental activity diminished, and a condition of intellectual torpor induced,—he states that experience will alone be able to answer the question. He has never seen any harm done himself, and believes that the hypnotic sleep in itself is as beneficial and as free from harm as natural sleep. Bernheim would not like to state absolutely that certain delicate brains predisposed to mental alienation might not receive serious harm from inopportune and awkward experiments from either hypnotic or post-hypnotic hallucinations. He has personally seen no harm ensue, but he thinks that in such cases experiments are dangerous, and should only be performed prudently and with great reserve. Bernheim fully realizes the dangers which may result after many experiments have been performed upon an individual, and he definitely states that, unless the operator has guarded against it, such individuals may be susceptible to hallucinations and suggestions at the hands of any one who knows how to force them upon them; but he goes on to say,—"Must we proscribe a thing which may be efficacious because the abuse of it is injurious? No one proscribes wine, alcohol, opium, quinine, because the immoderate or intemperate use of these substances may bring about accidents. Doubtless suggestion used by dishonest or awkward men is a dangerous practice. Law can and should intervene to suppress its abuse."[1]

Turning to Moll, we find that he quite appreciates the dangers of hypnotism, and also the objections which have been raised against its use, but he plainly states that no new method of treatment of any value has at once gained for itself universal approval. His expression of opinion agrees with that which I myself hold, and to which I gave expression at the commencement of this paper. He acknowledges that the possible dangers of hypnotism should be looked in the face, and he remarks that there are many drugs employed in everyday practice which, if not used with care, might act deleteriously; he thinks that in the use of any active drug three questions require to be answered with reference to its possible dangers: 1. Are we acquainted with the conditions in which danger may occur? 2. Do we recognise the dangers, and, recognising them, can we prevent them? 3. If we do not know the dangers or how to avoid them, is the benefit which we expect for the patient sufficient to justify the risk incurred? With regard to hypnotism, he maintains that the questions above propounded may be answered thus: We know the conditions completely under which hypnotism may act injuriously, and we are also in possession of the power in any given case to use the proper and harmless methods by which these conditions, and therefore the dangers of hypnotism, may be avoided. We cannot make this assertion with regard to many drugs. He believes "that any little inconvenience which hypnotism may at first cause is not to be compared with the benefits it confers."

---

[1] *Hypnotic Therapeutics*, Bernheim, 1889, p. 412, *et seq.*

Moll goes on to give in detail the various dangers which have been brought forward as possibly ensuing after hypnotism. We need not follow him very closely, although all he has written deserves attention. I will only refer to a few points.

It is stated that a general nervousness may follow hypnotism, that nervous people may become more nervous, or that nervousness may be produced in otherwise healthy people. This Moll admits, but he points out that in those cases in which these accidents have happened they have almost invariably been caused by the patient being hypnotised through fixing of the eyes and not through suggestion, or by the operator being ignorant of the subject. He holds, and I suppose all would agree with his opinion, that if a man professes to be able to hypnotise he should avoid faulty methods, as he would avoid making a false passage in passing a catheter. He gives three rules for the avoidance of any nervous phenomena following hypnotism: 1. Avoid as far as possible all long-continued stimulation of the senses. 2. Lessen as far as possible all suggestions tending to psychical stimulation. 3. Completely neutralize the suggestions that have been made before waking the patient.

Referring to another danger laid to the charge of hypnotism, namely, that of inducing hysterical convulsions, Moll says he must absolutely deny it—admitting, however, that hystero-epileptic patients may be thrown into convulsions during hypnotism. He considers that there is not a single case on record in which hypnotism has induced hysterical convulsions in any ordinary person, provided that it has been induced in a proper manner. He goes on to allow that occasionally certain slight inconveniences occur after the hypnotic sleep, such as drowsiness, lassitude, a heaviness in the limbs, etc. He thinks that these conditions are induced by auto-suggestion, and he justly remarks that they may be all prevented. He is perfectly willing to admit that the real and great danger connected with hypnotism is the strong possibility of increasing a patient's liability to hypnotic influence, as well as rendering him exceedingly prone to be affected by suggestions during the waking stage—that is to say, hypnotic sleep may be induced in such a person either against his will or without his knowledge, or in other cases he may be exceedingly prone to act upon a stranger's suggestion even although hypnosis is not induced. He agrees with Bernheim in saying that this condition may be absolutely prevented by means of suggestion before awakening, and he attributes its occurrence mainly to the use of the method of hypnotising by fixing the eyes.[1]

With regard to the dangers of hypnotism, Professor Obersteiner in Vienna says:—"I have repeatedly observed that many susceptible persons are unwell for a considerable time, it may be several days, after a single hypnotic sitting. Their nervous system is exhausted, and they are prostrated. Should they be often hypnotised, well-marked hysteria may be induced if they be predisposed to it. Many slightly nervous women may soon present a most typical hysterical condition after being repeatedly hypnotised. With this view an anonymous writer agrees, and maintains that if this can happen in grown-up people, how much more easily can children be affected.[2]

In referring to the possible dangers of hypnotism, especially with regard to medical jurisprudence, Forel is of opinion that it is far easier for an evil-disposed person to do harm to a hypnotised subject, by means of indirect and cunning suggestions, than by committing outrages upon him. He believes it to be possible for a hypnotised person to be murdered, to be robbed, or to be raped, just as it is possible for the same crimes to be committed on an insensible, idiotic, or apparently dead person, but it does not follow that such a crime would not be discovered. To this, however, we must refer later on. Dealing with the possibility of hypnotised persons being influenced to commit crimes, or to perform wrong actions on account of hypnotic suggestions, he calls attention to certain facts which should always be kept in view, namely, that individuals are influenced severally in different degrees, and that the individual factor of the character and the training received must be taken into account; and he holds, rightly I think, that each case must be judged on its own merits. It is necessary in dealing with this subject to consider in any given case—first, the individual degree to which the patient reacts to suggestions; second, the permanence of the impression produced by a suggestion upon the brain of the hypnotised person; third, the degree of hypnotic education the subject has received; fourth, the depth of the sleep in connexion with the loss of the power of resistance to suggestions; fifth, the psychical power of the operator, and his ability to produce in the patient vivid and effective suggestions; sixth, the normal individuality of the hypnotised person, taking into consideration the degree of his ethical and æsthetic disposition, his power of will, and his bringing up; seventh, the psychical condition of the person when hypnotised. The sixth point referred to is most important, because a person possessing loose morals will be, *ceteris paribus*, more liable to be acted upon by a criminal suggestion than a person whose moral sense is strongly developed.[1]

---

[1] *Der Hypnotismus*, Moll, 1889, p. 203, *et seq.*

[2] *Der Hypnotismus in der Pedagogik*, Berlin, 1888, p. 6.

(*To be continued.*)

VII.—CASE OF HEART DISEASE COMPLICATING LABOUR TREATED SUCCESSFULLY BY NITRITE OF AMYL.

By W. Fraser Wright, M.B.

(*Read before the Edinburgh Obstetrical Society, 24th July* 1889.)

I have much pleasure in recording the following case which occurred at the Glasgow Maternity Hospital during my period of house surgeonship. It illustrates the serious effect that labour has sometimes on heart lesions, and especially the danger to be feared in the third stage.

Mrs F., æt. 36, iii.-para, was admitted on the 7th April 1889 at 11 P.M. On admission she was in a very bad state, too ill, indeed, to give any reliable account of her condition. When called to see her I found patient sitting up in bed struggling for breath, any attempt to lie down being followed by a feeling of suffocation. The face was cyanotic and of a grey hue, lips livid, pupils dilated. In addition she was much troubled with severe cough and expectoration of frothy, muco-purulent, blood-stained sputum. The history (taken twelve hours after admission) showed that she was now in her third pregnancy, and at full time. The first two labours were easy and normal, the last occurring two years before. During each of these pregnancies there had been slight swelling of the legs and feet, but not nearly to such an extent as in the present case. Since birth of last child she enjoyed good health till five months advanced in this pregnancy, when she began to suffer from cough and shortness of breath. About same time her right leg and foot began to swell, the left side swelling only a fortnight before admission. There was no history of rheumatism. Labour pains came on early in the morning of the day on which she was admitted, and continued, though feebly, throughout the day. Owing to her serious condition it was impossible to make a thorough examination. The heart sounds could not be made out owing to the presence of adventitious breath sounds, râles and rhonchi. Pulse 130, weak, irregular in volume, and slightly in rhythm. Respirations 44. Temperature normal. Urine contained no albumen. She was put on 3 m. doses of tr. of strophanthus every hour, alternating with tablespoonful doses of brandy, and under this treatment she greatly improved, the breathing especially becoming much easier. In the morning it was possible to make out the character of the heart sounds. A well-marked murmur was heard in mitral area replacing first sound. There was also a faint presystolic murmur, both conducted into left axilla. Second sound somewhat impure. In the aortic area the first sound was normal, but the second was replaced by a murmur conducted down and heard very distinctly over base of sternum. Second sound in pulmonary area slightly accentuated. Other sounds normal. The lesion, apparently, was a double one—aortic regurgitation with mitral stenosis and incompetence. Numerous râles and rhonchi heard all over chest. On palpating abdomen the uterus was very large, and it was thought to be probably a case of twins, but of this I could not be sure, owing to the difficulty with which palpation was done while she was in a sitting posture. No fœtal heart sounds could be heard.

---

[1] *Der Hypnotismus*, Forel, Stuttgart, 1889, p. 67, *et seq.*

Per vaginam.—Cervix obliterated and os uteri slightly open. Only one fœtus could be felt, and it presented by head. There was much liquor amnii, ballottement being very distinct. Strophanthus was now replaced by digitaline granules, and she continued to improve during the day. Labour pains came on at intervals, and although not very strong, the os was dilated to size of a florin in the evening. The labour proceeded without any noteworthy incident, till at 10.30 P.M. on the 10th, *i.e.*, seventy-two hours after admission, she gave birth to twins. Both were very small and dead. There was a considerable amount of liq. amnii. The first child presented by head, and second by breech. The placenta, which was single, with two cords and two sets of membranes, was expelled with the second child. Scarcely any blood was lost, and no ergot given. Up till the birth of the placenta the labour was comparatively easy; the dyspnœa and distress from which she suffered on admission still existing to a certain extent, but not being so great as to cause much anxiety. Immediately the placenta was expelled, however, a sudden change took place. She was seized with great difficulty of breathing, coughing, and pretty free hæmoptysis, the face becoming very livid, so that she was in extreme danger of dying from suffocative engorgement of the lungs. The indication being, therefore, to relieve this great congestion, I was about to bleed her from the arm when I resolved to try the effect of nitrite of amyl. I had previously seen it do great good in a condition somewhat similar, viz., in a case of severe dyspnœa in a man, due to mitral disease. She therefore got five or six drops to inhale, and the effect produced was very marked; the breathing immediately became easier, the lividity of the face passing off and being replaced by red flushings. There was, however, not much bleeding from the uterus. Still, a small quantity of blood did escape, probably about 8 oz., but this is merely approximate. She continued in her improved condition, and was soon out of immediate danger. The digitalis was administered for a few days longer, and fourteen days after she left hospital feeling very well.

This case is, I think, of some interest in connexion with the similar cases recently reported by Dr Hart, and more particularly with regard to the treatment recommended. There can be no doubt that this woman was in imminent danger from the great engorgement of the lungs and right side of the heart. The indication, therefore, was to relieve this engorgement, and no doubt bleeding would have done so, but I had an idea that nitrite of amyl might have a beneficial effect; and so the event showed. How this came about I am not exactly prepared to say. But there are at least two possible explanations:—

The first is by its action on the uterus. I had hoped and expected free bleeding and relief in this way. But while there was a certain amount of blood lost after its administration, it was not, I think, sufficient to account for the immediate benefit she derived. Perhaps, however, it was. Although the action of this drug on the uterus has not been definitely determined, there seems some reason to believe, arguing from its action on non-striped muscle generally, that it will cause relaxation. Dr Barnes, indeed, recommends its use in some cases of tedious labour before applying forceps, and states that it will relax the uterus in two or three minutes. For this purpose he prefers it to morphia, chloral, or even chloroform. Then, again, cases are recorded in which it has caused (at least apparently) severe hæmorrhage when given in eclampsia. In this case I regret that the effect on the uterine muscle was not noticed, but from the fact of there being so little bleeding, I fancy there was not much relaxation.

The second explanation is, that by dilating the arterioles, and so allowing a greater quantity of blood accommodation in the periphery of the circulatory system, the heart would be required to hold less blood. I am well aware that this is open to serious criticism, as the effect of nitrites is to increase the amount of blood in the veins by dilating arterioles, but still it is not improbable that the extra amount of blood thrown on the circulation after the placenta is expelled may find room in the dilated arterioles and capillaries, and it is while these are filled that the quantity of blood in the heart is diminished. The fact remains, however, that great relief was obtained by its use, and this, I think, will warrant its being tried in suitable cases before recourse is had to venesection.

VIII.—NOTE ON A CASE OF LOSS OF MEMORY FOLLOWING CRANIAL INJURY.

By JOSEPH BELL, F.R.C.S.E., Surgeon to the Royal Edinburgh Hospital for Sick Children.

THE following case reported by the patient, a distinguished member of the legal profession, is interesting as bearing on the subject illustrated by a series of cases I published, with comments, some years ago. In this one the loss of memory has been permanent for certain subjects extending over a certain area of time preceding the accident. In all other respects the mental faculties are of a very high order.

"When 12 years and 10 months old I fell over a cliff at Howth, County Dublin. The cause of my accident was a kind of landslip, and I fell and rolled about 30 feet when I caught a bush, which gave way with me, and I fell about 30 feet more on to rocks. I was picked up quite insensible. My jaw was broken in four places, but no other bones. I am told, however, that my appearance was like that of some one who had been beaten into a jelly from head to foot. I have no recollection of the accident beyond holding on to the bush or bramble which gave way with me. Nor do I remember being picked up, nor anything which subsequently occurred, until about ten days after the accident, when I seemed to awake out of a long sleep, in great pain, and seeing Surgeon Butcher standing over me and setting my jaw, or doing something to it which caused me great pain.

"I was more or less incapable of doing anything for seven or eight months owing to the shock to my system. My father had died about seven months before the accident, and I am told that I used constantly to be with him, and that he was very fond of me, but I have not the smallest recollection of him, or what he was like, nor can I remember a single incident of my life before the accident, and, in fact, up to the time it occurred everything is a complete blank in my memory both as regards individuals and events.

"I am told that I was practically insensible for about a week after the accident occurred."

Part Second.

REVIEWS.

Debate on Anæsthetics. Journal of the British Dental Association, September 1889, *London.*

THE number of this Journal under review contains the report of a discussion upon Anæsthetics before the annual meeting of the British Dental Association, August 22, noteworthy as one of the most generally important of all the debates which have ever occurred in the history of that scientific body. Four papers, followed by a long and exhaustive discussion, were submitted to the Association: one on "The Use of Sulphuric Ether," by Dr Cruise of Dublin; another, "On Chloroform," by Mr W. B. Macleod of Edinburgh; a third, "On Certain Anæsthetic Mixtures," by Dr Frederick Hewitt, M.A. Cantab.; and a fourth, "On Recent Researches upon Nitrous Oxide Narcosis," by Dr Dudley Wilmot Buxton, B.S. and M.R.C.P. London.

Much that is of a purely scientific nature has been written upon all forms of anæsthetics. Their nature, their mode of action, their respective toxic effects, have all been subjected to exhaustive research, and been thoughtfully set forth over and over again. The practical teachings, however, of every-day experience are of more immediate value to the operator; and it is to these that the report under notice is more particularly directed. As the nature of the Association in which the discussion was carried on will imply, it was to anæsthesia in the more rapid and less severe forms of operation that the remarks applied, and in which cases certain anæsthetics necessarily entail less risk than if they were employed in the more protracted and serious operations of general surgery. Many of the operations of dentistry where anæsthetics are administered are frequently of so trivial and momentary a nature as scarcely to warrant their exhibition at all; while those of a more extensive, painful, and tedious description have been shown to demand the same anæsthetics as are found best fitted for the major operations of surgery. Of all the anæsthetics most suitably adapted in many ways for the first class of cases, Nitrous Oxide is perhaps the most convenient and satisfactory, since, not to speak of being sufficient to allay a patient's apprehension of suffering, it is in a brief operation comparatively safe, generally obtunding pain, and if judiciously given, in a healthy individual usually followed by no disagreeable effects. For the latter class of operations again—those involving considerable time and the risks of exhaustion or collapse—it seems to be admittedly unsuitable. Dr Buxton is well known as an authority on the subject of nitrous oxide, and much might be conceded to his judgment and experience; but even he, along with other partisans of this anæsthetic, somewhat overshoots the mark in estimating its superiority and the range of its services. Their remarks appear to be one of those instances of special pleading suggestive of the weak side. They failed to elicit what they seem to desire—an approval of the all but universal applicability and absolute safety of nitrous oxide anæsthesia in all cases. "Extreme rarity" of any untoward symptoms occurring under "Gas" is a conclusion which they assume to be on all hands accepted by dentists. Dr Buxton considers that his medical colleagues in pronouncing any patient as not strong enough to take gas, exhibit what he denominates "a curious dread of nitrous oxide," and he complains "that the average medical mind regards nitrous oxide narcosis as a modified form of asphyxia, and is prone to communicate this idea to the patient, who very properly translates asphyxia as being smothered or choked." He denies that the gas acts as an asphyxiant, and on this ground, with the exception of some cases such as where the heart is displaced mechanically, as by tumours or effusions, or is suffering from functional irrespective of organic disease, he finds little or no reason at all to interfere with its administration. He admits, however, in reference to the insensibility it leads to, that the "action of this substance, if any, must depend upon itself or upon the deprivation of oxygen," and allows that if it were a member of the class of asphyxiants "it would be most detrimental in every form of heart and pulmonary disease." Now, in reference to these statements it may at once be safely said, that untoward symptoms are not rare after the use of gas; that they do occur, and that they are and have been met with by the majority of dentists, death in some cases supervening, and being as clearly traceable to this agent as death is traceable to any other anæsthetic. The condition of a large number of persons quite warrants the dread or warning of a medical attendant that they are precarious subjects for nitrous oxide administration. The effect of nitrous oxide really is beyond all controversy a modified form of asphyxia, as whatever other action it may possess, all the causes and all the phenomena ot asphyxia accompany its use, and could scarcely fail to do so, seeing it is conceded that "it does not split up in the organism" so as to yield oxygen to the blood, and that no atmospheric air is admitted to the lungs during its exhibition, which was elsewhere also stated to be the danger when adding ether to this gas. And lastly, it has been proved by direct experiment that it is beyond all question powerfully destructive to life, as warm-blooded animals immersed in an atmosphere of it very speedily die. It may here, of course, be said, as one of the speakers remarked, that death in the human subject, when it occurs, is due to some error in its administration; but the same argument equally applies to all other anæsthetics as well, and calls for no remark.

Nitrous oxide was, accordingly, not in every respect borne out as constituting the summit of perfection as an anæsthetic, which some of its advocates would apparently make it out to be. The outcome of the discussion at the Association seems rather to have shown that it answers well enough in short and slight operations; that it produces, as a general rule, no unpleasant consequences when judiciously administered to healthy patients, who are neither too old nor too young to take it safely; but that it is quite capable under other circumstances of inducing symptoms which are equally distressing, equally alarming, and equally fraught with danger to life as either chloroform or ether. It was evidently granted that the physical symptoms so rapidly and so markedly produced by it could scarcely be denied to manifest its dangers; and, indeed, that only by its withdrawal, instinctively felt by any one administering it to be absolutely necessary and imperative on their appearance, did its safety in a great measure depend.

Anæsthetic Mixtures and the nature of their combined action is another interesting and important question, which has frequently engaged the attention both of the medical and dental profession, and their discussion was accorded a prominent place at the meeting. In the case of ether and chloroform being employed in conjunction, the anæsthetic effect produced is probably due in about an equal measure to both. This mixture is a very long known one, having been discussed in this the *Edinburgh Medical Journal* so far back as 1855. But in those more protracted instances where such a combination as nitrous oxide gas and ether is used, it appears not unlikely that the ether performs the lion's share of the work; while in those cases where nitrous oxide is employed merely as a preliminary step to exhibiting ether, the whole after maintenance of the insensibility may, from all that was adduced, be fairly set down to the ether alone. Dr Hewitt, whose paper originated this section of the discussion, also alluded favourably to two other mixtures, namely, that of ether and oxygen when given under increased pressure, and of alcohol, chloroform, and ether, the well-known A. C. E. mixture. With the first mixture Dr Hewitt describes the asphyxial phenomena as being considerably reduced; but with reference to the anæsthesia resulting he states, "I can only say it is *usually* of a very satisfactory kind," while the period of inhalation required is stated by him to be two or three times as long as when using nitrous oxide by itself. The A. C. E. mixture has been long known and considered to be less depressing to the heart's action on account of alcohol and ether being regarded as cardiac stimulants. Dr Hewitt seems to believe that the anæsthetic action of the mixture is mainly due to the chloroform. He is, however, of opinion that in many cases where chloroform or ether would be contra-indicated in certain bronchial or pulmonary or pleural affections, the A. C. E. mixture answers admirably, and that it is well fitted for either young children or persons over 60 or 65 years of age. Before departing from the consideration of gas and its compounds, the observations of Mr Bailey are too remarkable to be passed over. He is evidently an ardent admirer of nitrous oxide anæsthesia, and no doubt has devoted considerable attention to it, and possesses experience in its use. He advises practitioners not to be afraid of it. It was, he assured them, a pure anæsthetic independent of asphyxia. Somebody might die of it, but it would not be by asphyxiation. Of course, says he, if any one did die it would be said it was the gas. (What about deaths under ether or chloroform?) No case of death from gas, he stated, had ever been recorded. With regard to examination of the heart he thought it should not take place. A patient came to be anæsthetized; what were they going to do, he asks, if they found the heart wrong? They still had to anæsthetize somehow, and if the operator examined the heart and found something wrong it was likely to make him nervous. He therefore thought they did not gain anything by examining the heart. Mr Bailey is a bold if not a rash man. No doubt, "where ignorance is bliss 'tis folly to be wise;" but were Mr Bailey or any other party dubious about his own heart's fitness for anæsthesia, a preliminary examination might not be deemed injudicious, and upon the whole would be satisfactory, even if it did reveal something calculated not only to make the operator nervous, but to lead to the exhibition of any anæsthetic, whether it killed by asphyxia or in any other way, being positively declined. Wherever it is practicable in the administration of anæsthetics by dentists, and certainly in all anxious or protracted operations, not only the opinion but the presence of a fully qualified medical practitioner should be secured. The Dental Act takes no cognizance of deaths or death risks, or death certificates, as appertaining to the practice of dentistry. The risk of death in ordinary dental operations is remote and infinitesimal in comparison to the immediate dangers possible in the use of anæsthetics; and it does not seem at all clear that either the Medical or the Dental Act ever contemplated the certificate of fitness to practise dentistry to confer the privilege of undertaking single-handed any contingencies where life and death are imminently in the balance.

The discussion on the able papers of Dr Cruise and Mr W. B. Macleod on Ether and Chloroform respectively will be found peculiarly interesting and full of valuable information. It is customary in the south of these dominions for medical practitioners of all kinds, as well as dentists, to regard chloroform and even ether with fear and abhorrence. A considerable number of unfortunate cases, selected and tabulated among minor operations where these agents have been employed, may so far account for this. Another explanation may be, that the administration of anæsthetics is fought shy of by the general practitioners there, and handed over as a specialty to experts. In this way the training and experience of the general body of the profession in their use is restricted, with the consequent lack of any familiarity or facility in their administration. This creates a want of confidence on the part of the medical man, while a terror of them spreads as a natural result among his patients. The dangers ascribed to these dreaded anæsthetics seem, however, to be immensely exaggerated, if not unfounded. It need scarcely be said that no anæsthetic as yet known is capable of producing such a condition, either in man or any other animal, as insensibility to suffering and unconsciousness of existence—analgesia and anæsthesia—so complete that all cognizance of anything whatever should be entirely abolished, and the severest operation be enabled to be commenced and finished unknown to the patient, without incurring any danger. The condition induced is one much too unnatural ever to expect this, and it is indisputable that all such conditions may occasionally tend to fatal results. But every death which occurs during anæsthesia, as Mr Bailey observes in regard to gas, is not to be set down as due to the anæsthetic employed. If, as Mr Macleod justly argues, the use of every anæsthetic which might possibly be a cause of death were to be interdicted, it would amount to excluding every general anæsthetic whatever. The answer to all this, no doubt, will be that the object sought for is to employ the anæsthetic showing the smallest amount of danger to life in the record of its use. And this seems to be precisely the dilemma on which the whole contention rests. Nitrous oxide might be, in one view, as safe, or, as some argued, safer than chloroform, but its comparative safety altogether depends upon the duration of the operation performed. It seems hopeless to maintain that it was possible efficaciously, and without danger to life, to keep up the anæsthesia produced by it for the same length of time as in the case of ether or chloroform, so that any extra safety attributed to it becomes altogether conditional. In very short operations only was it generally considered fit to be used. Yet in this manner, upon the evidence afforded in these short operations, its safety is proclaimed. The number of dentists registered in Great Britain is considerably over 5000, but the number of medical practitioners registered is as much over 27,000. The operations for which these 5000 exhibit anæsthetics are almost invariably confined to extraction or some operation equally brief and simple, connected exclusively with teeth. In the cases in which the 27,000 exhibit anæsthetics, there is embraced, on the other hand, all those occurring in the whole range of medicine, surgery, and obstetrics. Yet it is not unfrequently found that the comparison between the absolute, not the relative deaths, occurring in the one and the other set of anæsthetics is employed in arguing as to their safety. The fallacy here need not be pointed out. That grave responsibilities attend the use of chloroform was admitted by Mr Macleod, and has been already long ago pointed out in these pages to attach to every general anæsthetic. But the causes and methods of death, as he says, have now been well ascertained, and it seems quite within the power of the skilled administrator to avert fatalities, except in those few cases, such as fright or idiosyncracy, which cannot be eliminated or detected. Its dangers, he pointed out, were above all others those of a respiratory nature, and its safety was enhanced according to the simplicity of the mode of its administration, and by making sure that the patient was thoroughly under its influence. No agent of the kind had been so well abused, so universally used, so misunderstood, and so carelessly handled. Yet after a fair and prolonged trial, as he also stated, and was supported by a very general voice in the meeting, chloroform appears to be of all others the anæsthetic which has best fulfilled its true purposes of immunity from pain, utter unconsciousness, relaxation of muscular tonicity, pleasant recovery, freedom from subsequent inconveniences, and last, though not least, extreme simplicity and pleasantness in its inhalations, and that not in minor alone, but in every form of operation. Dr Cruise speaks from a long and varied experience in both ether and chloroform, and his remarks are worthy of much attention and consideration; and while they apply to ether in very much the same manner as those of the other speakers do to chloroform, he considers ether, in dental operations of any duration, as the safest anæsthetic. Dr Stack also expressed his preference in certain cases for ether. Both chloroform and ether, however, he considered had their respective good qualities, and either he held to be vastly superior to nitrous oxide, both in respect to their immediate advantages in affording time to the operator, and also in their freedom from danger in several possible serious after effects upon the patient. He, however, considered that dentists ought to use all the anæsthetics according to circumstances, and with due care and circumspection, whether ether, chloroform, or nitrous oxide,—a position which seems to be the true and proper view of the whole question. Mr Hutchinson's judicious remarks on the precautions to be observed in administering anæsthetics are very much to the point. Not only does he uphold the propriety of patients being in the horizontal position—every article of dress being loose about them, and a proper interval having elapsed since last taking food—but he very properly commends to his fellow-practitioners that they should put themselves out of the way, and visit the patients at their own homes. This advice ought in every possible case to be followed, especially with chloroform or ether. Dr Bonwill's rapid respiration theory of anæsthesia was shortly discussed, but it seems as yet to be scarcely well enough worked out for much being said about it. He asked, however, that a fair trial should be given it, as he is convinced that it promises success.

History repeats itself. The conclusions arrived at by the British Dental Association's meeting might be summed up by quoting those of a paper appearing on the same subject some quarter of a century ago in these pages, and in which the teachings of the debate were all inculcated. In the *Edinburgh Medical Journal* for March 1866 will be found that the risks of asphyxia are pointed out as predominant, and the modes of averting them shown to be clearing out the fauces and pulling forward the tongue, both to be effected by the finger—not by forceps or any other instrument requiring to be kept in the mouth; that by anything of the nature of a gag, swallowing is held to be not only prevented, but a free access to the larynx kept persistently open, not alone for air, but for the entry of blood, saliva, or whatever other fluids are lodging at the entrance and endangering suffocation; that the dangers of syncope are dwelt upon, and its causes in many cases ascribed to insufficient anæsthesia—a whiff or mere driblet of chloroform being set down as a practice fraught with danger; that the horizontal posture of the patient in dental as well as other operations during anæsthesia is enforced: while undue reclination of the head backwards is also demonstrated as productive of evil effects; and, lastly, that the discrepancy of the appearances on post-mortem examination of a number of cases where death ensued during anæsthesia, pointed to its occurrence as being more likely due, in many instances, to accidental causes rather than to any toxic properties inherent in the anæsthetic itself. The Association is much to be congratulated on the results of the meeting of August 1889—in the thoroughness with which the question of anæsthetization has been there approached from every point of view.

Cyclopædia of the Diseases of Children. Vol. I. Edited by J. M. Keating, M.D. Philadelphia: J. B. Lippincott Co. Edinburgh and London: Young J. Pentland: 1889.

This is the first instalment of a very large undertaking, which is to consist, when completed, of four volumes, and to embrace "not only the medicine and surgery of pædiatrics, but also all the specialties tributary to it, as well as collateral subjects of interest and importance." It is, in fact, a collection of monographs arranged in the form of a systematic treatise, and devoted to the consideration of the anatomy, physiology, medicine, surgery, and hygiene of infancy, childhood, puberty, and adolescence; and even this broad scheme does not take in all the subjects treated of, as in the present volume, which contains almost 1000 pages, we find articles of considerable length on "Outlines of Practical Bacteriology," "Maternal Impressions," "Diseases of the Fœtus," "Wet Nurses," and "Embryology," which do not usually come under the head of diseases of children.

The articles are written for the most part by authors, both American and British, whose names are well known in connexion with the subjects treated of; and if the remaining volumes are conceived and executed in the same broad spirit, and with the same general excellence, the cyclopædia will be a valuable addition to the physician's library, as a storehouse of information in all that pertains to childhood in health and disease. The present volume is divided into two parts—Part I. dealing with general subjects, Part II. with fevers and miasmatic diseases.

Of the general subjects, the chapter on Anatomy, by Dr G. M'Clellan, is very well done, and is well up to date. To the practitioner it should be of great use, as it contains in a condensed form much information as to the size, exact position, and growth of organs which is not to be found in any treatise on the diseases of children, and which should help to make the examination of the child more exact and scientific. It is illustrated by a number of excellent photographs, a form of illustration which is much better suited to anatomical than to pathological subjects.

The chapter on Physiology, by Dr Angel Money, we do not think so good ; the subject is, as the author admits, "a comparatively unworked field," and there is nothing specially new introduced.

There is an excellent chapter on Diagnosis by Dr Finlayson, in which he rightly insists on the necessity for greater accuracy in this department. It contains many hints on the conduct of the examination of the sick child which may be read by practitioners, old and young, with much advantage. His remarks on "Diagnosis Made Easy—Teething," are very much to the point. He deals in an interesting way with the peculiarities in the disorders of the various systems of childhood.

In the article on "Maternal Impressions" a large number of cases are collected and compared, and the conclusion is arrived at that such impressions do occasionally, though not often, cause the defects which are ascribed to them.

The "Diseases of the Fœtus" form an interesting chapter, which has the advantage, not shared by most of the articles, of containing copious references to original sources. These would be out of place in a text-book, but in a book of reference such as this they are indispensable. The chapter on "Injuries to the New-born," by Parvin, contains a mass of matter not easily accessible elsewhere. "Infant Feeding," of course, requires a long chapter, in which the character and composition of human milk and the various milk foods are very fully and ably discussed, and all that relates to the preparation and administration of food to infants is reviewed.

In Part II., which contains articles on the Fevers and Miasmatic Diseases, *Malaria*, *Cholera*, etc., in detail, there are some admirable chapters. One of the best in the book is that on Diphtheria, by Dr J. Lewis Smith. It begins with a most interesting historical *résumé*. The disease, he considers, is due to a micrococcus, and does not originate *de novo*. It is extremely contagious, and is likely to be "the scourge of America in the future." There is much that is new as well as alarming in his views of the various ways in which the disease is propagated and spread. The questions of pathology cure, and treatment are discussed with a breadth of knowledge and experience which make it a most valuable contribution to the subject.

Rheumatism is the title of another excellent chapter, contributed by Dr Cheadle, who takes a broad and comprehensive view of the subject, and shows that the rheumatic state in childhood comprises a series of phenomena not limited to the joints and serous membranes, but extending frequently to the skin, mucous membranes, and even the central nervous system. Chorea, tonsillitis, erythema, pleurisy, tendinous nodules, are all common results of rheumatic disturbance. He insists strongly upon the vast importance of heredity in this disease. He attaches more importance than most authors to the appearance of fibrous nodules, and looks upon them as specially associated with chronic endo- and pericarditis.

Erysipelas, Yellow Fever, Dengue, Malaria, and Cholera Infectiosa, are each fully treated of in this part of the work.

The articles vary much in length, and are of unequal value. The majority of them, some of which we have briefly noticed, are of great and permanent value; one or two, as, for instance, the chapter on the Care of the Child at Birth, might have been omitted with advantage.

The type, printing, and illustrations are all good.

Hygiene of the Nursery. By LOUIS STARR, M.D. Second Edition. London: H. K. Lewis: 1889.

THIS little book is intended for the use of the mother, or other person in charge of children, rather than for the medical man, but doctors are often at a loss what book to recommend to their patients, and a careful perusal of this book has convinced us that it is admirably suited for this purpose. The chapter on "The Features of Health," which embraces a great many points in relation to the general look and habits of a child in health, and the changes induced by disease, as well as some notes on the rates of growth and development, is specially worthy of perusal. The principal chapters deal with clothing, exercise, sleep, food, dietary, emergencies. The whole book is good.

Nouvelle Méthode de Traitement de la Diphthérie. Par le Docteur G. GUELPA. Paris: Octave Doin: 1887.

THIS is a pamphlet containing three papers, two of them read to the Société de médecine pratique, and the other a letter addressed to M. Cadet de Gassicourt, and entitled "Premières Applications de ma Méthode de Traitement de la Diphthérie faites à l'hôpital Trousseau." Dr Guelpa's new method consists in the injection, by means of a rubber bag, through the mouth or nose, of a solution (one per cent.) of perchloride of iron in tepid water. He claims for it that it has been remarkably successful in epidemics of the disease occurring in Algiers in 1878, 1879, 1880, that patients submit readily to it, that it prevents extension to the larynx, that it can be made with the utmost facility, and that it is very inexpensive. The work, he explains, is preliminary to a more extensive treatise on the nature of diphtheria and its treatment.

The Comparative Merits of Tracheotomy and Intubation in the Treatment of Croup. By GEORGE W. GAY, M.D., Visiting Surgeon to the Boston City Hospital. Boston: Cupples & Hurd: 1889.

A REPRINT of a paper read before the American Surgical Association. Its tone is eminently judicial. The author comes to the conclusion that in a majority of cases of membranous laryngitis intubation may be done with a fair prospect that it will effectually relieve the dyspnœa for the time being, but that in severer cases situated at such distances or under circumstances in which only ordinary and not skilled assistance can be obtained in an emergency, tracheotomy is the safer method.

Du Catarrhe Chronique Hypertrophique et Atrophique des Fosses Nasales, de l'Ozène, Obstruction Catarrhales des Trompes d'Eustache, Végétations Adénoides du Pharynx, Traitement par la Galvano-caustique Chimie. Par le Dr GARRIGOU-DESSARÈNES. Paris: Adrien Delahaye et Emile Lecrosnier: 1888.

THE important chapters in this book are the 14th and 15th, in which the author describes at length a mode of treatment of these troublesome affections which he claims to have been the first to introduce into practice. This new method is that known as electrolysis (or, as the author prefers to call it, *la galvano-caustique chimique*), applied to the nose somewhat after the fashion in which Apostoli employs it in gynæcology. The earlier part of the work deals with the anatomy and physiology of the nasal fossæ, modes of examination, instruments used, the pathology, and older methods of treatment along with their inconveniences. In Chapter XIV. the action of electrolysis is considered, and the general rules to be observed in its application are laid down. Its immediate effects are said to be slight. A little pricking is felt at the commencement of the passage of the current, which soon disappears. There is no pain if the patient keeps himself steady, and is careful to avoid the movements of deglutition. This last point appears to be of importance, as in some cases there is an increased salivary flow excited by the current, and it is better to let this dribble from the mouth into a napkin than to swallow it. Several sittings are required, but these are not attended by any bad after-effects, and the patient can at once return to his occupation after each of them. The operation has been performed on women during the menstrual period and during pregnancy without any untoward results. In Chapter XV. more particular directions are given as to its application in the various forms of nasal disease, hypertrophic and chronic catarrh, ulcerations, Eustachian catarrh, adenoid tumour (after ablation), and epistaxis. A series of cases and statistics of an encouraging nature occupy the concluding chapter.

Phthisie Laryngée. Par le Dr A. GOUGUENHEIM, médecin de l'hôpital Lariboisière, et PAUL TISSIER, interne des hôpitaux. Paris: G. Masson: 1889.

THE distinguished editor of *Annales des Maladies de l'Oreille, du Larynx, etc.*, and M. Tissier, have produced a very excellent treatise on the very important subject with which it deals. Since the work of Trousseau and Belloc, published about fifty years ago, no complete monograph on this affection has been produced by any French writer. During that period there have been great advances in its pathology and treatment. The introduction of the laryngoscope has practically revolutionised all the older views of the disease, and since the discovery of the valuable properties of cocaine the treatment has been pursued with greater hopefulness than many of the earlier laryngologists dared to express.

In the work before us MM. Gouguenheim and Tissier have endeavoured to present as faithfully as possible the advances that have been made, as well as to show the actual state of our present knowledge and the views held by specialists in regard to these matters. The great experience of the former surgeon has been so utilized that the book is not a mere compilation of the writings of others, and we may say at once that we have seldom read the work of any foreign specialist with greater pleasure than we have done this one.

A short historical review prefaces a minute account of the pathology and symptomatology, the latter embracing an elaborate clinical study of the stenoses which may occur in tubercular disease of the larynx, and their various causes.

The chapter on treatment is naturally one of the most important in the book. The question of the curability is first discussed, and it is interesting to observe the array of names for and against. It is denied by Eppinger, Heinze, and Krishaber, but admitted by Ziemmsen, Schnitzler, Stoerck, B. Fraenkel, Schech, Moritz, Schmidt, Reichert, Krause, and Hering. Hitherto anatomical proofs, such as those furnished by Krause, Hering, and Seifert, have not been obtained in France, but prolonged clinical observations do not permit either Gouguenheim or Tissier to deny the possibility of cures being established.

Of the various medicaments, cocaine, iodoform, and lactic acid have in their hands proved more useful than any others; cocaine, because of the relief of pain and the removal of that other distressing symptom dysphagia; iodoform, because of its modifying influence on the tubercular ulcers; and lactic acid, the application of which by Krause and others has been followed with such excellent results as cicatrization of ulcers and diminution of infiltrations. It is, according to Gouguenheim and Tissier, "an ideal caustic, not diffusible, acting solely on affected parts, and readily tolerated by the majority of patients." In certain cases its application may be facilitated by the previous use of cocaine. After the employment of a considerable number of other medicaments, Gouguenheim has returned to the regular use of this medicament at his clinique in the Hôpital Lariboisière. Menthol, of which so much has been heard lately, particularly in the lay press of our own city, appears to be regarded with but scant favour. As an anæsthetic it is held to be inferior to cocaine, and nowhere is any reference made to Rosenberg's statement that it is inimical to the bacillus.

Turning to the more purely surgical methods, scarification, curetting, and galvano-cauterization, the authors do not speak very highly of the first of these. Curetting, as performed by Hering, they consider superior to it; but though healing has taken place after the application of the curette, the cure has never been a permanent one. The galvano-cautery has been found of use in the treatment of the polypiform vegetations and ventricular hernia as they occur in the course of tubercular laryngitis. Since the introduction of lactic acid it has been much less frequently employed. Tracheotomy is advocated for the cases of stenosis in which attacks of asphyxia are threatened, especially if the pulmonary disease is not far advanced and the temperature not too high. It is true this is not the teaching of the majority of specialists in this country, but our own experience, small undoubtedly, leads us to agree with it. There is no doubt that cases of laryngeal phthisis suffering from stenosis may be greatly benefited by tracheotomy. One disadvantage of it is that the tube must be worn for the remainder of life.

A short chapter on the general treatment brings to a close this really admirable work. A word of praise is due to the publisher for the excellent manner in which it is turned out. The colouring of the plates and the printing are all that could be desired, while the errors in the spelling of English names so commonly met with in Continental works are very few.

A Text-Book of Pathology, Systematic and Practical. By D. J. Hamilton, M.B., F.R.C.S.E., F.R.S.E., Professor of Pathological Anatomy, University of Aberdeen. Vol. I. London: Macmillan & Co.: 1889.

A SYSTEMATIC and practical text-book on pathology from the pen of him who, if we mistake not, was the instrument in the inauguration of practical pathology as it is now taught in Scotland, must attract very special notice. Professor Hamilton's reputation is not however, based on that alone, but on the stronger foundation of good work done in advancing the bounds of pathological knowledge in many directions. Those who know his work recognise the strong individuality and marked originality of it, however much they may disagree with some of his conclusions. These qualities, combined with the faculty of lucid exposition and an enthusiasm which is infectious, have always made him a teacher whose influence and popularity it would be difficult to rival. For all these reasons this work, the first volume of which is in our hands, has a quite unusual interest to Scottish pathology.

It is the only book in English which professes to be both a systematic and a practical treatise—in fact, the author's object is to make it a guide to the laboratory worker as well as to the ordinary student, so the scope of the work is wide.

The first part is devoted to such technical matters as the mode of performing a *sectio cadaveris*, under which he gives a specimen of a medico-legal report; the preparation of tissues for detailed examination and for permanent museum preparations; the microscope; and practical bacteriology, including the preparation of culture media and the examination of air and water. All this section shows him to be a master of laboratory technic.

Part II. deals with general pathological processes, and includes infiltrations and degenerations, inflammation, suppuration, healing organization, etc., and some points here deserve special consideration.

Under hypertrophy he includes what is now pretty generally defined as hyperplasia, a distinction which can hardly be ignored, and which is in accordance with facts acknowledged by the author: while atrophy has a new definition given to it which is too limited, and which, we venture to think, will not commend itself. That atrophy does result from "direct and continuous over-pressure" is certain, but this application of the term is too restricted, and it becomes still more difficult of application when it is complicated by the further proviso, that "the blood supply is not deficient." It fails to include even the diminution in size which undoubtedly occurs in the essential tissues of organs in senility and other states.

The section on inflammation is very elaborate, and contains the well-known experiments devised by the author for the elucidation of its phenomena. The purely mechanical explanation given of them is thought by many to be too rigid and too exclusive. Still the original work in this section is of very great value, and the study of inflammation in different tissues, including the cornea, will command much attention; in the latter of these, he has brought the process into harmony with that in other tissues. Of equal interest are the chapters on the healing of wounds and on organization, and includes the valuable observations made by the author on the use of sponge in the healing of ulcers.

The description of the various new formations and tumours is concise and lucid, and there is an absence of the somewhat appalling, composite nomenclature to be found in some books which includes the name of every tissue element and of every form of accidental degeneration in a tumour. The term cancer or carcinoma he considers inferior to epithelioma, and it may be a comfort to surgeons to know that such a distinguished pathologist holds that "the flat-celled cancer of the skin or of a mucous membrane is an easily eradicated tumour if treated early enough."

The chapters on the blood show that the author has experienced the difficulties which beset the pathologist on the threshold of the study of the so-called blood diseases, from the want of harmony and definiteness in the views of physiologists as to the life-history of the corpuscles. He examines at length the views and work on the subject, and leaves no room for doubt as to what he believes to be true; this definiteness of opinion has many advantages, and is not likely to impede the realization of fuller truth.

The concluding sections are devoted to the heart and the blood-vessels, and these illustrate what is also notable in the preceding sections, and is a new feature in a work on pathology, namely, the free use of clinical observations in the description of diseased conditions, their consequences, and the phenomena they produce.

There is an appendix on the making and painting of casts and models, which will be of value to those engaged in teaching.

A fairly copious bibliography adds materially to the value of the book as a work of reference.

The illustrations are numerous and excellent, and possess the great charm of having been executed by the author from his own preparations; in fact here, as all through the volume, the record is one of original and independent work and observation, and it is a record with which British pathology may well be gratified. We may express the hope that the second volume will soon appear.

Cancer and its Complications. By CHARLES EGERTON JENNINGS, F.R.C.S. Eng., M.S., M.B. London: Baillière, Tindall, & Cox.

ALTHOUGH the question whether cancer is to be regarded as a local or a constitutional disease is still open to scientific controversalists, practically there is little doubt that the surgeon shows his belief in its being local by the operative measures he is ready to adopt for its removal. The author of this volume is an ardent upholder of the latter doctrine, and in support of this view he quotes at great length from a discussion which took place at the Pathological Society of London some years ago. Although in that discussion arguments of great weight were advanced in favour of this view, and with a display of dialectics worthy of the metropolitan profession, there was, to a certain extent, a beating of the air and an avoidance of some of the more subtle problems suggested by cancer growth to the student of pathology. Leaving, however, this theoretical aspect of the book, there can be but one opinion as to the utility of presenting the whole question in the way Mr Jennings has done. He lays stress upon the facts, which are becoming more and more recognised, that cancer is often preceded by a local injury or by some other affection from which it seems directly to spring; but the fact that this is capable of a considerable weight of proof is evidence that there is a constitutional factor which cannot be ignored, and the determination of which is as far from definition as it has ever been. This factor may still be, as it has doubtless been in the past, exalted unduly, and has probably influenced surgeons as to the time and the manner of operative procedure, but modern surgery can hardly be charged with hesitancy or procrastination with the knife. Still, although this is the spirit of the time, a book of this kind is of service in again bringing the subject strongly and forcibly before the bar of the profession, and in again insisting on the fact that if cancer is to be treated with any hope of success by the knife, it is most likely to be so treated in its earliest beginnings. Another question which is dealt with is that the occurrence of cancer may often be prevented. The possibility of the truth of this stands in the same position as that of catarrhal affections in persons of the strumous or tubercular constitution, and is a consideration which the thoughtful practitioner will not lose sight of, while it gives an interest to the treatment of certain affections which they might not otherwise command. The author gives a sketchy outline of cancer in its several more common local seats, with indications as to the methods of operating. He is not even without hope that investigation may yet reveal some specific drug which will do for cancer what mercury and the iodides have done for syphilis. Altogether the book is to be commended to the practitioner for the lessons it teaches in promptness and decisiveness.

The Pathology, Clinical History, and Diagnosis of Affections of the Mediastinum other than those of the Heart and Aorta. By HOBART AMORY HARE, B.Sc., M.D., Demonstrator of Therapeutics, and Instructor in Physical Diagnosis in the Medical Department, etc., of the University of Pennsylvania. Philadelphia: P. Blakiston, Son, & Co.

THIS monograph was awarded the Fothergillian medal of the Medical Society of London in 1888. It is an elaborate investigation into the literature of mediastinal disease, in which 520 cases have been brought together, tabulated and discussed. The commonest affections occur in the following order of frequency,—cancer, abscess, sarcoma, lymphomata, and lymphadenoma. And the anterior is the most commonly affected of the three divisions of the area dealt with. Some suggestive points are indicated as a guide to differential diagnosis, but it need hardly be said that the various growths have many features in common. The essay forms the most complete work we have on the subject.

Archives of Surgery. Vol. I., No. 2. By JONATHAN HUTCHINSON, LL.D., F.R.S., President of the Royal College of Surgeons.

THIS second part of the *Archives* fully maintains the promise of the first. It is full of excellent material of real practical value. The first and largest paper, on Three Cases of Gangrenous Ulceration of the Arm after Vaccination, is a model of close reasoning based on acute observation, and conveys in its candid confession of doubt a most valuable lesson to those men who, like Macaulay, are "cocksure of everything." "Canons in Surgery" is the name given to formulated rules, such as the one in hernia, "When in doubt, operate." Dr Hutchinson gives some founded on his own experience:—Never give chloroform till the blood has been first well dosed with ether. Remove at once an eye which is so damaged by a wound that it can never be again useful for vision. Save the limb in all cases of compound dislocation or compound fracture into joints, unless they be attended by exceptional complications. Operate early in cancer.

Excellent reports of peculiar skin diseases, with plates, and a continuation of his paper on Intestinal Obstruction, are other features in this admirable volume.

The Pathology and Treatment of Displacement of the Uterus. By Dr B. S. SCHULTZE, Professor of Gynæcology, Director of the Lying-in Institution, and of the Gynæcological Clinique in Jena. Translated from the German by JAMESON J. MACAN, M.A., M.R.C.S. Eng., and edited by ARTHUR V. MACAN, M.B., M.Ch. etc., Master of the Rotunda Hospital, Dublin. With 120 Illustrations. London: H. K. Lewis, 136 Gower Street, W. C.: 1888.

SCHULTZE's views on the question of displacement of the uterus have from time to time been noticed and criticised in our pages, and it is with pleasure that we now see his full work translated and thus brought within easy reach, both of the specialist and of the general practitioner at home. The thanks of the profession are due to the translator, both for this and for the excellence of the manner in which he has performed his task. To quote from the editor's preface—"It would, perhaps, be too much to expect that the truth of the views here put forward should be at once acknowledged in all countries, but their acceptation will assuredly come. In the meantime our gynæcologists cannot but be influenced by such an example of patient and careful observation, while the illustrations which the book contains must tend to raise the standard of accuracy for all future gynæcological publications."

We notice that Schultze, in his preface to the translation, says—"In regard to those displacements which essentially depend on relaxation of the muscular attachments of the uterus—that is to say in many cases of retroflexion and prolapse—our therapeutics have been enriched by the knowledge of Thure Brandt's method of mechanical treatment," but no further reference to this treatment occurs in the body of the work. This is the system of treatment advocated by Dr Macan, the editor, before the British Gynæcological Society, and which was so keenly and adversely criticised by the members at one of their meetings lately. Even admitting the possibility of cures or improvements under the system, there can be little doubt that it is open to grave objections, and we would refer those of our readers who may not yet have had their attention drawn to the subject, or may not have come to a decision as to its merits, to a paper in the June number of the *American Journal of Obstetrics* by Boldt, an enthusiastic advocate of the "Manual Treatment in Gynæcology," and we venture to think that they will agree with us after its perusal, and on considering what the effects of the methods of examination and of treatment involved may be.

The Gulstonian Lectures on Secondary Degenerations of the Spinal Cord. By HOWARD H. TOOTH, M.D., F.R.C.P., Assistant Physician to the National Hospital for the Paralyzed and the Epileptic, etc. London: J. & A. Churchill.

THESE lectures are the record of a laborious investigation into an intricate subject surrounded by difficulties. In the first of the three lectures, the changes indicative of degeneration in the peripheral nerves and the cord are considered, and interesting points of difference are indicated. From that, the author proceeds to investigate various *systematic* degenerations, and has added materially to the definiteness of our knowledge by the careful examination of the cords of monkeys, operated on by Mr Horsley for that purpose. This is supplemented by an equally careful examination of cords in the human subject which had been injured in various ways. One of the most important and original points in the lectures is the contention that the fibres in the cord which go to the viscera are distinguishable from others, their position and course being even indicated. He has also made important observations on the vesicular column and the antero-lateral ascending column. Altogether the work is of a very high order, and is submitted with a want of ostentation worthy of its ability and value. To all interested in neurology the book is indispensable.

References to Papers in Anatomy: Human and Comparative. By JOHN STRUTHERS, M.D. Edinburgh: Maclachlan & Stewart: 1889.

PROFESSOR STRUTHERS, on the eve of retiring from the active duties of a teacher of anatomy, has prepared a list of his contributions to anatomical science. Appended to the title of each paper is a brief summary of its contents. Many of the earlier papers appeared in this Journal, and deal mainly with questions of medical, surgical, or pathological interest. The later ones are chiefly on comparative anatomy, and most of them are devoted to the description of various points connected with the structure of the cetacea.

In a prefatory note Dr Struthers states that he has prepared these references for the use of his present and former pupils. The idea is an excellent one, and the author may rest assured that not only his own pupils, but anatomists generally, will study this work with interest and profit.

It is earnestly to be hoped that, freed from the arduous duties of a teacher of anatomy in a large school, Professor Struthers may soon be restored to his wonted vigour, and that anatomical literature may continue for many years to be enriched by the fruits of his keen observation and wide experience.

Memoir on the Anatomy of the Humpback Whale—Megaptera longimana. By J. STRUTHERS, M.D. Edinburgh: Maclachlan & Stewart: 1889.

THERE is probably no group of animals which present more points of interest to the biologist than the whales, affording as they do so many examples of modifications of structure in adaptation to an aquatic life, while retaining all the main features of the mammalian type to which they belong. Notwithstanding the enormous size to which some of them attain, they present in their hair, teeth, and limbs numerous examples of rudimentary and apparently functionless structures. Thus Professor Flower found in a male razorback whale, 67 feet long, that the femur was only 1¼ inches in length and entirely cartilaginous.

This memoir is based upon an account of a humpback whale which appeared in the Firth of Tay in December 1883, and the subsequent exploits of which attracted a large amount of public attention. Dr Struthers apologizes for defects in his observations on the grounds that its scientific examination had to be subordinated to its public exhibition. Thus, when he commenced his dissections the proprietor advertised a special admission, with the additional attraction of a band of music. In spite of many difficulties, Dr Struthers has succeeded in making many interesting and original observations on its anatomy, more especially with reference to its skeleton and the condition of the muscles of its limbs.

This communication is only one of many other papers which have placed Professor Struthers amongst the front rank of cetologists.

Leprosy as a Cause of Blindness. By C. FRED. POLLOCK, M.D. (Glasg.) London: J. & A. Churchill.

THIS little book contains nothing new on the subject of the leprous diseases of the eye. It is a record of the cases seen by the author amongst the patients in the Norwegian hospitals last summer. Of 463 lepers, 41, or 9 per cent., were blind. The blindness was either a direct or indirect consequence of the specific disease. Of the indirect causes a common one was found to be ectropion with subsequent alterations in the cornea, from, it is inferred, exposure. No doubt, however, in many cases a paresis of the fifth nerve had conduced towards the corneal changes, as it is well known, although the author does not mention it, that exposure alone need not be followed by serious corneal changes. The eyelids were sometimes found to be disfigured from leprous deposits, and in such cases the eyes were often unaffected. Severe ulcerative destruction of the eyelids was also met with. In four cases of anæsthetic leprosy ulceration had led to a union of upper and lower lids along their margins. The cornea may be affected by the disease directly, either by the growth of a superficial tuber or by an infiltrating process.

Aids to Ophthalmic Medicine. By J. HUTCHINSON, Jun. London: Baillière, Tindall, & Cox.

A GOOD deal of sound and useful information is given in an unpretentious manner in this little book of 104 small pages. The author has no doubt had the advantage of the experience and mature practical judgment of his father, as he steers clear of many of the somewhat unpractical views which are prevalent among the younger ophthalmic surgeons. As the book is intended merely as an introduction to the subject, it does not call for any detailed criticism; suffice it to say, that it is sound so far as it goes, and, indeed, remarkably free from any serious faults. There is, however, room for improvement in most of the diagrams.

Account of the Life and Works of Maister Peter Lowe, the Founder of the Faculty of Physicians and Surgeons of Glasgow. By JAMES FINLAYSON, M.D. Glasgow: James Maclehose & Sons, Publishers to the University.

THIS is a most delightful little work, the result of great labour and research, which have been a labour of love, amidst his many other labours as teacher, author, and physician, to its learned author Dr James Finlayson. His position as librarian to the Faculty gave him both the will and the power to work at its records, and to try to make a life of the real founder of the Corporation. Out of few materials he has done much; but nothing is more astounding to our nineteenth century curiosity and historical zeal than the paucity and poverty of details as to really important lives which were lived less than three hundred and fifty years ago. This eminent man—for in many ways he was eminent: a scholar, a teacher, an army surgeon (Chirurgion-major) of large experience, an author who wrote works which were translated into foreign tongues, the founder of the Glasgow Faculty, an eminent and trusted citizen of Glasgow, has left so little to remind us of him that we do not know where he was born, when he was born, or even in what year he died. His tomb is in Glasgow, but lacks all these important particulars. It has a date, but it is probably an incorrect one, or one that tells only when the tombstone itself was cut, probably in the lifetime and by the orders of its owner. The admirable portrait which, reproduced in photogravure, is the frontispiece, makes us long to know more of the man. A fine, handsome, self-possessed courtly gentleman, with flowing curls parted from a lofty broad brow, noble honest eyes, that look out on the world with no prudish asceticism, but would outface an enemy or brighten to a pretty lass with equal vigour and delight; a firm but merry mouth, shaded by a brisk upturned courtly moustache over a peaked Charles I. beard. Well dressed, with fine linen and costly lace ruffles, his modish gauntlet gloves and smart snuff-box are with us still, would we had more of him. Some of his published works remain, a copy here and a copy there; some are hopelessly lost, like the books of Livy, and others which he quotes in the extant ones seem to have perished in manuscript —dead before the birth. From what Dr Finlayson quotes from what remains of his works, we can gather that Maister Peter Lowe was a shrewd, somewhat unbelieving surgeon. His shrewdness liberated his mind from much of the nonsense then taught. He had sense to see and tell us that in gunshot wounds there was no special "venenosity in the powder or burning in the bullet," and he cured many with no more difficulty than in other contused wounds. He describes pyæmia and scrophulæ, which he says is "alleadged by Frenchmen" to be cured by their king. Our Peter believes it not. He is a good hater, and hates and despises the "Barbors and other abusers." He had diagnosed a cervical aneurism in the person of his great friend the "valiaunt Captain Bayle;" he had called in consultation his good friend Andrew Scot of Paris, who agreed in his diagnosis; however, the apothecary sent for an "ignorant Barbor like unto himselfe." They promised the Captain they would cure him, opened the aneurism, and the Captain promptly died. Lowe knew well the nature and prognosis in cancer, understood the use of the ligature, and taught it long before Wiseman did. He describes the counterclift (contrecoup) in fracture of the skull, and gives an illustrative case, which he had verified by a post-mortem examination. His first work was on the "Spanish Sickness," for he wrote of it in France, and speaks of the "ignorant, malicious people who call it the French sicknes." We call it syphilis, and thus do not commit ourselves to geographical or ethnological false witnessing. Peter has a cure, which is good in summer and winter, young and old, recent and moderate, but, alas! he refers us to the "Poore Man's Guide," and the work was either left in MSS. or has been completely lost. For fuller details as to his surgical works we must refer to Dr Finlayson's delightful book. On three at least of the title pages of his extant work we find after his name the strange word "Arellian." This has been interpreted in various ways so as to mean that Lowe was a native of Ayr, of Errol in the Carse of Gowrie, that he studied at Orleans, and that he was a Paracelsist (Aureolus Paracelsus). Most of these interpretations seem far-fetched, but we are glad to see Dr Finlayson seems, on the whole, to lean to the hope that it means that Peter Lowe was born on the bonnie braes of the Carse. He finds Lows both in Errol and in Kilspindie, the next parish, about that period. Wherever he came from, he was a sturdy, warm-blooded, warm-hearted, hot-tempered Scotsman, and deserves well of Glasgow for the part he took in founding the Faculty. In a footnote we find that he dedicates one preface to some of his good friends, including Gilbert Primrose, indicating that he knew and respected an early President of the Royal College of Surgeons of Edinburgh, which was founded in 1505, ninety-four years before Peter Lowe's wisdom and foresight started the Glasgow Faculty. There is one lovely episode in his career that we long to hear more of. He was by order of the Presbytery called up before the Session and ordered to the "piller," and on his first appearance he had behaved himself with such levity that he had to appear again. What had he done? He was probably about 48 years old; he had been an army-surgeon in France for many years. Was it heresy, or was it something else? Anyhow he repented, and within five years he founded the Faculty and courted the "Parson of Glasgow's daughter, married her publicly" about 1604, and had a son in August 1605. The parson's daughter survived him, and within a very short time, how short we know not, for we do not know the date of his death, she married Walter Stirling, and thus came into the direct line of one of the oldest families in the west. Every one who "loves to look upon a man" some three hundred years after will thank Dr Finlayson for this delightful book. The paper, type, and general get-up are perfect; the title page is worthy of the press that printed Quasi Cursores.

Part Third.

MEETINGS OF SOCIETIES.

ROYAL MEDICAL SOCIETY.

Oct. 25.—First meeting of the One hundred and fifty-third Session—H. H. Littlejohn, M.B., in the Chair.

The Inaugural Address was delivered by *Professor Sir Douglas Maclagan*, before a large and enthusiastic audience of students and practitioners. A hearty vote of thanks was accorded to Sir Douglas Maclagan at the close of what was admitted to be one of the most brilliant and interesting addresses ever delivered before the Society.

Nov. 1.—G. W. Sanders, M.B., in the Chair. *R. D. Rudolf, M.B.*, read a communication on Cheilo-palato-gnathos, showing specimen and cast. The specimen of cleft palate and single harelip was taken from the body of a man 72 years of age, and was cut into transverse vertical sections, the cast representing the lower surface. It illustrated the new doctrine concerning the production of the cleft, viz., that it occurs between the two centres of ossification of, and not outside the premaxillary bone. The specimen showed that there was room for only one incisor tooth internal to the cleft in the alveolus.

Vaughan Harley, M.B., read a communication on Micro-organisms, showing microscopical preparations, and describing the methods of scientific research at the Sorbonne and Pasteur Institute. The Bacillus Diphtheriæ, Bacillus Pyocyaneus, Bacillus of Malignant Œdema, Pneumonococcus Fränkel, and Pneumonococcus Friedlander were among the specimens shown. An interesting account was given of the effects of inoculation with the various forms.

Reginald Horsley, M.B., read notes of a case of Cyst of the Posterior Extremity of the Inferior Turbinated Body, in which the complication of nasal reflex asthma existed. On the removal of the cyst the asthma was cured.

Nov. 8.—A. L. Turner, M.B., in the Chair. *G. W. Sanders, M.B.*, delivered his Presidential Address.

F. D. Boyd, M.B., read a communication on a case of Absence of the Rectum in a Child, details of which appear at page 529 of this Journal.

James Middlemass, M.B., read a dissertation on the Chemistry of Bacteria.

Nov. 15.—G. W. Sanders, M.B., in the Chair. *A. L. Turner, M.B.*, delivered his Presidential Address.

C. Wilson showed a case of Deformed Sternum.

R. O. Adamson, M.B., showed a specimen of Chronic Myocarditis, with microscopical preparations, showing the great development of fibrous tissue. The heart was much hypertrophied, and there was extensive atheroma of the arteries. The left ventricle was dilated, there was diffuse cirrhosis of the kidneys, some cirrhosis of the liver, and emphysema of the lungs.

New members up to date, 33.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By William Craig, M.D., F.R.S.E., Lecturer on Materia Medica, Surgeons' Hall, etc., etc.

Antipyrin in Renal Disease and Diabetes.—In the summer of 1888, while acting as honorary medical officer in the Cottage Hospital, Clevedon, Somerset, the following case came under my care:—On the card over the patient's bed was written, "J. B., aged 29, Bright's Disease." The matron informed me that the case was regarded as hopeless. The patient was admitted 3rd March 1888, having been in good health till two months before. She was then seized with a diphtheritic sore throat, and soon after ordered to Clevedon for change of air. On the journey she caught cold, and dropsy quickly supervened. From the date of admission to the following June, in spite of careful nursing and medical treatment, the malady made steady progress. I saw her for the first time on 14th June. There was the extreme general dropsy, the abdomen, face, and extremities being tensely distended with dropsical effusion; great pain in the back, especially in the lumbar region; intense headache, failing vision, heart action weak and irregular, difficulty of breathing, owing, probably, to a pushed-up diaphragm and pleural effusion; the skin sallow, tense and shining; urine scanty, of high specific gravity, and loaded with albumen; anæmia and general prostration. The condition of the eyes was very characteristic, the lower lids forming two distended, translucent pouches; but the condition of the ocular conjunctivæ was very striking; these were, to a large extent, raised from the sclerotic in transparent bladders of effusion, producing chemosis, and seriously endangering the circulation of the cornea. From previously slight, but inconclusive observations of antipyrin in renal cases, and with a view to relieving the intense headache, I ordered three grains twice a day, with strict instructions that the action of the heart should be carefully watched. The need for this precaution was quickly demonstrated by the increased irregularity, fainting, etc. On the third day the dose was reduced to three grains daily. From this date general amelioration of symptoms was observed, and constant, steady improvement recorded. After a few days a tonic supplemented this treatment, and in the course of three weeks a great change was observable. The dropsy was much less, the intense headache relieved, the appetite improved, and the patient much brighter and more hopeful. By the beginning of August she was up and about visiting friends. All traces of dropsy had disappeared, the general health was good, and there remained only a small quantity of albumen in the urine. On 13th August she was discharged for a convalescent home.

With regard to the effect of antipyrin in diabetes, I may be allowed to cite the following case, though incomplete. While spending some of the winter months at Arcachon, a clergyman, aged 64, consulted me. He had been in failing health for some time, and his French physician was treating him for diabetes. I found a large quantity of sugar in the urine. I again adopted the antipyrin treatment, and the same cardiac intolerance was manifested. I reduced the dose to three grains daily. At the end of three weeks his health was much improved, and the amount of sugar reduced to one-fourth. I afterwards lost sight of the case (M. H. Feeny, L.R.C.P. Ed., etc.)—*British Medical Journal*, 21st September 1889.

CAFFEINE IN THE TREATMENT OF ADYNAMIC CONDITIONS.—Nearly every one acknowledges the diuretic action of caffeine, but that it will act as a cardiac tonic and excitant when injected subcutaneously is not as generally admitted. Perhaps the reason for this condition is the fear as to the dangerous results of large doses of caffeine. In the *Revue Générale de Clinique et de Thérapeutique*, June 20, 1889, Dr Henri Huchard claims that the efficacy of subcutaneous injections of caffeine is superior as a cardiac stimulant to that of ether, and he refers to a number of cases which seems to substantiate his claim. One of these was a subject suffering from an attack of pneumonia on the right side, which, twelve days later, extended to the left lung, and during the entire attack was accompanied by intestinal complications of the nature of dysenteric colitis and profound depression. There was extreme feebleness, frequent threatenings of syncope, condition of sub-delirium, cardiac adynamia, and necrosis of the inferior maxillary bone. During the space of thirty days Dr Huchard practised ninety-five injections of 4 grains each of caffeine, fifty-one injections of ether, and nineteen injections of nitro-glycerin, the latter being administered to prevent accidents due to cerebral anæmia. The patient was cured, and no general or local accidents followed the one hundred and sixty-five injections, with the exception of a single abscess. In addition, Dr Huchard refers to five other cases, in which caffeine, employed alone, appeared to produce remarkable results. One of these was a severe case of typhoid fever, in which fifty similar injections were employed, and terminated in cure. Four other cases of severe pneumonia are also referred to, in which great benefit appeared to follow the injections of caffeine. Dr Huchard insists upon the necessity and the utility of large doses, and he writes that he does not hesitate to employ doses of from 30 to 45 grains of caffeine in twenty-four hours, administered subcutaneously. Dr Huchard claims that his experiments have shown him that caffeine, before acting on the heart, exercises marked action on the central nervous system; and there is thus, therefore, a marked difference in the action of digitalis and caffeine, the former being primarily a cardiac remedy, the latter acting first on the nervous system.—*Therapeutic Gazette*, August 1889.

PRIMULA OBCONICA.—In the *British Medical Journal* for 28th September, Mr Charles E. Oldacres, M.R.C.S., relates a series of cases in which a rash, in some respects resembling urticaria, in other respects resembling eczema, was produced by handling this common plant *Primula obconica.*

PHENACETIN IN WHOOPING-COUGH.—Dr R. Heinmann reports in the *Journal de Médecine de Paris,* 7th July 1889, that he has obtained excellent results in whooping-cough from the employment of phenacetin. In cases in which, before the treatment, the paroxysms of coughing occurred ten to fifteen times daily, they have, under this treatment, been reduced to three, and in some cases have completely disappeared within a few days, only occurring occasionally at night, when the administration of the remedy was suspended. The doses which he employed were as follows:—6 grains, given in four doses to a child 3 years of age; 5 grains, in three doses, to a child 2 years of age; 3 grains, in four doses, of about $\frac{3}{4}$ of a grain, to a nursing infant 7 months of age. Dr Heinmann writes that he never observed any unpleasant after-effects. It does not appear, however, that the remedy produced a cure, for it is stated that whenever the treatment is suspended, the paroxysms of coughing return with their original intensity and frequency.—*Therapeutic Gazette*, August 1889.

SULPHONAL AS AN HYPNOTIC IN INSANITY.—In an article on sulphonal as an hypnotic in insanity, Dr Samuel Garnier, in *Annales Medico-Psychologiques* for January and March 1889, reports at length upon its action in seventeen cases, and concludes that,—1. Sulphonal is an hypnotic of most remarkable intrinsic value in cases of insanity. In nearly every case of insomnia sleep was produced by doses varying from 2 to 5 grammes. 2. It proved efficacious in cases in which paraldehyde, urethan, chloral, and hypnone had invariably failed to act. 3. The advantages of sulphonal in the insomnia of the insane are,—The nature and amount of sleep procured by the doses mentioned, the facility in administering the drug by reason of its freedom from taste and odour, and also the fact that it has no appreciable effect upon the respiration, action, circulation, or digestion. 4. It should be stated in regard to its action upon digestion that, as in 5·5 per cent. of the cases it excited vomiting, and in 17·7 per cent. slight diarrhœa, its use is contraindicated when these phenomena appear. 5. The use of sulphonal is sometimes attended with vertigo and difficulty in keeping the equilibrium, the patient appearing in this respect as if intoxicated. It may possibly be contraindicated in the congestive forms of insanity. 6. The fact that it had a diuretic effect in 17·7 per cent. of the cases should be borne in mind when administering it. 7. Its calmative action by day is wanting even after a large dose, and if produced it is usually only a somnolence induced the day following the administration of the drug—a condition it is well to avoid by reducing the dose. 8. After divided doses, in the single instance in which it was so administered no sedative effect resulted by day, and scarcely any at night. 9. About one-half of the cases to which sulphonal was administered only on alternate days were comparatively restful during the intervening nights. 10. In sane subjects sulphonal appears, unquestionably, to provoke and prolong sleep, as well as to render it more profound. The phenomena attending its use are few and of little consequence. It is indicated in all cases of nervous insomnia.—*Boston Medical and Surgical Journal*, 18th April 1889.

Agaric Acid.—Pure agaric acid is a pure, glistening, light crystalline powder, which, under the microscope, is found to consist of quadrangular tabular crystals, and when recrystallized out of absolute alcohol forms groups of needles or rosettes. The melting point is 138° C. The free acid is only slightly soluble in cold water, but readily soluble in boiling water, and on cooling is redeposited in a crystalline form. All the salts of this acid, in combination with alkalies, are readily soluble, but the combinations with the heavy metals, on the other hand, are insoluble. Hofmeister has recently subjected the pure agaric acid to accurate physiological investigation, and his results are reported in the *Therapeutische Monatshefte* for June 1889. From the similarity of action of agaric acid and atropine on the sweat secretion, it is of interest to determine as to whether this similarity is restricted to this single point, or whether the parallelism extends to other actions. Of Dr Hofmeister's results, the following deserve attention from their general interest. In the first place, he found that if agaric acid should perhaps not be included among the list of actively poisonous substances, it was not to be regarded as inert. Applied locally, marked irritation is produced, which in subcutaneous injection produces marked inflammation, extending to suppuration. An internal administration of doses of from 7½ to 15 grains produces vomiting and diarrhœa. In cold-blooded animals it produces gradually increasing paralysis and reduction of activity of the heart. Agaric acid is, however, not capable of antagonizing the action of muscarine—that is, it will not, like atropine, start the heart of the frog which has been arrested through the action of muscarine. In warm-blooded animals, from the slow absorption, internal administration of the pure acid does not produce any very marked results. In subcutaneous or intravenous injections of the soluble sodium salt the action is principally exerted on the pneumogastric and vasomotor centres of the medulla; both centres are at first excited, and then paralyzed. As a consequence, there is at first slowing of the pulse and increase of blood-pressure followed by fall of the latter. Death follows after convulsion, from arrest of respiration; but in animals in which artificial respiration is maintained, death is not prevented, but is then attributable to great reduction of blood-pressure. Unlike atropine, agaric acid possesses no mydriatic action, but when the sodium salt is instilled into the conjunctiva severe irritation is produced, and, indeed, slight contraction of the pupil results, which may, however, be overcome through the use of atropine. The influence of agaracine on the sweat secretion is not central, but depends upon direct action on the secreting apparatus. Irritation of the sciatic nerve in animals under the influence of agaracine does not produce any secrétion of sweat, although pilocarpine is even now active. Therefore the similarity between atropine and agaric acid is confined to the influence of these two drugs on the sweat glands. The degree of action is, however, different. Thus Brobsting estimated that the influence of agaracine on the perspiration is twenty times less than that of atropine, and Hofmeister concludes that even this estimation is still too favourable for the agaric acid. Hofmeister's investigations would seem to indicate that for therapeutic purposes the pure agaric acid alone should be employed, and that when this substance is given internally there need be no fear of vomiting or diarrhœa. In Professor Kahler's clinic, doses of from ¾ to 2 grains were given repeatedly without any symptoms of poisoning, with the exception of transient nausea. Doses of from ⅓ to ½ grain were exceptionally well borne; and since the power of reducing perspiration only occurs several hours after the administration, and then persists for twenty-four hours, the employment of repeated small doses is greatly to be preferred to the administration of a single large dose. The subcutaneous injection of the soluble sodium salt is to be avoided, as tending to produce severe local irritation.—*Therapeutic Gazette*, August 1889.

THERAPEUTIC NOTES.

By W. A. Henderson, L R.C.S. & L.R.C.P. Ed., Chefoo.

Marion Sim's success with the glycerine tampon in congestion of the os uteri suggested to me its application to the skin in similar morbid conditions, but in the diluted form. Destruction of its tissue, especially in dependent parts, is accompanied by anasarca which mainly impedes healing. Now the application of the diluted glycerine rapidly reduces swelling, leads to the formation of healthy tissue and the final restoration to the normal state. The proportion which I have found most successful is that of one of glycerine to twelve of water, applied by means of absorbent cotton, kept in a state of saturation by frequent re-dipping in the mixture. One of my last cases was that of destruction of the skin in front of the wrist. The patient was in business, necessitating frequent use of the pen. In this case swelling was considerable. The application led to its speedy subsidence and final healing, while still continuing to ply the pen. The mixture also forms a most charming application in boils.

In the *Edinburgh Medical Journal* for March 1881 appeared my experience of the treatment of malarial fever, remittent and intermittent, by the combined action of the salicylate of sodium and quinine. During the four subsequent years of residence in Ningpo, I found those drugs almost invariably successful in suppressing continued malarial fever within the first ten days after its manifestation, and here I would supplement what was then stated. In Ningpo, latitude 29° 55′, the mean annual range of temperature is 9°, and in some months the range is as low as 5°. The degree of humidity thus indicated, together with surrounding rice-fields, decaying vegetable matter, and general high temperature, offer the conditions of malarial development. Of continued malarial fevers thus generated there are a variety of types, with a duration, so far as I have been able to judge, not infrequently inversely proportionate to the degree of pyrexia. When the temperature is high, as thus indicated, they may extend from a couple of weeks to about as many months; when low, *i.e.*, with a daily rise of about a couple of degrees, the fever may persist for as many years, or more. In the hot season of 1880, the above fevers became prevalent, and, quinine proving powerless, I determined to try it in combination with other drugs. The salicylate of sodium had previously, in the hands of others, been tested without success, but, owing to its hepatic action, determined by Dr Rutherford, I resolved to give it further trial as indicated. Of the salicylate the maximum dose used was 40 grs., with ten grs. of quinine, and that twice daily. A frequent method was to administer the drugs per rectum, and in the following way:—The salicylate was suspended in mucilage, the quinine dissolved in glycerine, and the two mixed. No rectal irritation resulted from the combination. As a rule the above was successful in all the forms of fever when recent. If no decided effects followed, calomel, in doses of gr. ⅓ thrice daily, was given, with readministration of the mixture. In recent continued malarial fever, when the temperature was a degree or two above the normal, quinine and ipecacuanha were very effectual, whereas quinine with benzoate of sodium was useless. Dr Begg of Hankow, in his Customs Medical Report for 1885, acknowledges the success of the treatment in Hankow, but prefers salicine in the combination to the sodium salt.

A further note I would make in regard to the treatment of aural sea-sickness. In the autumn of 1882, I had under treatment a case of epilepsy. As the depression occasioned by the bromide was considerable, I resorted to Brown-Sequard's method of the three bromides, to the great relief of the patient. Shortly afterwards I had a patient, an extreme sufferer from sea-sickness, about to make a voyage, upon whom I determined to try the three bromides. The potassium salt, given alone, is too depressing, the sodium salt ineffective unless in very large quantity, but under the influence of the three the voyage was made in comfort. Since then I have had a number of cases that have been equally successful. The dose suggested by Brown-Sequard, viz., a gramme of each of the potassium and sodium salts, and half a gramme of the ammonium at bed-time, and one-sixteenth of the combined dose after each meal, is generally sufficient. In a few days, when the influence is established, it may be suspended until the recurrence of the nausea. In many cases the nausea is due to hepatic derangement, in which case a dinner pill is all that is necessary.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. Allan Jamieson, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

I. Over-Fatty Resorcin-Salicylic Soap.—A resorcin soap has, till now, been a desideratum, but it was found hitherto that when resorcin was incorporated with soap it was decomposed with the production of a greenish-brown discoloration, and this occurred more rapidly in an alkaline soap than in a neutral or over-fatty one. But an over-acid soap is an impossibility, or rather has been. Salicylic acid, however, can be added to soap without injury, and this addition renders the further addition of resorcin possible, and such a soap is permanent. This mode of acidifying soap is of the highest value. The resorcin-salicylic soap contains 3 per cent. of each. It is particularly useful in diseases in which there is a hyperplasia of the epidermis. Such are eczema seborrhœicum, psoriasis, acne, and ichthyosis. In the milder forms, the soap can be used simply to wash the affected parts with, while in more severe cases it is rubbed firmly in, and the lather allowed to dry on, or the part can be enveloped in addition with guttapercha tissue.

II. Over-Fatty Salicyl-Resorcin Sulphur Soap.—Sulphur and its combinations are of recognised efficacy as dermato-therapeutic agents. The effect of free sulphur is that of a weak reducing agent. The soap contains it in the proportion of 10 per cent., and cannot, therefore, be used in acute inflammatory ailments, but rather in chronic ones with hyperplasia. It can be employed in various forms of acne and rosacea, in psoriasis, ichthyosis, and in eczema seborrhœicum with deeper infiltration of the cutis.

III. Over-Fatty Salicyl-Resorcin Sulphur-Tar Soap.—This is indicated in chronic eczema seborrhœicum, and psoriasis, contraindicated in acute, artificial, and neurotic eczemas, and in acne. In psoriasis it is most efficacious when the lather is not removed, but after it has become dry is covered with guttapercha tissue, so as to exclude the air and prolong the action of the soap.

IV. OVER-FATTY QUININE SOAP.—This contains 3 per cent. of sulphate of quinine, and Eichhoff has found it to cure tinea versicolor very rapidly. The parts are well washed with the soap and the lather allowed to become dry. In the intertriginous eczema in children it is also of use. The parts are washed with it, and subsequently dusted several times a day with an unirritating powder.

V. OVER-FATTY HYDROXYLAMIN SOAP.—Hydroxylamin is a powerful reducing agent, and has been recommended in psoriasis, parasitic sycosis, and lupus. The soap contains 3 per cent. of hydroxylamin, which Eichhoff has tested in two cases of psoriasis, and has not found toxic. It does not stain the clothes, but it requires further observation to decide on its value and safety.

VI. OVER-FATTY IODOFORM SOAP.—This contains 5 per cent. of iodoform. The odour is much masked by the soap. He recommends it as a disinfectant for the hands, and to lessen the chance of fissuring when corrosive sublimate solutions are made use of. It can also be usefully employed to cleanse and stimulate weak ulcers

VII. OVER-FATTY KREOLIN SOAP.—This also contains 5 per cent., and loses much of its peculiar penetrating smell. He has found it a pleasant agent for treating scabies in private practice. The body is washed with the soap and the lather allowed to dry on. Any excess can then be rubbed off with a dry towel. It is also of value in the treatment of impetigo contagiosa, employed to wash with some degree of friction. And, similarly, he has found it to cure hyperidrosis and bromidrosis of the feet. The feet are washed and well rubbed with the soap at night, and the lather allowed to dry on. In the morning they are once more washed with the soap. The stockings must be changed at least once a day.

Other soaps whose value is more theoretical are the *over-fatty ergotin soap, iodine soap*, and *salicylic creosote soap*. Cases are related in which each of these has apparently proved useful. Yet it is difficult to understand how a lupus can be cured by the use of a soap which only contains 5 per cent. of salicylic acid, even though the parts are covered with guttapercha tissue. If the "soap treatment," used in its newer signification, proves moderately successful, its cheapness and the facility and cleanliness which are its greatest recommendations, must speedily bring it into use.—*Monatshefte für praktische Dermatologie, Ergänzungsheft*, 1, 1889.

HAIR RESTORERS.—Lassar adheres to the opinion formerly expressed, that alopecia prematura and areata are both communicable diseases, and are best cured by parasiticide remedies. Indeed, he holds that premature baldness is a misnomer. No hairs should fall from a well-kept and healthy scalp. There is nothing in human beings corresponding to moulting in animals. That which resembles it is nothing else than the regeneration of hairs lost pathologically. In treatment, he advises that the scalp should be washed by an experienced hand for ten minutes daily for six to eight weeks, afterwards less frequently. In doing this a strong tar soap, such as Berger's tar soap, is the best to use. The scalp is to be well soaped, the hair for this purpose being divided into as many locks as possible, and then the soap washed quite away by means of an irrigator or small watering can, first with warm, afterwards with cold water. The cold douche prevents chill. After gentle drying, the head is now rubbed with—

℞	Sol. hyd. perch.,	0·5 : 150·0.
	Glycerini,	
	Sp. cologniensis,	āā 50. M.

Then rubbed dry with absolute alcohol with half per cent. of naphtol, and, finally, pretty freely with—

℞	Acid salicylic,	2·0.
	Tinct. benzoin,	3·0.
	Ol. Pedum tauri,	100·0.

If this procedure is persisted in from day to day, a satisfactory result will be attained in the large majority of cases.—*Sonderabdruck aus Therapeutische Monatshefte*, 1889.

A New Treatment of Favus.—Dr Schuster proposes as a new method of treatment, by means of which the cure can be completed in six or eight weeks, the employment of dry sulphurous acid gas. A pasteboard cylinder 20 inches high is made, open at both ends. The lower portion fits over the head as far as the forehead, the upper can be closed by a well-fitting pasteboard cap. A stage, made by stretching across pieces of string, is formed at the level of the lower third, the holes in the cylinder being closed by a piece of paper pasted on. On this stage a saucer containing sulphur can be placed. When in use the cylinder is put on the head of the patient and secured by an india-rubber band or by a bandage, so as to be air-tight. Through the upper opening a saucer containing burning sulphur is let down till it rests on the stage of threads of string, and the cover is put on. The patient remains sitting quiet for half an hour. The space contained within the cylinder is thus filled with sulphurous acid fumes; the sulphur becomes extinguished on the exhaustion of the contained oxygen. At the expiry of six or eight weeks of this treatment, which is repeated daily, the scalp is quite free from disease, the previously diseased parts have become smooth, white, bald places. While this is done the hair must be kept short, the crusts softened by means of oil, and the hair kept clean by soap and water. He suggests that this plan might also be found applicable to tinea tonsurans of the scalp.—*Monatshefte für praktische Dermatologie*, Band ix., Heft 1, 1889.

THIOL AND ICHTHYOL.—Thiol, a substance obtained synthetically by sulphuretting hydrocarbons, has recently been recommended as a substitute for ichthyol. It is inodorous, and is said to be chemically and therapeutically identical with ichthyol. Reeps has tried it at the Charité Clinic at Berlin, and reports favourably upon it. But his conclusions, which went to prove its superiority to ichthyol, have been called in question by Max Lange. He has subjected both substances under similar conditions to careful tests. In order to prevent any preconceived opinions he prescribed them as fish-oil No. 1 and No. 2. In rheumatism, in burns, and in eczema his observations were all in favour of ichthyol; the imitation proved inferior to the genuine article. He strongly recommends the prescription of ichthyol in lanolin, and asserts that burns can be dressed with bandages spread with the combination, and healing obtained without discontinuance of the occupation in suitable cases.—*Monatshefte für praktische Dermatologie*, Band ix., Heft 1, 1889.

TREATMENT OF LEUKOPLAKIA.—Rosenberg reviews the measures which have been adopted for the treatment of leukoplakia, and comes to the conclusion that none of those previously recommended are entirely satisfactory. He has found balsam of Peru to check the pain and to exert a favourable effect on the healing of the parts which are denuded of epithelium, and at the same time to clear up cloudy epithelial patches, and to cause deposits to disappear. In using balsam of Peru two points are to be attended to. One, that it is not possible in most cases to remove the affection by its means in a few days, or even in a few weeks, but, especially in cases of long standing, a correspondingly protracted application is necessary. Another, that it is essential that a pure preparation be employed, the drug being extensively adulterated. When there is much pain, and when fissures are present, it is to be applied by means of a fine brush. When thick deposits are present and the parts are not sensitive, a brush with stiff bristles must be selected, so as to act in some degree mechanically. After the application to the mucous membrane a slight burning sensation is experienced, while the saliva flows freely. The patient must be warned not to permit the balsam to be washed of by the saliva. The application should be made by the patient himself from one to three times a day, and the drug should be kept for at least five minutes in the mouth. He cites a number of instances, some consequent on syphilis some not, treated in this way successfully. So far there have been no recurrences, but he intimates that while Peruvian balsam locally applied can cause the disappearance of patches of leukoplakia, it can have no effect on the cause of the complaint; this should, therefore, in all cases be sought for, and if found, removed.—*Sonderabdruck aus Therapeutische Monatshefte*, 1889.

ALOPECIA AREATA.—Dr Bulkley has made this the subject of a clinical study, and has brought out some interesting points. In comparing statistics he finds that it is considerably more frequent among private than public patients; it is relatively much less frequent among the poor than the rich, which is, in a certain measure, an argument against its supposed parasitic origin, and in favour of its neurotic nature. It appears to occur far oftener in males than in females. Of 119 cases, 78 were males, 41 females. This proportion, however, varies somewhat at different decades. Thus the numbers were small in the first ten years of life, and four-fifths were females. The third decade, between twenty and thirty, exhibited the largest share—nearly one-third of all the cases—and the males were very greatly in excess, four to one. As to the date at which the disease began, the greater number began in this same decade. Spontaneous recovery takes place in a certain proportion of instances, but in Dr Bulkley's experience this is relatively rare, and in the large proportion is not permanent. He is a believer in the nervous origin and nature of alopecia areata, has invariably failed to find any parasitic elements in the hairs or scales, and in no instance was the disease presented in such a manner as to show contagion. In treatment we should endeavour to place the patient in the best hygienic condition, more especially as regards nerve nourishment. Thus fat and phosphates should be increased in the dietary, and fresh butter, cream, and the fat of meat should be largely partaken of. Milk is of much service if well borne, but should be taken absolutely alone in the intervals between meals, as one hour before each meal and at bedtime, provided this is three or four hours after supper, or the last meal at night. The phosphates are best in whole-wheat bread, and fish should be taken once daily. In medicines the free and continued administration of strychnia with phosphoric acid is recommended. Locally, stimulation of the diseased patches constitutes the whole treatment, and for this preference is given to veratria in ointment, of the strength of from five to ten grains or more to the ounce. The best results are obtained when the complaint is regarded not as a local affair, but as a manifestation of a constitutional derangement, having its basis in lowered vitality of the nervous system.—*New York Medical Record,* 2nd March 1889.

TREATMENT OF ECZEMA BY RESORCIN COMPRESSES.—Unna points out that till quite recently eczema was regarded as occasioned either by internal disorders or by external irritants. It is constantly, however, becoming more evident that in the greater number of cases we have to do with a parasitic affection. There are already, he states, a variety of types of eczema differentiated, and it is to be expected that for each a special form of cutaneous parasite will shortly be discovered. In future, in his opinion the conception of an eczema will be solely that of a parasitic catarrh of the skin. This will simplify the treatment. One of the best known and most widely diffused of such types is seborrhœic eczema. And this in individual cases can be cured by sulphur, resorcin, chrysarobin, or pyrogallic acid. Of these resorcin causes least of all any local or constitutional secondary effects. There is no stage, no part of the body, no individual condition of skin, no age, no complication of seborrhœic eczema, which some mode of using resorcin will not be found to suit. There is one way of employing it which combines intensity of action and certainty of result, with great simplicity of application. He prescribes—℞ Resorcini, glycerini, āā 10·0; sp. vini, 180·0. This is diluted when used with four times as much water. A thin layer of cotton wool is dipped in this lotion, applied to the part, covered with some impermeable material, as gutta-percha tissue or oiled paper, and secured with a bandage. Under this treatment the horny layer swells, and if the dressing is discontinued during the day, some oily material must be applied. Unna recommends his zinc-oxide salve pencils. The only contraindication is the rare resorcin idiosyncrasy when resorcin irritates. This has only occurred ten times in 2000 cases treated with resorcin. In these the resorcin must be discontinued, else the skin becomes painful and swells; and if still persisted in, large blisters form, and the œdema may spread from the point of application over the whole body.—*Monatshefte für praktische Dermatologie*, 1st July 1889.

TREATMENT OF HAIRY MOLES.—Dr Arthur Jamison has found ethylate of sodium the most satisfactory remedy in such cases. The hair is cut short, and the ethylate rubbed in with a glass rod till the surface assumes an orange or black hue. The crust so formed is allowed spontaneously to separate, for which a period of three weeks is sometimes needed. Most of the hairs will then be found destroyed, and a white smooth scar left. Any hairs which remain can be dealt with by the same method seriatim. In using the ethylate care must be taken that it does not run down on the sound skin, and the part treated must not be touched with water till the crust has come off.—*Practitioner*, July and August 1889.

DOES THE HUMAN CUTANEOUS FAT CONTAIN LANOLIN?—Liebreich from his researches concluded that cholesterin fat or lanolin occurred in those tissues which contain keratin, particularly in human epidermis, hair, and vernix caseosa. He also inclined to the belief that the eleidin of Ranvier was a mixture of albumen and cholesterin fat, and possibly was an early stage of the latter. Gottstein observed that lanolin was not permeable by bacteria, and deduced from this that the important biological function of lanolin was to protect the epidermis from the intrusion of parasites. Santi has made a series of careful investigations into these points, and finds that neither in the epidermis, cutis, or subcutaneous cellular tissue of the human skin are traces of lanolin discoverable, and that all the indications which Liebreich and others believed to point to lanolin related exclusively to the presence of cholesterin. This is indeed existent in unexpected quantity; whether it possesses the protective qualities which have been ascribed to the cholesterin fat remains to be proved.—*Monatshefte für praktische Dermatologie,* August 15, 1889.

ON THE VALUE OF FREQUENTLY REPEATED DOSES OF ARSENIC IN THE TREATMENT OF BULLOUS DISEASES OF THE SKIN, ESPECIALLY IN CHILDREN.—Dr Bulkley cites a number of cases in corroboration of the opinion, long since expressed by Jonathan Hutchinson, of the value of arsenic in pemphigus. Bulkley's cases embraced several varieties of bullous eruption. That form due to the administration of compounds of iodine and bromine, particularly with potassium, met with occasionally, yields best, after the discontinuance of the offending drug, to the free exhibition of arsenic. There was little in common in the instances cited by Bulkley, except the fact that each of them presented bullæ, and that they were mostly of a chronic character, with repeated accessions of eruption, until checked by the action of the arsenic. He specially draws attention to its value in children. As a rule, arsenic acts with them as a purgative before it manifests its other physiological effects, and it may generally be safely given in bullous eruptions in increasing doses. If the disease does not yield, it may be increased till it purges; then the dose should be slackened to a point where it is perfectly borne. In regard to the action of arsenic, he quotes Mr Hutchinson. "Arsenic does not merely repress the eruption, but remedies the unknown constitutional causes on which that symptom depends, always very much benefiting the general health of the patient. Arsenic does not prevent the liability to subsequent attacks, but such attacks are always much less severe than the original one, and tend, if treated by the same remedy, to diminish in intensity on each successive occasion." He administers the arsenic in many cases every two hours, and thus being taken apart from a meal at times it must be well diluted, at least in a third of a tumblerful of water. Vichy water is an excellent medium, rendering it more readily borne by the stomach, and sometimes also increasing its efficacy.—*New York Medical Journal,* April 13, 1889.

CLINICAL CONVERSATIONS HELD AT THE ST LOUIS HOSPITAL, PARIS.—*Giant cutaneous Tuberculosis spreading serpiginously.*—M. Fournier showed a girl, 10 years old, who had on the back and chest an immense collection of scrofulous abscesses. These seemed to have been started by a fall from a staircase. There was also evidence of tubercular mischief within. From the extent of the lesions there was risk of iodoform poisoning were that agent employed, and M. Besnier recommended lactic acid. At first a 10 per cent. solution should, he thought, be employed, followed by a simple dressing. If the application were well borne, the proportion of acid might be progressively increased. He considered that lactic acid had a very remarkable action on tubercular lesions. He could cite the cases of several phthisical persons in whom tubercular ulceration of the tongue had been rapidly benefited by the local use of lactic acid. The pain of the lactic acid could be lessened by preliminary applications of cocaine. He counselled the removal of the little patient to the seaside. *Late Syphilitic Roseola.*—M. Fournier showed a patient, a young woman, who had had syphilis five years before. On the trunk were roundish or oval patches, brownish, pigmented, slightly scaly, decolorized in the centre. Some were well defined, others faint. Search for parasitic elements, in particular for the trichophyton, was unsuccessful. Very reluctantly, M. Fournier was led to regard such as manifestations really specific, as a species of late, if not a tertiary roseola. It is remarkable that treatment has little effect on those pigmentary and scaly patches. In the case shown, specific treatment, employed for several months, had not cured the lesions. M. Besnier remarked that there was nothing to guide us to a decision whether these manifestations are or are not syphilitic. As they are seen at a late period in syphilitic subjects, one is led from statistics to regard them as specific. In leprosy, which presents so many analogies with syphilis, similar manifestations are found—pigmentary lesions, pityriasis-like, and very superficial. (Mr Hutchinson, *Syphilis*, p. 272, has related some cases resembling this, and believes that one cause, at least, is wearing flannel under-vests.—W. A. J.) *Itching in Lichen Planus.*—M. Quinquand drew attention to a common and remarkable fact that in those affected with lichen planus, even when pruritus is well marked, scratching leaves no trace. M. Besnier added that the same might be said of old men labouring under pruritus senilis, while those, on the contrary, who suffer from pruritus due to other causes (toxic, diabetes, etc.) bear evidences of scratching. In lichen planus, however, if, in place of the transient irritation of scratching, there is at any point permanent irritation, as from the pressure of corsets, garters, etc., or combined with this, the eruption is produced in abundance. M. Lailler also remarked that one does not find lesions due to scratching in urticaria. *Acne Keloid.*—M. Besnier showed a vigorous young man, aged 26, who for about two years had an eruption of little nodules, localized on the nape of the neck, exactly at the lower edge of the scalp, while elsewhere there are many spots of ordinary acne. Only a year before, the lesions commenced to run together, and this continued irrespective of treatment. There are now prominences, rising a centimetre or more from the surface, irregular in outline, and occupying nearly the whole of the nape of the neck. The surface is nodular, with a slight coloration, and exhibits scales, red crusts, and small papular eminences with a hair in the centre; also smooth cicatricial points, including either single hairs or bunches as in a brush. The association of the acne keloid of the neck and acne vulgaris elsewhere, and the apparent development of the more complex condition from the simpler, are interesting points. *Zoster.*—M. Vidal showed an instance of zoster affecting certain branches of the fifth nerve, the lacrymal, frontal, and external nasal. The internal nasal was not involved, hence there was no panophthalmitis. The cornea was not implicated, only the conjunctiva, but there was chemosis. M. Besnier drew attention to the fact that in this, as in all examples of ophthalmic zona, there was anæsthesia of the cornea, and though the case had already lasted ten days, this still persisted. So long as this continues it is necessary to watch the cornea.—*Annales de dermatologie et de syphiligraphie,* Nos. 3 and 4, 1889.

PERISCOPE OF OPHTHALMOLOGY.

By George A. Berry, M.B., F.R.C.S.E., Ophthalmic Surgeon, Royal Infirmary.

Transitory Amaurosis from Lead Poisoning.—Visual defects directly caused by lead are of rare occurrence. They appear not to be met with in more than 1 per cent. of cases of lead poisoning. When the sight is affected, there may or may not be ophthalmoscopic changes found to account for it. Most frequently there are. As a rule, either the optic nerve or retina is the site of the changes. Guensburg of Breslau has lately (*Archiv f. Augenh.*) added another to the list of published cases in which there has been more or less sudden blindness along with other symptoms of lead poisoning, and unaccompanied by any ophthalmoscopic changes. In all the cases the blindness has been complete, or all but complete, but the pupils have contracted to light. Recovery has usually taken place in a few days. In Guensburg's case the sight returned after a purge. The other symptoms of lead poisoning were marked. Little is known definitely as to the exact pathology of those cases. The only post-mortem examinations which have been made have not revealed anything definite. According to one theory, which has the support of Leber and Vulpian amongst others, the retention of the lead in the system acts as a direct irritant to the nervous system. Guensburg points out that this is a somewhat vague theory. Jaccoud and others hold that the cause is some transitory disturbance of the circulation; but here, again, there is an absence of any evidence from ophthalmoscopic examination, and yet it is known that very marked circulatory changes may be met with in the retina without any high degree of amblyopia resulting. Another hypothesis, recently advanced by Stood, is that an effusion takes place into the sheath of the optic nerve. Guensburg himself suggests that when the amaurosis occurs it is really uræmic, and induced by a disturbance with the functions of the kidney. This certainly appears to us the most plausible explanation, as it is unlikely, as Guensburg remarks, that a transitory effusion into the nerve sheath could give rise to such complete amaurosis.

SCHOELER'S OPERATION FOR DETACHED RETINA, FOLLOWED BY DEATH.—Dr Gelpke of Carlsruhe has published (*Centralblatt f. Augenh.*, September 1889) a case in which Schoeler's operation for detachment of retina (the injection of a few drops of tincture of iodine into the vitreous chamber) gave rise to panophthalmitis and meningitis which proved fatal. The operation appears to have been performed with the most thorough antiseptic precautions, so that there can be little doubt that the serious result was due to the introduction of the tincture of iodine. The benefits which Schoeler claimed for his operation have by no means been substantiated by the results obtained by others. Dr Gelpke's case, therefore, leads one to question whether the proceeding is not altogether too risky in proportion to the possible advantages which may be obtained by it.

ON THE ACTION OF LIGHT ON THE ANTERIOR PARTS OF THE EYE.—Experiments have recently been made by Widmark (*Nordiskt Med. Arkiv.*, 1889, xxi., 1) to determine the nature of the irritation produced on the superficial parts of the eye by the light of the sun and the electric arc. The arc light used was one of 1200 candles power. Both produced in the rabbit's eye a condition closely resembling snow-blindness and electric ophthalmia as they are met with in man, viz., chemosis, conjunctival injection, transient opacity of the cornea and conjunctival epithelium. The symptoms lasted from two to four days. In order to determine whether these symptoms were due to direct irritation, or to a reflex from the retina, a screen, pierced with a hole only 2mm. in diameter, was placed in front of the eye. All the experiments conducted in this way, by which, as the rabbits always turned their eyes to the light, the retina was subjected to the same stimulus, resulted in the absence of any action on the anterior portion of the eye. On the other hand, in experiments made in such a manner as to exclude the external tissues of the eye, the characteristic symptoms were always elicited. Widmark therefore concludes, as indeed is now generally held to be the case, that the irritant action is a direct one. To find out which are the most active rays he made use of glass and rock crystal, the former of which absorbs the ultra-violet rays, while the latter transmits these rays. The result was that little or no irritation was produced by the light allowed to pass through the glass, while the same strength of light passing through a crystal screen was found to be very active. Ultra-violet rays have therefore the property, independently of their action as heat rays, of producing an irritant action on the eye.

A NEW MYDRIATIC.—De Vriese of Bologna has recently (*Annales d'Oculistique*, ci. 3) tested the action of a substance, to which Merck has given the name of ephedrine, on the pupil. Ephedrine is an alcoloid got from the *Ephedra vulgaris var. helvetica.* It is soluble in water. In animals it causes death by paralyzing the heart and respiration. A 10 per cent. solution dropped into the conjunctival sac has the following effects:—It produces dilatation of the pupil in from forty to sixty minutes. The duration of the dilatation is from five to twenty hours, and the accommodation is not paralyzed. It produces no irritation or subsequent inflammation even after prolonged use. Merck has also obtained another alcoloid from the same source, which differs in some respects as to its physical properties: This, which is called pseudo-ephedrine, was found by De Vriese to have the following properties:—A 10 to 12 per cent. solution produces in man dilatation in from thirty to thirty-five minutes. This lasts six to nine hours. There is no paralysis of accommodation, nor any effect on the intra-ocular tension. From experiments which he has made, De Vriese concludes that the mydriatic effect is due to an action on both the terminal fibres of the oculomotor nerve, and on the fibres of the sphincter pupillæ.

AN ANALYSIS OF NINETY CASES OF CHRONIC GLAUCOMA, WITH SPECIAL REFERENCE TO THE EFFECTS OF IRIDECTOMY.—Although the beneficial effects of iridectomy even in chronic glaucoma have for long been sufficiently well established, yet any carefully prepared statistics, giving the results obtained by the same operator in a number of cases, are always of value. Dr Stedman Bull, of New York, has published his results in the *New York Medical Journal* for 10th August. In all his cases he performed a broad iridectomy, usually upwards, and as a rule with a lance-knife. In forty-five cases the operation was performed in both eyes simultaneously. Peripheral opacities of the lens existed at the time of operation in thirty-five out of 180 eyes, and the operation was found to have hastened the development of the cataract in twenty of those eyes. The following are some of the conclusions at which Dr Bull arrives:—1. It is one's duty to operate, and the earlier the better in chronic glaucoma. 2. If the disease appears to be stationary the operation may be delayed, when the patient can be kept under close observation. 3. The operation is sometimes followed by rapid, partial, or complete loss of sight. 4. The disease is bilateral in about 80 per cent. of all cases. 5. The condition of vision, central or peripheral, affords no guide as to whether or not an operation is likely to be successful. 6. The health of the patient is of some importance; any evidence of senility gives a bad prognosis.

EXTIRPATION OF THE LACRYMAL GLANDS IN THE ORBIT FOR INCURABLE EPIPHORA ALONG WITH GRANULAR LIDS.—Truc of Montpellier publishes several cases (*Archives d'Ophthalmologie,* July and August 1889) in which he has, he believes, got good results by removing the orbital portion of the lacrymal gland. He makes his incision through the eyebrow at the outer part, just over the orbital margin, and carries it down to the periosteum. In this way he finds that one comes readily upon the gland. He concludes:—1. That in certain cases of otherwise incurable epiphora and trachoma combined, a cure may be effected in this way. 2. The operation immediately stops the epiphora, and rapidly improves the granular condition. 3. It is particularly indicated in old-standing cases of both affections.

SURGICAL PERISCOPE.

By A. G. Miller, F.R.C.S.E.

Perforative Peritonitis (J. S. M'Ardle, F.R.C.S., *Dublin Journal of Medical Science*, September 1888).—In discussing the probable cause of death in cases of perforation, Mr M'Ardle comes to the following conclusions:—"The initial collapse of gastric perforation may be accounted for by shock, the subsequent gradual sinking being usually due to septic absorption. The early and prolonged prostration of intestinal perforation, especially when low down, may be accounted for by absorption of H_2S, accompanied by shock, the fatal issue being of septic origin. In both, when the intestine is overloaded, the products of putrefaction may come into play, and add the symptom of early cardiac failure. Now, if I am correct in the views above put forward, they teach a lesson of which modern surgery has already commenced the study—that is, that the cause of death is removable in those apparently hopeless cases."

Buried Animal Sutures.—Dr Henry O. Marcy, of Boston, in speaking of excision of the breast, suggests that the skin flaps might be brought together by sutures made subcutaneous, by using a "blind stitch, including the connective tissue just beneath the skin, from side to side." One long suture is made to do, being crossed both ways like a boot lace. Dr Marcy uses kangaroo tendon. He has had to give up catgut, because it always produced suppuration. —*The Journal of the American Medical Association*, 21st July 1888.

Laparotomy or Enterostomy?—Dr Rosewell Park, of Buffalo, N.Y., has, in the *Medical Record* for 3rd March 1888, a most valuable and interesting paper on the above question. His "conclusions" are as follows:—"So far, then, as a careful study of the recent literature of the subject would permit conclusions to be drawn, they are:—When a case of acute obstruction is seen *very* early, an exploratory laparotomy may be justified. When a case is seen very late, enterostomy is probably the only justifiable operation. Laparotomy is especially indicated when the nature of the obstruction is recognised or made reasonably evident; when a small tumour indicating intussusception is made out; when the obstruction is caused by neoplasms which can be removed; when the condition of the peritoneal cavity demands washing out or drainage. Enterostomy is especially indicated when no idea can be formed of the nature of the obstruction; when the malignant element precludes hope of permanent benefit; when the abdomen is particularly tympanitic; when the patient's power of endurance has been undermined by the severity of the disease. To put this somewhat differently, laparotomy is the more radical and surgical measure, but somewhat the more dangerous. Enterostomy is the safer and quicker, causing little or no shock, but entailing certain very disagreeable sequelæ, suitable for all desperate and some more favourable cases. Abdominal section should never be done *in extremis;* enterostomy may be. Since many cases are only seen by the surgeon in this terrible condition, the lesser operation will still find a wide applicability. It is accompanied by scarcely any danger of peritonitis, and may even be done under cocaine, so that the shock of an anæsthetic may be avoided. Cases die after laparotomy as the result not merely of the disease; an enterostomy, on the contrary, complicates the case, if at all, in extremely small degree."

WHAT IS CANCER? (James Braithwaite, M.D., *The Lancet*, 30th June 1888.)—In this thoughtful and suggestive paper we have the answer to the question, "Why is the proliferation of epithelium cells malignant when the proliferation of other cells is not so?" The epithelium cell is hardy, and can grow in any soil; it proliferates readily and rapidly; therefore in competition with other cells it wins. It is not rounded like a fat cell, nor does it felt into bundles like muscle fibres, and therefore in multiplying it does not press the tissues aside and form a capsule, but infiltrates them; besides, it is hard and incompressible. In this way an epithelial growth, like "an inverted wart," infiltrates the tissue that a fatty fibrous or muscular tumour would form into a capsule by pressing aside. Next comes the question, "How do the epithelial cells begin to infiltrate?" Dr Braithwaite answers this by supposing some injury to the basement membrane by which their escape is permitted. Such an injury must be very common without cancer following, and he therefore supposes that debility of tissue or system is present as an essential factor.

DISLOCATION OF HIP COMPLICATED WITH FRACTURE OF THE FEMUR (Fred. Kammerer, M.D., *New York Medical Journal*, 16th February 1889).—Dr Kammerer says that there are only 19 recorded cases of this rare complication, including one reported by himself. He goes thoroughly into the subject, with the following results:—Of the dislocations 12 were backwards, 2 were downwards, and 5 were forwards. The femur was fractured at the neck in 6 instances, and the shaft was broken in the remaining 16 cases. The double injury is usually the result of two separate forces, such as a fall and a blow. It is not possible to make out this exactly, for many of the cases are very imperfectly reported. The dislocation has usually been overlooked at the time of the injury, the fracture (especially when of the shaft) being much more evident. As regards treatment, reduction of the dislocation should be made at once. Where this has been done it has usually been found easy, which is not what one would expect. The manner of reduction has not been fully recorded. Pressure, manipulation, and traction are spoken of. Authorities are divided as to the probable usefulness of traction, some holding that the fracture makes traction of no avail. Dr Kammerer thinks that traction should be useful, because the limb is placed in a favourable position for manipulation and pressure on the displaced head; and because, especially when the shaft is fractured, the force will be transmitted through the muscles (vasti) to the upper fragment. Failing reduction by these means, Dr Kammerer considers that the fracture should be cut down upon, and the head of the bone reduced by traction and manipulation with the aid of strong forceps. Experimenting on the dead body, he found this easy. He used lion forceps. If reduction is not effected at once, Dr Kammerer recommends that the fracture should be treated with the limb in a useful position—*i.e.*, straight. The head of the bone acquires new relations, and the limb becomes fairly useful, as is well known. There is no advantage in trying to obtain union of the fracture in the altered and displaced position; for by the time the fracture is firmly enough united to make efforts at reduction possible, the likelihood of a successful reduction is not great.

SURGERY OF THE BLADDER (Dr Willy Meyer, *New York Med. Journ.*, 23rd February 1889).—In a paper read before the New York Surgical Society, 28th November 1888, Dr Meyer said:—"I shall not consider the question of the most preferable method of incising the bladder, whether above or below the pubes. Although I do not deny the great value of the median section in proper cases, I follow the views of my highly esteemed former chief, Professor F. Trendelenburg, director of the Surgical Clinic of the University of Bonn, and prefer suprapubic cystotomy, as not being in itself a dangerous operation. I adopt it, therefore, wherever I can, on account of its many great advantages—the easy manipulation inside of the vesical cavity, and the perfect view to be obtained by it of the interior of the bladder in the posture of the patient that has been advised by Trendelenburg. The points I propose to consider to-day, in reporting my own six cases of epicystotomy, are: 1. The best after-treatment of this operation; whether it be drainage of the bladder, or whether we should prefer suture of the bladder. 2. The great value of cystoscopy in the preliminary examination of the patient. Of the six suprapubic cystotomies recently performed by me, four were done for tumour of the bladder (one for recurrent cancer), one for stone, and one for foreign bodies. I shall report them chronologically."

Dr Meyer then reported these cases *in extenso.* The last-mentioned case is a very remarkable one. The patient had hæmaturia and cystitis, for which he was recommended to wash out the bladder with a solution of permanganate of potash. The cystitis not being relieved, Dr Meyer examined the bladder with the cystoscope, and discovered several foreign bodies. These, on removal, proved to be concretions consisting of *fibrin* and *potassium permanganate,* in almost equal proportions. The patient was completely relieved by the operation.

With regard to the question of suturing the bladder wound after cystotomy, Dr Meyer says:—"Summing up, I should say on this question: 1. If there is no catarrh of the bladder, or only a slight one, the wound of the bladder ought to be closed by primary, and eventually the abdominal wound by secondary suture. 2. If the catarrh of the bladder is serious and combined with septic suppurative pyelitis, the bladder ought to be drained. 3. In less serious cases both methods may be combined with advantage, *i.e.*, the greatest part of the wound of the bladder may be sewed up, and only a small spot in the middle left open for passing the tube."

Dr Meyer strongly recommends Trendelenburg's position as the best for seeing into the bladder. In this position the patient's pelvis is raised to a considerable height by hanging him up with his legs over an assistant's shoulders. "Every surgeon who has once employed this posture for intravesical operation has enthusiastically acknowledged its great advantages. It is by all means preferable to illumination of the inside of the bladder with an electric surgical light while the patient is flat on his back. The electric light ought to be used rather in that posture when the trigonum Lieutaudii, usually the most interesting spot within the bladder, is not covered by blood and urine, but, being the highest point in the bladder, may be sponged and kept dry. To be entirely satisfied with the posture, it should be kept in mind that the pelvis is to be raised sufficiently high. Many fail in that respect. Eigenbrodt emphasizes the fact that this elevation helps the surgeon to avoid the prevesical peritoneal fold at the time of incising the bladder."

There are many other interesting points in Dr Meyer's paper dealing with the details of the operation of cystotomy and the examination of the bladder with the aid of the cystoscope. At the conclusion of his paper he says:—"I could add many an interesting experience yet which I have had in the last few months. Unfortunately, I cannot support all that I have seen in the various bladders by ocular inspection at the operation. A few of the patients refused the operative interference which I had proposed in order to effect proper treatment. Nearly all of them were examined at my office by myself alone, no colleague being present to verify my cystoscopic diagnosis. But, nevertheless, I may be allowed to mention in passing that I have seen in this time, among twenty more or less interesting patients, five tumours of the bladder, two stones, once foreign bodies, and the case of gangrene of the mucous membrane as just reported. There is no doubt, in my opinion, and no surgeon doubts it any more to-day, that cystoscopy has a great future. Of course we have to learn yet in many respects to recognise the disease in the picture, and may often be mistaken in explaining what we see, just as that occurs to the laryngologists, and very experienced ones, with diseases of the larynx. In many obscure cases of the uropoietic system the cystoscope will render great aid in arriving at a correct diagnosis. But to be successful with it, it requires a close study and a great deal of personal experience."

Part Fifth.

MEDICAL NEWS.

Royal College of Surgeons, Edinburgh.—The examination for the Gold Medal presented to the College by Colonel William Lorimer Bathgate in memory of his late father, William M'Phune Bathgate, Esq., F.R.C.S.E., known as the "Bathgate Memorial Prize," took place on the 26th October. The examination was a special written one in Materia Medica and Therapeutics, and was open to all candidates who have passed the Second Professional Examination for the Triple Qualification of the Royal Colleges of Physicians and Surgeons of Edinburgh and Faculty of Physicians and Surgeons of Glasgow during the current year, and who have attended the course of Materia Medica required by said Board at the Edinburgh School of Medicine. Three candidates underwent examination, and the medal has been awarded to Mr Charles Edward Lester, student of medicine, 14 Melville Terrace, Edinburgh. Through the kindness of Colonel Bathgate this Prize will be awarded annually.

The "Pinol-Eucalyptia" Dry Inhaler.—We have much pleasure in recommending this inhaler to the notice of the profession. The instrument is small and most convenient. It is filled with absorbent sawdust, and is an excellent means of administering the volatile oils which are suitable for inhalation.

Messrs Burroughs, Wellcome, & Co., the well-known manufacturing chemists, have sent us along with this inhaler, as a specimen, a very pure sample of the oil of Eucalyptus globulus, which they designate Eucalyptia, and also a sample of Pinol, both of which essential oils are most useful medicines, and the purity of the specimens sent are everything which can be desired.

MESSRS BURROUGHS, WELLCOME, & CO. have sent us a sample of Fairchild's Pepsin in scales, which is an elegant preparation, and excels all other preparations of pepsin with which we are acquainted.

MESSRS BURROUGHS, WELLCOME, & CO. have also sent us a specimen of their "Thermo-Safeguard Feeding Bottle." This is the best of all feeding bottles. The shape is such that there are no angles or corners in which the milk can coagulate. It is also graduated into ounces, so that the nurse is able to see how much the child takes at each feeding—and having a thermometer in the bottle, the temperature of the milk is easily observed. It ought to be universally used.

THE same firm has also sent specimens of copies of their Duplicate Prescription Books, which are useful, and enable the prescriber to preserve copies of their prescriptions.

WE have also received from the same firm a copy of their A.B.C. Medical Diary and Visiting List, which we have much pleasure in highly recommending to the favourable notice of our readers. The book is elegantly got up, and contains much valuable information, and has an A.B.C. Materia Medica, especially of those new and rarer drugs which are now so universally used in the treatment of disease. The book is enclosed in a beautiful morocco case.

CANTICLE SUNG AT THE ANNUAL DINNER OF THE SCOTTISH MIDLAND WESTERN MEDICAL ASSOCIATION, GLASGOW, 11th October 1889 (1489?).

Cum al ye jollie Doctouris quha toille in streete or glenne,
I'se telle ye off ane secraite consultantis dinna kenne:
Quhat is ye greatest blysse ye Practitionour can name?
It's to heare ye wee bit skyrle quhan ye weanne cumis hame.

Quhan ye weanne cumis hame,
Quhan ye weanne cumis hame,
Quhydder gloaman, noone, or mirk,
Quhan ye weanne cumis hame.

It's no in pompe or pallaisis yat wee can hope to dwelle,
In monie ane awsome handlin wee haif to take ane spelle;
Quhydder riche or puir we tend to us its al ye saime,
Gin wee heare ye wee bit skyrle quhan ye weanne cumis hame.

See yonder sonsie Surgeoun, sae couthie and sae slee,
O' scyence, sense, and lear, ane unco store has hee;
Wi' brichtlie beamin' e'e, he spiers nor prayse nor blame,
Gin hee hears ye wee bit skyrle quhan ye weanne cumis hame.

Quhan ye weanne cumis hame,
Quhan ye weanne cumis hame,
Quhydder mornin, noone, or mirk,
Quhan ye weanne cumis hame.

Gowanlee, Slamannan. J. B.

OBITUARY.

RICORD (1800–1889).

THIS illustrious French Syphilographer is no more. Those of our readers who were present at the International Congress of Dermatology and Syphilography will recall the figure of the amiable octogenarian who presided so happily at the closing banquet, and it is not without deep emotion that they will recall the words with which he ended his toast: "*Je n'ai pas le courage de vous dire adieu, je préfère vous dire au revoir.*" Seeing him then so full of life, no one would have thought that our venerable President would be so soon taken away from the friends who surrounded him.

A few weeks ago he had a pneumonic attack, which he appeared to be getting over, thanks to his robust constitution. He had even dared to take a drive. A relapse caused by fresh cold soon exhausted his strength, and in spite of the great care of MM. Potain Bouchut, and Piguot, M. Ricord died on Tuesday, 22nd October, at 3 o'clock A.M. Philippe Ricord was born on 10th December 1800, at Baltimore,in the United States, of French parents. His grandfather had practiced medicine in Marseilles. After having made his preliminary medical studies in the country of his birth, under the direction of his eldest brother J. B. Ricord, the distinguished naturalist, he came to France in 1820 with another brother, Alexander, and both were registered at the Faculty of Medicine at Paris.

Received as hospital resident in 1822, Ricord assisted Dupuytren and then Lisfranc in the hospital *de la Pitié*. He passed his inaugural thesis in 1826.

Not having sufficient means to remain in Paris, the young doctor now went to Olivet, near Orleans; then he established himself at Crouy-sur-Ourcq, a little village near Meaux. But he had not given up the project of pursuing his studies, and soon he quitted his humble post in the country to compete for the place of Surgeon to the Hospitals of Paris. He was nominated in March 1828. He entered the hospital *du Midi* in the following June in place of Bard. It was in this hospital that he spent his career, until he was forced to retire from age at the end of the year 1860. He quitted the hospital, where he had worked for 30 years, with a fame which will not perish.

Placed on new ground for him in the midst of the chaos which then reigned on the question of venereal diseases, the new Surgeon *du Midi* directed himself especially to the attentive observation of facts, and was able to prove at last, in a manner which could not be refuted, the difference which exists in the nature of gonorrhœa and syphilis. The demonstration of syphilitic intra-urethral chancre succeeded in making clear the question which had contributed to obscure the celebrated experiment of Hunter.

It was only later that the same work of selection established the doctrine of the duality of chancres, a work which was completed by his pupil Bassereau in 1852. At this time *l'Ecole du Midi* had reached its zenith. The teaching of Ricord drew many pupils from all parts, and often in fine weather the master would hold his course in the garden of the Hospital, an example followed by Alibert in the St Louis Hospital. In this first period of his life he published several works. Several memoirs on venereal diseases and their treatment, and above all, the *Practical Treatise on Venereal Diseases* (1838), annotations to the translations by G. Richelot, of Hunter's *Treatise on Venereal Disease* (1840), and the *Clinique iconographique* of the Venereal Hospital (1842-1851), a work published in parts, and the plates of which are very fine, still form valuable works of reference. Shortly after this appeared the celebrated *Letters on Syphilis* in the *Union Médicale* 1850-51, and in 1857 the *Lectures on Chancre,* revised and enlarged by his assistant of that time, Alfred Fournier.

Being a member of the Surgical Society, Ricord was nominated a member of the Academy of Medicine on 23rd April 1850, in the section of Surgical Pathology. He became President of this learned Society in 1868.

It was soon after his entrance into the Academy that the celebrated discussion took place on Syphilisation and the Contagion of Secondary Symptoms. The discussion was taken up by Velpeau and continued for several months. Velpeau was supported by Gilbert, who believed that secondary symptoms were contagious. Ricord took the opposite side. On both sides there were fine bursts of oratory, but the doctrines of the celebrated surgeon were shaken during the discussion, and he himself had afterwards to acknowledge the truth of the views of his adversaries.

A few years after, he left the hospital *du Midi* (31st Dec. 1860), and devoted himself exclusively to his large practice.

During the siege of Paris, Ricord was President of the Committee of Ambulances of the Press, and one saw him then at 70 give a fine example of civic and professional courage by going himself on to the battle-field.

In August last he opened, as Honorary President, the International Congress of Dermatology and Syphilograhy, which met at the St Louis Hospital. He presided at the closing banquet, and made a bright speech full of spirit. It was a true pleasure to him to see assembled round him so many eminent doctors from all the countries of the world, of whom many had been his hospital pupils; and the ovations of which he was the object recalled to him the most glorious triumphs of former days.

Blessed with an excellent disposition, a wit whose sallies have become proverbial, a remarkable surgeon, Ricord obtained eminent success as a doctor and as a man of the world.

Until he fell ill, Ricord attended at his consulting room, and saw every day numbers of patients. He attended assiduously the meetings of the Academy of Medicine, and if he walked with difficulty, his mind was always bright. "*Je me porte bien, répondait-il à ceux qui lui demandaient de nouvelles de sa santé, ce sont mes jambes que ne me portent plus.*"

Fond of art, Ricord had collected round him beautiful sculptures and numerous pictures. He had even published a poem in three cantos when he was a simple practitioner of Crouy-sur-Ourcq; and he himself showed us, scarcely two months ago, a few lines he wrote in honour of Mr Edison's arrival in Paris.

Even during his illness he retained his joyous disposition up to the moment when the progress of disease caused him to lose consciousness. Ricord had been a Grand Officer of the Legion of Honour since 1871, and was the recipient of almost all the foreign orders.

His obsequies were celebrated with great pomp on Saturday, 26th October.

The burial took place at Père-Lachaise, where the orations were pronounced by MM. Péan, Horteloup, Le Dendu, Peyron, Riant, Fournier, Mauriac, Diday, etc.—(Henri Feulard, *Annales Dermatologie et de Syphiligraphie,* 25th October 1889.)

CORRESPONDENCE.

(*To the Editor of the Edinburgh Medical Journal.*)

7 Orsett Terrace, W.,
November 5, 1889.

Sir,—Whilst attending a clinique at the London Hospital, given by Mr Frederick Treves, I had the opportunity of observing a case of the disease named "Acromegaly." The following notes I made on the features of the disease, which were admirably demonstrated by Mr Treves:—A woman, aged 50. She had ceased to menstruate early, the cessation being marked by the onset of the disease; this is invariably so. The lower jaw was greatly enlarged; when the mouth was closed the teeth of the lower jaw projected beyond those of the upper by half an inch. This enlargement of the lower jaw gave the lower part of the face a peculiar heavy look. The face was puffy in appearance and the tongue enlarged. The nose was increased in size; in these cases the nasal cartilages are frequently enlarged and sometimes bony; the same occurring in the pinna. The hands were about double their natural size, but not deformed. Hair, skin, and muscular systems normal. The intellect not impaired.

This disease occurs between 35 and 55 years of age. All the changes are symmetrical. Nearly all the recorded cases had some growth in the thyroid gland. The feet are often enlarged as well as the hands, but in this case no enlargement of the feet had taken place. The clavicle, ribs, and patellæ are also sometimes enlarged. The disease is far more common in females. Thinking the case to be of great interest, and never having seen a similar case in Edinburgh, I send the above few notes.—I am, Sir, yours truly,

P. Carr White, M.B.

PUBLICATIONS RECEIVED.

Wm. H. Bennett, F.R.C.S.,—Clinical Lectures on Varicose Veins. Longmans, Green, & Co., Lond., 1889.

A. M. Brown, M.D.,—The Animal Alkaloids. Hirschfeld Brothers, Lond., 1889.

A. A. Bowlby, F.R.C.S.,—Injuries and Diseases of Nerves and their Surgical Treatment. J. & A. Churchill, Lond., 1889.

D. J. Cunningham, M.D.,—A Manual of Practical Anatomy. Maclachlan & Stewart, Edin., 1889.

Prof. Julius Cohnheim,—Lectures on General Pathology. The New Sydenham Society, Lond., 1889.

Sir Dyce Duckworth,—A Treatise on Gout. Charles Griffin & Co., Lond., 1889.

E. H. Fenwick, F.R.C.S.,—Electric Illumination of the Bladder and Urethra as a Means of Diagnosis of Obscure Vesico-Urethral Diseases. J. & A. Churchill, Lond., 1889.

James Finlayson, M.D.,—Account of the Life and Works of Maister Peter Lowe. James Maclehose & Sons, Glasgow, 1889.

M. Foster, M.A., M.D., LL.D., etc.,—A Text-Book of Physiology. Part II. Macmillan & Co., Lond., 1889.

F. J. Gant, F.R.C.S.,—The Student's Surgery. Baillière, Tindall, & Cox, Lond., 1890.

F. W. N. Haultain, M.D., etc., and J. Haig Ferguson, M.B., etc.,—Handbook of Obstetric Nursing. Young J. Pentland, Edin., 1889.

Index-Catalogue of the Library of the Surgeon General's Office, United States Army, Vol. X.

Wm. W. Ireland, M.D.,—Through the Ivory Gate. Bell & Bradfute, Edin., 1889.

John M. Keating, M.D.,—Cyclopædia of the Diseases of Children, Vol. II. Young J. Pentland, Edin., 1889.

Skene Keith, M.B., etc.,—Introduction to the Treatment of Disease by Galvanism. Truslove & Shirley, Lond., 1889.

Thos. Keith, M.D., LL.D., and Skene Keith, F.R.C.S.E.,—Electricity in the Treatment of Uterine Tumours. Oliver & Boyd, Edin., 1889.

H. R. Kenwood, M.B., C.M.,—The Essentials of Medical Anatomy. Young J. Pentland, Edin., 1889.

C. B. Lockwood, F.R.C.S.,—Hunterian Lectures on the Morbid Anatomy, Pathology, and Treatment of Hernia. H. K. Lewis, Lond., 1889.

J. G. M'Kendrick, M.D., LL.D., etc.,—A Text-Book of Physiology. James Maclehose & Sons, Glasgow, 1889.

John M'Lachlan, M.D., etc.,—Applied Anatomy. E. & S Livingstone, Edinburgh, 1889.

C. J. Nixon, M.D., etc.,—Handbook of Hospital Practice and Physical Diagnosis. J. & A. Churchill, Lond., 1889.

Dr C. A. M. Pitta.—Madère. Baillière et Cie, Paris. Lond., 1889.

C. T. Pollock, M.D.,—Leprosy as a Cause of Blindness. J. & A. Churchill, Lond., 1889.

G. V. Poore, M.D., F.R.C.P.,—London (Ancient and Modern) from the Sanitary and Medical Point of View. Cassell & Co., Lond., 1889.

Alex. Porter, M.D., etc.,—The Diseases of the Madras Famine of 1877–78. H. K. Lewis, Lond., 1889.

Sanitary Condition of Melbourne: Report of Royal Commission, Parts I., II.

Hubert Snow, M.D.,—The General Theory of Cancer Formation. J. & A. Churchill, Lond., 1889.

Thirty-first Annual Report of the General Board of Commissioners in Lunacy for Scotland.

Wm. Thorburn, B.S., M.D., etc.,—A Contribution to the Surgery of the Spinal Cord. Charles Griffin & Co., Lond., 1889.

Transactions of the Clinical Society of London, Vol. XXII. Longmans, Green, & Co., Lond., 1889.

Transactions of the Medical and Chirurgical Faculty of the State of Maryland. 1889.

Transactions of the Obstetrical Society of London, Part 3. Vol. XXXI.

D. H. Tuke, M.D., etc.,—The Past and Present Provision for the Insane Poor in Yorkshire. J. & A. Churchill, Lond., 1889.

W. J. Walsham, M.B., etc., and D'Arcy Power, M.B., etc.,—A Handbook of Surgical Pathology. Baillière, Tindall, & Cox, Lond., 1890.

Watts' Dictionary of Chemistry. Vol. II. Longmans, Green, & Co., Lond., 1889.

R. P. Williams, A.M., and B. P. Lascelles, M.A., F.C.S.,—An Introduction to Chemical Science. Ginn & Co., Lond., 1889.

H. C. Wood, M.D., LLD.,—Syphilis of the Nervous System. George S. Davis, Detroit, 1889.

BINDING LIST JAN 1 1945

R
31
E22
v.35
no.1-6

Biological
& Medical
Serials

Edinburgh medical journal

PLEASE DO NOT REMOVE
CARDS OR SLIPS FROM THIS POCKET

UNIVERSITY OF TORONTO LIBRARY

STORAGE